PRIVATE KNOWLEDGE, PUBLIC TENSIONS:

THEORY COMMITMENT IN POSTWAR AMERICAN LINGUISTICS

by

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Abstract

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Propelled by a desire to understand natural language, American linguists of the post-war period brought the tools of the era to bear on the study of syntax: computer science, mathematical graph theory, and even Cold War strategy. Three syntactic theories were enunciated, each trying to untangle the mysteries of our ability to form and use sentences. These theories interacted on a nearly daily basis, influencing and challenging each other through the 1960s. By the end of the decade, one had established clear dominance: Noam Chomsky's theory, developed at MIT. Combining contemporary history of science tools with linguistics-specific concepts, this study explores the dynamics of the syntactic theory-choice debates from 1957 to 1970. I argue that these debates can only be fully understood through a confluence of four themes: explanation, pedagogy, knowledge transmission, and lay linguistics. Together, these themes explain how linguists selected and evaluated theories, how students were trained to think about and use syntax, how ideas and people spread across the United States, and how academic theories played out in peripheral disciplines. They also resolve the central paradox running through this study: how did Noam Chomsky's theory – a theory whose proponents valued the private transmission of un-

derground knowledge and actively prevented outsiders from accessing research – spread across the country and gain a majority of supporters? By paying particular attention to the ideas and problems which mattered to the linguists of the time, this study presents a critical and novel history of postwar American linguistics. In doing so, it rectifies the lack of a balanced, historically-informed account of the discipline. What little literature exists on the history of syntax in America bears the imprint of Whig interpretations: it omits the rival syntactic theories which competed with Chomsky's theory, the technical linguistics debates of the period, and pedagogy and the training of young linguists. Most importantly, it cannot account for the paradox of private knowledge. This study contributes to our historical understanding by both providing the first history of science based investigation of postwar American syntax and showcasing a powerful way of investigating theory development, theory choice, and theory change.

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Abbreviations

ALPAC Automatic Language Processing Advisory Committee

ICG Immediate Constituency Grammar

LACUS Linguistic Association of Canada and the United States

PSG Phrase Structure Grammar

SG Stratificational Grammar

TG Transformational Grammar

1   The Most Unlikely of Buildings

> ... to many it seems that the sun has risen, not in the Orient, but in MIT.
>
> Archibald Hill, in *The Promises and Limitations of the Newest Type of Grammatical Analysis* (1966)[1]

Archibald Hill's sun rose over MIT's Building 20 – a temporary structure erected during World War II to house top-secret radar work – which, in the 1960s, grew into the epicenter of groundbreaking linguistic research. Known as the "plywood palace" and the "magical incubator", Building 20 was where Noam Chomsky's now famous ideas on language and the human brain were nurtured and developed.[2] Dilapidated, "dirty and noisy and hot", full of asbestos, and continually slated for demolition, the building was not the first place one would expect to find state-of-the-art language research conducted by dozens of fine minds.[3] The linguistic theory which emerged from the building, however, was one of terrific influence and impact: a theory which, in under a decade, restructured the study of language in America and commanded the attention of psychologists, philosophers, and cognitive scientists.

Language has long fascinated human beings. It is what allows us to communicate with ease and agility, and what sets us apart from our evolutionary cousins. From Pāṇini's fifth century BC extant grammar of Sanskrit to the Brothers Grimm study of Germanic sound

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[1]Archibald A. Hill (ed.), *The Promises and Limitations of the Newest Type of Grammatical Analysis* (Cincinnati: The University of Cincinnati, 1966), p 18.

[2]Paul Penfield Jr, MIT's Building 20: The Magic Incubator, 1943–1998, MIT Department of Electrical Engineering and Computer Science Commemoration Events, 1997.

[3]Jerrold Zacharias, *Generations: The Story of Building 20 (Video)* (MIT Archives, Committee on the Visual Arts Records, 1945–1980).

shifts in the early 19th century to Franz Boas' turn of the 20th century work on sound perception in Inuit languages, we have a long history of studying language structure. Some of the most exciting American linguistic research of the last century took place in the 1960s, at MIT's Building 20 and other universities across the country. The questions pursued were stimulating and far-reaching: *What structure governs sentence-level language phenomena? How is this structure manifested in the human brain? How can we use mathematics and computer techniques to study natural language syntax?* Propelled by a desire to understand syntactic structure, American linguists brought the tools of the era to bear on their work: computer science, anti-behaviorist philosophy, and even Cold War strategy. Several syntactic theories were enunciated, each trying to untangle the mysteries of our ability to form and use sentences. Through the 1960s, these syntactic theories competed for academic recognition, journal space, students, and funding. By the end of the decade, one had established clear dominance: Noam Chomsky's theory, which had begun in the most unlikely of buildings, but which still today informs linguistic research across the continent. This study investigates the emergence, dynamics, competition, and eventual fates of these syntactic theories.

The three main syntactic theories of the 1960s – constituency grammar, stratificational grammar, and Noam Chomsky's transformational grammar – all aimed to answer fundamental questions about natural language syntax, but they used drastically different technical tools, subscribed to irreconcilable philosophies, and gained diverse socio-professional followings. Proponents of the theories clashed in print and in public, each trying to position their grammar as the way forwards for American linguistics. In the end, transformational grammar won the allegiance of the majority of the American academic linguistics community. Constituency grammar and stratificational grammar were left with small followings, little influence, and few resources. Using history of science techniques and methodology, this study aims to understand the dynamics of the syntactic theory-choice debates of the 1960s. By focusing on concepts of explanation, pedagogical practices, theory transmission, and lay linguistics, I combine contemporary history of science tools with linguistics-specific concepts to explore a subject which has received little attention from historians of science

and ideas. At stake are questions which will affect our understanding not only of American linguistics, but of theory-choice and theory-transmission in the history of science more broadly: how are explanatory criteria established and promoted within disciplinary communities? How can pedagogical and institutional needs influence theory diffusion and student training? How do cultures of private knowledge interact with the academic norms of open publishing and knowledge-sharing? And, finally, how does the relationship between a core discipline and peripheral manifestations affect theory-choice?

I argue that an understanding of the syntactic theory-choice debates of the 1960s requires an understanding of four themes: explanation, pedagogy, underground culture, and lay linguistics. These themes underlie the organization of this study. Chapter 3 investigates concepts of explanation in linguistics: what counts as a syntactic explanation?, and who decides? I show that transformational grammarians successfully redefined explanatory criteria in the 1960s. Forced to respond, proponents of rival theories soon found themselves devoting as much time and effort to fulfilling the transformational criteria as to pursuing their own interests. Chapter 4 explores the impact of pedagogical needs – specifically, the explosion of university linguistics departments and programs in the 1960s – on theory transmission. I argue that transformational grammar captured the open textbook and pedagogical markets, ensuring that a generation of students was trained in the transformational paradigm. Chapter 5 asks how a theory with a pervasive underground culture can rapidly achieve dominance on a nation-wide level. Through the 1960s, transformational grammarians deliberately kept their work out of mainstream journals and maintained a tight network of communicants. I show that a theory can place high value on private knowledge but still influence the mainstream via selective disclosure, oral transmission, and personal contact. Chapter 6 investigates the influence of theory choices in lay linguistics – language teaching, fieldwork, missionary work, and machine translation – on academic theory-choice debates. I show that the strong theoretical commitments typical of the academic scene were blurred and conflated on the lay scene, and that lay linguistics had little influence on the rise of transformational grammar.

Informed by history of science methodology, this work provides a novel and critical account of the rise of transformational grammar. It builds on the recent emphasis on *tools* and *calculations* in the history of science, promoted most prominently by David Kaiser and Andrew Warwick, by bringing the historiographic lessons of this work to play in a discipline where theories were at the heart of discourse.[4] In an academic environment in which tools were fully integrated into broader theories, I use a thematic approach to capture linguistic practices and activities. Further, I add to the recent focus on pedagogy and training by exploring the relationship between pedagogy and theory transmission in a particularly unusual situation: one in which an underground culture flourished and private knowledge reigned. Kaiser's recent work on theoretical physics in postwar America presents an exciting wedding of the pedagogical and the theoretical, and is an historiographic model I both build from and build on.[5] I also emphasize the role of textbooks in knowledge transmission, a topic recently brought to the forefront by, among others, the European Science Foundation's investigation of chemistry in the long 19th-century.[6]

More broadly, this work is both a disciplinary history and an exploration of theory-choice in a relatively unstudied scientific community. As such, it follows in a long line of history of science tradition. In aiming to explain *why* transformational grammar prevailed over alternative syntactic theories, and *how* transformational theory spread across America, I emphasize interrelationships between the theoretical, pedagogical, academic, and practical manifestations of scientific theories. In this respect, the two books which have most influenced my historiographic approach are Suzanne Zeller's study of inventory sciences in Victorian Canada and Jed Buchwald and Andrew Warwick's anthology on the birth of microphysics.[7] The first exemplifies a seamless and carefully constructed integration of

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[4]This work includes David Kaiser, *Drawing Theories Apart: The dispersion of Feynman diagrams in postwar physics* (Chicago: The University of Chicago Press, 2005), Andrew Warwick, *Masters of Theory: Cambridge and the rise of mathematical physics* (Chicago: University of Chicago Press, 2003), Andrew Pickering, "Against Putting the Phenomena First: The discovery of the weak neutral current," *Studies in History and Philosophy of Science* 15/2 (1984), and Andrew Pickering, *Constructing Quarks: A sociological history of particle physics* (Chicago: University of Chicago Press, 1984).

[5]Kaiser, *op. cit.*

[6]Anders Lundgren and Bernadette Bensaude-Vincent (eds.), *Communicating Chemistry: Textbooks and their audiences* (Canton, Massachusetts: Watson Publishing International, 2000).

[7]Suzanne Zeller, *Inventing Canada: Early Victorian science and the idea of a transcontinental nation* (Toronto:

technical, practical, social, and pedagogical concerns in the development of a young science, while the second provides a multifaceted and challenging view of the rise of a particle and a discipline. Finally, this study contributes to the growing body of work on the history of science in Cold War America by investigating a discipline in which Cold War funding and thinking impacted on research topics, institutional growth, and professional mentalities.[8]

1.1 Historiography

The arguments I put forward challenge the existing literature on the rise of transformational grammar. This literature focuses on socio-professional factors (including philosophical beliefs, institutional leadership and control, and funding), inherent technical appeal, and rhetoric, but largely ignores rivalries between syntactic theories, technical debates, pedagogy and training, and underground culture. These omissions, I argue, lead to an incomplete and imprecise analysis of 1960s American academic linguistics – an analysis which fails to fully account for the realities of the discipline.

The history of linguistics in America is a small discipline – one which is rarely featured in university courses or programs, and one for which relatively little literature exists. The vast majority of works on the rise of transformational grammar have been written by linguists who were themselves intimately involved in the theory-choice debates of the 1960s – among others, Frederick Newmeyer, Robert Hall, H.A. Gleason, and P.H. Matthews. Their historical writings reflect their experiences and commitments: Newmeyer, an ardent transformationalist, writes with strong pro-transformational grammar sensibilities; Hall's writings display his vehement anti-transformational commitments; and Gleason's work is shaped

University of Toronto Press, 1987), Jed Z. Buchwald and Andrew Warwick (eds.), *Histories of the Electron: The birth of microphysics* (Cambridge, MA: MIT Press, 2001).

[8]Within this body of work, Joel Isaac's study of the human sciences, Roger Geiger's seminal history of American research universities, Mark Solovey's exploration of funding and the social sciences, and Philip Loring's (unpublished) work on behaviorism, linguistics, and instruments in Cambridge, Mass., are particularly interesting. Joel Isaac, "The Human Sciences in Cold War America," *Historical Journal* 50 (2007), Roger L. Geiger, *Research and Relevant Knowledge: American research universities since World War II* (Oxford: Oxford University Press, 1993), and Mark Solovey, *Follow the Money: The rise, decline and fragmentation of social science in Cold War America* (Forthcoming).

by his adherence to stratificational grammar. Only a handful of outsiders have written in depth on 1960s American linguistics. They include, most prominently, rhetoricist Randy Allen Harris and sociologist Stephen O. Murray. These accounts, too, are influenced by biases: Murray adopts a confrontational attitude towards transformational grammar, while Harris overstates and exaggerates the mood and radicalism of the era. Lacking is a balanced account of the era, informed by history of science methodology. This study fills this gap.

The most prolific – and most divisive – commentator on the rise of Chomskyan linguistics is Frederick Newmeyer, a linguist at the University of Washington. Having completed his Ph.D. at the University of Illinois in 1969 under transformationalist Robert Lees, Newmeyer subsequently worked on the development of one branch of transformational grammar – generative semantics. He has served the American linguistics profession throughout his career, as editor of *Natural Language and Linguistic Theory* (1988–2003) and as associate editor of *Language* (1981–1985) and of *Language and Communication* (1990–2008). At the Linguistic Society of America, he has acted as secretary-treasurer for five years and as president in 2002. He is well-known as a vocal proponent of transformational grammar, and his views come through strongly in his historical works including, most prominently, *Linguistic Theory in America: The first quarter century of transformational generative grammar* (Academic Press, 1980) and *The Politics of Linguistics* (University of Chicago Press, 1986). These works paint a heroic picture of Chomsky, his colleagues and their theory, while all but ignoring rival syntactic theories.

Newmeyer builds his case for the rise of transformational grammar from a combination of socio-professional and technical factors. On the socio-professional side, he argues that the mentalist basis of Chomsky's theory offered a desirable alternative to older linguistics programs, which were rooted in behaviorism. "The crisis in linguistics which led to the downfall of structuralism and the victory of transformational generative grammar had its roots in philosophy", he writes: "[l]ittle by little, the philosophic and scientific underpinnings were knocked out from under structuralism. [...] As its philosophical props gave

way, structural linguistics found itself in a distinctly unstable posture. Not surprisingly, it was relatively simple for a new theory [...] to topple it completely".[9] Much of the immediate appeal of Chomsky's theory, Newmeyer asserts, came from offering an approach to linguistics which explicitly rejected older methods.

Newmeyer also highlights the intellectual quality of the first generation of transformationalists. Chomsky's strong personality and academic leadership, he argues, enabled him to attract "some of the brightest young scholars in the United States" to his MIT-based linguistics group.[10] "The quality of the first two classes to enter MIT (in 1961 and 1962) was instrumental to the early success which the theory achieved", Newmeyer writes, and "[n]ot one individual who has failed to contribute to linguistic theory is found in its list".[11] Young linguists found transformational grammar appealing, he continues, in part because of its rejection of what had come before: "the 1960s were a decade of rebellion, and the intellectual and political ferment going on in American universities at that time provided an ideal atmosphere for the intellectual movement sweeping linguistics, which was bent on overthrowing the rigid dogmas of American structuralism. Just as students began en masse to question the 'common sense' political assumptions of their upbringing which they felt were rationalizing an imperialist foreign policy and oppressive domestic policy by the American government, they began to question the 'common sense' pseudoscientific assumptions [of behaviorism in structural linguistics]".[12]

Newmeyer attributes the transmission of transformational theory across America to the dispersion of graduate students, drawing on a point memorably made by philosopher John Searle, who wrote in the New York Review of Books in 1972 that "Chomsky did not convince the established leaders of the field, but he did something more important, he convinced their graduate students".[13] These graduate students, Newmeyer argues, were "in a

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[9]Frederick J. Newmeyer, *Linguistic Theory in America: The first quarter-century of transformational generative grammar* (New York: Academic Press, 1980), p 13–15.

[10]*Idem*, p 49.

[11]*Idem*, p 50.

[12]*Idem*, p 49.

[13]New York Review of Books, 29 June 1972, p 19, quoted in Frederick J. Newmeyer, *The Politics of Linguistics* (Chicago: The University of Chicago Press, 1986), p 83.

unique position to extend Chomsky's influence" because when they received their doctor-ates in the early-to-mid 1960s, "[j]obs were for the taking in the new departments then being organized at state universities in Illinois, California, Texas, Ohio, Massachusetts, Washing-ton, and elsewhere".[14] The first generation of transformationalists dispersed across Amer-ica to fill new positions, and brought their theory with them. This speaks to the vast expan-sion of academic linguistics in America in the 1960s, a subject discussed here in chapters 3 and 5.

Finally, Newmeyer underlines the importance of the MIT linguistics group's access to vast amounts of military and civilian government funding. With this funding, Chomsky and his colleagues enjoyed "the kind of support for a linguistics program that no other university could hope to match", and were able to provide first-class support to students through the 1960s.[15] Transformational grammar "crucially depende[d] on funding from various branches of the Department of Defense", Newmeyer notes – and, as the only university linguistics program to benefit from a defense grant in the early 1960s, transformational grammar had a significant head start over rival theories.[16]

On the theoretical side, Newmeyer argues that the technical attributes of transforma-tional grammar were key to its success: "a significant number of linguists, particularly young ones, found the premises of Chomsky's theory convincing and its results impres-sive", he wrote – "[a]fter all, Chomsky had succeeded in solving problems of grammati-cal analysis that empiricist structuralist approaches had wrestled with unsuccessfully for years".[17] Because of the technical prowess of transformational theory, Newmeyer pro-claimed in 1986, "more has been learned about the nature of language in the past 25 years than in the previous 2500" – a strong claim which underlines Newmeyer's penchant for belittling all linguistic work apart from transformational grammar.[18]

While Newmeyer identifies several factors as central to the rise of transformational gram-

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[14]Newmeyer, *The Politics of Linguistics*, p 83.
[15]Newmeyer, *Linguistic Theory in America*, p 52.
[16]Frederick J. Newmeyer and Joseph Emonds, "The Linguist in American Society," in *Chicago Linguistic Society 7th Regional Meeting* (Chicago: Chicago Linguistic Society, 1971), p 288.
[17]Newmeyer, *The Politics of Linguistics*, p 80.
[18]*Idem*, p 250.

mar, his work suffers from two key flaws. First, he focuses entirely on transformational theory, rarely mentioning and never investigating rival syntactic theories. As such, his account is a classic example of Whig history: the singular story of a victorious theory and its practitioners, unimpeded and uninfluenced by outside forces. This is especially evident in his treatment of graduate student dispersion. Newmeyer traces the movement of students from MIT to other institutions, and correlates this movement with the spread of transformational theory – but he does not investigate the perpetuation of this theory within new institutions themselves. That is, he does not explore the role of pedagogy, classroom teaching, and textbook publishing on the training of young linguists. These are all areas in which rival syntactic theories could, and did, interact with transformational grammar. Further, while Newmeyer highlights the success of transformational theory's technical apparatus, he does not consider it in the context of the technical tools provided by rival theories. Through the 1960s, the transformational technical apparatus influenced and was influenced by other syntactic theories – a dynamic set of relationships vital to the theory-choice debates of that decade. This study rectifies these flaws by investigating in detail both the role of pedagogy in 1960s American linguistics and the evolution of technical explanatory criteria from the point of view of rival syntactic theories.

The second flaw in Newmeyer's work is his flagrant bias towards, and promotion of, transformational grammar. He treats transformational theory as the be-all and end-all of 20th-century American linguistics; as the only theory capable of answering fundamental questions about language. This attitude has garnered harsh reaction from both inside and outside the linguistic community. Newmeyer's bias "detract[s] critically" from his historical work and "belie[s] its author's claim to be presenting a generally dispassionate history", wrote linguist and pragmaticist Robin Lakoff in her 1989 memoir.[19] "Newmeyer's bias is the most dangerous kind", she continues: "inexplicit, perhaps not fully recognized even by the author. No one who lived through the period as a participant can claim the status of disinterested observer. By his appearance of doing so, Newmeyer misleads the

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[19] Robin Lakoff, "The Way We Were; or; The real actual truth about generative semantics: A memoir," *Journal of Pragmatics* 13 (1989), p 940.

reader and distorts the facts".[20] Sociologist Stephen O. Murray is vehement in his criticism of Newmeyer's bias, asserting that his work "may be regarded as history at MIT, though nowhere else".[21] Murray labels Newmeyer's *Linguistic Theory in America* as a "piece of hagiography" which places Chomsky – "Saint Noam" – on a pedestal.[22] H.A. Gleason, a stratificational grammarian who worked at the Hartford Seminary Foundation and the University of Toronto, accuses Newmeyer and other commentators of perpetuating an "origin myth" which misleadingly depicts pre-Chomskyan linguistics as "hopelessly unscientific" and paints transformational grammar as a "successful Revolution".[23]

A much less controversial treatment of transformational grammar is provided by Cambridge linguist P.H. Matthews' 1993 study *Grammatical Theory in the United States from Bloomfield to Chomsky*.[24] This is an intensively technical study of American linguistic theory from 1914 (the year of publication of Leonard Bloomfield's first book) to manifestations of Chomskyan linguistics in the 1980s. Proceeding chronologically through a jungle of articles, Matthews traces theoretical progression and intellectual development in the Bloomfieldian and Chomskyan schools, focusing on the separation of form and meaning, Chomsky's philosophy of linguistics, and the identification of intellectual precursors.

Written with technical expertise and based on a detailed study of published research, Matthews' book provides important insight into two schools of American linguistics. In particular, he carefully and successfully rectifies the Whig interpretation of Chomskyan linguistics, which paints transformational grammar as an entirely new and novel approach to language study – in short, as a revolution.[25] By tracing intellectual links between Chom-

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[20] *Idem.*

[21] Stephen O. Murray, "Review of Frederick J. Newmeyer's Linguistic Theory in America: The first quarter century of transformational generative grammar (1980)," *Historiographia Linguistica* 8/1 (1981), p 107.

[22] *Idem.*

[23] H.A. Gleason Jr, *Theories in Conflict: North American linguistics in the fifties and sixties (a memoir)*, p 75, capitalization in original.

[24] P.H. Matthews, *Grammatical Theory in the United States from Bloomfield to Chomsky* (Cambridge: Cambridge University Press, 1993).

[25] The Whig presentation of the rise of Chomskyan linguistics, associated most prominently with Frederick Newmeyer and introductory syntax textbooks, is the result of "the winner holding the pen": nearly ubiquitous through the 1970s and 1980s and still common today, this presentation was promoted by members of the Chomskyan school who had turned to historical writing. In this sense, the Whig treatment of the history of linguistics is following the same pattern as the Whig treatment of other areas in the history of science: an initial period of Whiggisms produced by the "winners" is slowly being supplanted by works which take into

sky and his teacher, University of Pennsylvania linguist Zellig Harris, Matthews uncovers the roots of transformational theory.[26] The book is, however, a technical description of linguistic work, and Matthews does not attempt to explain or analyze the *rise* of transformational grammar. Rival syntactic theories simply do not play a role in his story – and nor do transformationalists other than Chomsky. As such, Matthews' study gives important insight into Bloomfield's and Chomsky's technical work, but does not contribute to our understanding of the syntactic theory-choice debates of the 1960s.

Two other works deserve mentioning, although they, too, are technically-oriented and do not address the rise of transformational theory *per se*. In 2001, the University of Verona's Giorgio Graffi (Department of Linguistics, Literature, and Communication Sciences) released 200 *Years of Syntax: A critical survey*, a sweeping manuscript covering syntactic study in the 19th and 20th centuries.[27] Ranging from the psychologistic syntax of the 1800s to the advent of Chomsky's program and beyond, Graffi provides a detailed and primary source-based picture of syntax as a systematic and unified endeavor. He argues that syntax is not, as commonly believed, an invention of the 1950s, but rather that it has a long and rich history. With its inclusion of both European and American schools of thought, its highly technical focus, and long time-span, Graffi's book is an important source in the history of linguistics. More recently, Marcus Tomalin's *Linguistics and the Formal Sciences: The origins of generative grammar* was published by Cambridge University Press in 2006.[28] With a joint appointment in the English Department and the Engineering Department's Speech Research Group at Downing College, Tomalin maintains interests in English literature, speech recognition, and the history and philosophy of language and linguistics. *Linguistics and the Formal Sciences* traces the influence and impact of mathematics, philosophy, and logic on Leonard Bloomfield's linguistic thought and on early transformational grammar. Tomalin

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consideration a broader, and more balanced, perspective.

[26]The 'revolutionary' nature of Chomsky's program is also challenged in Konrad Koerner, "The 'Chomskyan Revolution' and its Historiography," in *Toward a History of American Linguistics* (London: Routledge, 2002).

[27]Giorgio Graffi, 200 *Years of Syntax: A critical survey* (Amsterdam: John Benjamins Publishing Company, 2001).

[28]Marcus Tomalin, *Linguistics and the Formal Sciences: The origins of generative grammar* (Cambridge: Cambridge University Press, 2006).

argues that, far from being divorced from linguistics, the formalism and logicism which pervaded mathematical philosophy at the turn of the 20th century were "unambiguously" associated with the development of transformational theory.[29] Even though the strength of this association has been challenged, Tomalin's study is highly valuable for its discussion of interrelationships between logic, mathematics, and linguistics.[30] While important in their own rights, however, these works provide little insight into syntactic theory debate and competition in the postwar era. As is representative of the existing literature, they cannot account for the success of Chomsky's linguistics program.

* * *

One of the few non-linguists to have written extensively on the rise of transformational grammar is Stephen O. Murray (b. 1950), a sociologist trained at the University of Toronto and the University of California at Berkeley, who now works as an independent scholar in San Francisco. Murray's 1979 University of Toronto doctoral dissertation, *Theory Groups and the Study of Language in North America: A social history* (published in 1993 by John Benjamins), is a sociological study of the formation of scientific groups in the American linguistics community during the 19th and 20th centuries. Murray argues for the Mullins-Griffith sociological theory, which states that three factors are necessary for the formation of a scientific group: good ideas (that is, ideas which the scientists in question believe will "lead to solutions to existing puzzles or to extend methods and theories to new research questions"), intellectual leadership (a person or persons who can build the aforementioned good ideas into a comprehensive research program and lead intellectual progress in the area), and organizational leadership (a person or persons who can secure funding and facilities, communicate research findings, and organize the research laboratory or group).[31] "Without all three factors", Murray argues, "no scientific group will emerge. All are necessary, no one is sufficient".[32] Applying this theory to 1960s American linguistics, he concludes that

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[29]*Idem*, p 186.

[30]For a challenging essay review of Tomalin's work, see Barbara Scholz and Geoffrey Pullum, "Tracking the Origins of Transformational Generative Grammar," *Journal of Linguistics* 43 (2007).

[31]Stephen O. Murray, *Theory Groups and the Study of Language in North America: A social history* (Amsterdam: John Benjamins Publishing Company, 1993), p 22.

[32]*Idem*, p 22–23.

transformational grammar achieved dominance because it was able to establish these three factors: transformational theory itself acted as the good idea; intellectual leadership was provided by Chomsky; and organizational leadership was provided by fellow MIT linguist Morris Halle. Murray also adds a fourth factor which he argues was essential for transformational grammar: funding. "While government support did not dictate the contents nor the structure of the linguistic theory developed at MIT with that support", he writes, "the monopoly of funding for linguistics at one institution considerably affected the fate of those ideas, most particularly the recruitment of students who, all other things being equal, preferred to obtain financial support".[33]

Murray's study is valuable and, indeed, unique as an informed sociological analysis of the rise of transformational grammar. However, from the point of view of the history of science, it has three faults. First, akin to Newmeyer, Murray's analysis of 1960s American linguistics takes into account only transformational grammar: he does not discuss or investigate rival syntactic theories of that decade. This omission limits the validity of his conclusions since he implicitly – and incorrectly – treats transformational theory as if it was developed in intellectual isolation. Second, Murray's sociological approach means that he entertains no technical discussion or analysis of linguistic theory. In an era when technical capacity and power were at the heart of theory-choice debates, this approach leaves many questions about the success of transformational grammar unanswered. In his 1985 review of Murray's work, linguist and psychologist Michael Dillinger remarked that Murray's sociological background makes him "approach the history of linguistics more as a question of group formation around intellectual and organizational leaders than one of an interplay of ideas".[34] Finally, Murray's language and tone make clear his bias against Chomsky. While his argumentation is itself relatively balanced, his descriptive and narrative language display a negative slant which detracts from the strength of his arguments. He describes the success of transformational theory as a "palace coup"; he refers to Chomsky's second major

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[33]Murray, *Review of Newmeyer*, p 109.
[34]Michael L. Dillinger, "Review of Stephen O. Murray's Group Formation in Social Science," *Historiographia Linguistica* 12/1 (1985), p 209.

work as "the constitution of the revolutionary regime in power"; he compares Chomsky to Stalin and Mao; and he labels behind-the-scenes transformationalists as "informants".[35] These analogies blemish what is otherwise an important sociological study.

A second outsider, rhetoricist Randy Allen Harris, is best-known for his work on the Semantics Wars – the clashes over the role of semantics in linguistic inquiry which consumed American linguistics in the late 1960s and early 1970s. His 1990 doctoral dissertation at Rensselaer Polytechnic Institute, *The Linguistics Wars* (published in 1993 by Oxford University Press), is an examination of the Semantics Wars from the point of view of rhetoric and group confrontation. Now in the Department of English at the University of Waterloo, Harris studies rhetoric, linguistics, and professional communication. While his early work focused on the Semantics Wars, he has also commented on the state of American linguistics in the 1960s. He argues that the rhetoric used by transformational grammarians through that decade was instrumental to the success of their theory. The first generation of MIT transformationalists enjoyed early success – and, he writes, "[s]uccess, we all know, is heady, and the group's most definitive character trait was cockiness: they were young, they were bright, and they were working on a novel and immensely promising theory in collaboration with one of the finest intellects of the century".[36] Tightknit, intelligent, and ambitious, he continues, this group developed a linguistic discourse and argumentation style which encouraged a blend of innovation, wit, and polemics. This rhetoric was successful at recruiting and retaining young linguists, at fostering a sense of group identity among transformationalists, and at minimizing criticism from proponents of rival theories.

Written with a lucid narrative style and colorful language, Harris' work is fast-paced and exciting to read. He paints American linguistics in the 1960s and 1970s as full of intense personalities, interpersonal conflicts, and rumbling arguments. This style, however, detracts from Harris' authority as an historical commentator: his tendency to exaggerate conflicts makes it difficult for the reader to determine where historical analysis ends and dramatization begins. Robert Barsky remarks that Harris employs a "soap opera style of fashioning a

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[35] Murray, *Theory Groups*, pp 153, 239, 431, 445.
[36] Randy Allen Harris, *The Linguistics Wars* (New York: Oxford University Press, 1993), p 68 .

narrative: intrigues are developed, villains are created, and plots thicken".[37] By emphasizing "power struggles among key players", Barsky continues, Harris' work "lends an air of intrigue to the field but [...] contributes little to our understanding of it".[38] Further, while recognizing the relationship between rhetoric and the transformationalists' culture of private knowledge, Harris does not investigate in detail the transmission of transformational theory in an underground culture.

<div align="center">* * *</div>

This historiographical outline makes it clear that there is no single received view of the rise of transformational grammar. Rather, the existing literature identifies three arcs: socio-professional factors (funding, ability to secure positions in university linguistics departments, strong leadership, and philosophical commitments), technical appeal (the inherent value of transformational theory), and argumentation style (rhetoric and oratory skill). These arcs go some way towards characterizing the success of Chomsky's linguistic theory. However, this characterization – and the literature it stems from – has four key flaws: it does not explore the rival syntactic theories which competed with transformational grammar in the 1960s; it largely ignores the technical linguistic debates of that decade; it fails to discuss linguistic pedagogy and the training of young linguists; and it does not account for the prevalence and perpetuation of private knowledge. This study aims to rectify these flaws. By paying particular attention to the ideas and problems which mattered to linguists of the era – establishing explanatory criteria, coping with unprecedented numbers of linguistic students, and navigating between public and private knowledge – I present a critical and novel account of American linguistics in the 1960s.

1.2 Boundaries

This study examines theoretical syntax in the United States of America between 1957 and 1970. The chronological, geographical, and disciplinary boundaries of this work have been

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[37] Robert F. Barsky, *Noam Chomsky: A life of dissent* (Cambridge MA: The MIT Press, 1997), p 57.
[38] *Idem.*

selected to achieve maximal coverage of the syntactic theory-choice debates. The chronological boundaries are guided by key events in American linguistics itself: I begin with the publication of Noam Chomsky's first major work, *Syntactic Structures*, in 1957, and end just over a decade later, in 1970, when Chomsky's linguistic theory had achieved dominance on the academic linguistics scene. *Syntactic Structures* provided American linguists with their first taste of transformational theory, and is widely considered to mark a seminal turning point in the field. Through this time period, three rival syntactic theories – transformational, stratificational, and constituency grammars – competed for the attention of American linguists. By the mid to late 1960s, the theory-choice debates had come to a clear conclusion: transformational grammar was thriving, and its two rival theories were declining. At the very end of the decade, the crux of debate in American academic linguistics shifted from syntactic theory-choice to the Semantics Wars – an internal clash between two groups of transformational grammarians. This study ends when the Semantics Wars begin. While the Semantics Wars have been studied by, among others, Randy Allen Harris, John Goldsmith, and Geoffrey Huck, the syntactic theory-debates have not received much attention.[39] The break between the two debates, as is so common in history, was not clean and instantaneous; rather, there was a period of overlap. While vestiges of the Semantics Wars can be seen as early as 1968, the syntactic debates were active until the end of the decade and – albeit at a slower pace – for a few years afterwards. My aim is to understand the dynamics of American linguistics during the syntactic theory-choice debates, and the endpoint of 1970 provides a guiding but not firm chronological boundary. Ideas do not conform to calendars, and where there is discrepancy I will follow the ideas. I will briefly comment on post-1970 linguistics – and, specifically, links between the 1960s syntax debates and the Semantics Wars – in Chapter 5.

   This study is restricted to the United States. Between 1957 and 1970, American syn-

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[39]Randy Allen Harris, "Generative Semantics: Secret handshakes, anarchy notes, and the implosion of ethos," *Rhetoric Review* 12/1 (1993), Harris, *Linguistics Wars*, Geoffrey J. Huck and John A. Goldsmith, *Ideology and Linguistic Theory: Noam Chomsky and the deep structure debates* (New York: Routledge, 1995), Geoffrey J. Huck and John A. Goldsmith, "On Comparing Linguistic Theories: Further notes on the generative semantics/interpretive semantics debate in American linguistics," *Historiographia Linguistica* 25/3 (1998).

tax was little influenced from outside. While contemporaneous European scholars – from the Dane Louis Hjelmslev to English-Australian Michael Halliday – were making waves in Great Britain and on the continent, their work did not penetrate far into America. European work was of interest to scholars who had fled Europe for America during the Second World War but, concentrated in the Northeast and with their own professional society (the Linguistic Circle of New York), these scholars were not heavily involved in American syntactic work. The isolation and self-containment of American linguistics in the 20th century is widely recognized by commentators. "[N]ew or contrary ideas" mattered to American linguists, P.H. Matthews wrote in 1993, only "when they arose in response to a problem recognized in America at the time, and come from members of the American community".[40]

The root cause of this isolation is the very different set of problems faced by linguists in America and in Europe. In the early 20th century, linguistics research in America was largely embedded in anthropology and directed towards Amerindian languages. "The native Indian population in America presented theoretical research problems and practical administrative problems which required expert linguistic knowledge for their solution", writes Bertil Malmberg: "[p]artly it was necessary for sheer practical reasons (e.g. missionary work) to be able to speak their native languages. Partly it was because the analysis of the Indian languages soon came to be a necessary complement to an integral part of the study of native culture, social structure, religion, myths and traditions".[41] As a result, the leading American linguists in the first half of the century were specialists in Amerindian languages and culture. While they took the description and analysis of these previously unwritten languages as their main task, on the other side of the Atlantic, European linguists concentrated on semiotics and historical linguistics. This bifurcation of tasks led to a bifurcation of interests, and to the self-containment of American linguistics. As such, the syntactic work in America of the 1950s and 1960s was homegrown, and little influenced by European linguistic work or traditions. My focus on the United States thus provides an inclusive and full account of 1960s American syntax. I will comment on linguistics work in

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[40] Matthews, *Grammatical Theory*, p 50.
[41] Bertil Malmberg, *New Trends in Linguistics: An orientation* (Sweden: University of Lund, 1964), pp 159–160.

other countries when it enriches my account.

The geographical and chronological boundaries of my work mean that I exclude the sweeping influence Chomsky's linguistics work has had in Canada and in Europe from the late 1960s on. While there is no denying the impact of transformational grammar outside of the United States, my choice not to comment on this work reflects a historiographic decision: a key flaw in the existing literature on the rise of transformational grammar is that it all but ignores rival syntactic theories. My study calls attention to rival theories, and argues that these theories played important roles on the 1960s American linguistics scene. Far from being peripheral, stratificational and constituency grammar actively engaged in technical debates, recruited students and attracted funding, influenced the linguistics activities of language teachers, fieldworkers, and missionaries, played a significant role in machine translation efforts, and challenged the thinking and activities of American linguists. By deliberately not commenting on the wider influences of Chomsky's linguistics work, I aim to put the spotlight squarely on rival syntactic theories, which hitherto have been slighted in the historical literature.[42]

Finally, this study examines syntax: the study of sentence-level phenomena within the broader discipline of linguistics. Linguistics encompasses a wide variety of subdisciplines, crudely classifiable into *structural* and *nonstructural* varieties. Non-structural linguistics includes sociolinguistics (the study of social and cultural factors in language use), historical linguistics (the study of language genealogy and proto-languages), and linguistic anthropology, among others. Structural linguistics – also called theoretical linguistics – comprises the study of human language as a structural system. It is often divided into four areas: phonology (the study of systematic relationships between sound and meaning, usually up to the level of the syllable), morphology (the study of word-, or morpheme-, level language phenomena, including word formation patterns), syntax (the study of the sentence-level language phenomena), and semantics (the study of systematic relationships between lan-

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[42] A wide literature is available to readers interested in the influences of Chomsky's linguistics work, including John Lyons, *Noam Chomsky* (New York: The Viking Press, 1970), Barsky, *op. cit.* and James McGilvray (ed.), *The Cambridge Companion to Chomsky* (Cambridge: Cambridge University Press, 2005).

guage and meaning, including how humans attach meaning to syntactic structure). Recently, a third broad linguistic subdiscipline has emerged: psycho-, or neuro-, linguistics. This subdiscipline focuses on the neurological and psychological mechanisms in the human brain which enable us to acquire, comprehend, and use language.

Structural linguistics came into its own in America in the 1930s with the work of Leonard Bloomfield, and grew considerably in the following decades. By the 1960s, structural linguistics – and, specifically, syntax – was a key player in American academic linguistics. American structural linguists focused on phonology in the 1930s, morphology in the 1940s, and syntax in the 1950s and 1960s. The study of syntax between 1957 and 1970 was motivated by the Cold War desire for machine translation, the formalization of linguistics, and the feeling that American linguists had satisfactorily dealt with phonology and morphology in the previous decades. With Chomsky's declaration of syntax as the king of linguistic subdisciplines in his 1957 *Syntactic Structures* came a great flurry of work on syntax. In the period of interest to this study, syntax was the defining pursuit of American linguistics: it attracted funding from military and civilian government bodies, it formed the basis for new university linguistics programs and departments, and it influenced work in non-structural linguistics, psychology, philosophy, and computer science. By focusing on syntax, this study focuses on the central idea of 1960s American linguistics.

Finally, linguistics is a broad and comprehensive discipline, and I cannot possibly do justice to all of the subdisciplines studied between 1957 and 1970. My work excludes, for the most part, William Labov's groundbreaking sociolinguistics work on dialect change and African American Vernacular English; Zellig Harris' string linear language analysis; and Kenneth Pike's tagmemics approach to language, among others. These areas have begun to have been covered by others.[43] I do, however, explore the interactions between syntax and closely-related areas of linguistic study and application, including machine translation, fieldwork and missionary linguistics, and language teaching. The intimate connections be-

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[43]The reader is directed to Bruce Nevin and Stephen B. Johnson (eds.), *The Legacy of Zellig Harris* (John Benjamins, 2002), Thomas N. Headline, "Kenneth Lee Pike (1912–2000)," *American Anthropologist* 103/2 (2001) and Konrad Koerner (ed.), "William Labov and the Origin of Sociolinguistics," in Konrad Koerner (ed.), *Toward a History of American Linguistics* (London: Routledge, 2002).

tween these areas and syntactic research in the 1960s means that they can greatly assist our understanding of that period in American linguistics. I also comment on other linguistics subdisciplines when immediately relevant to my study of syntax.

1.3 Organization of Study

While this study investigates three syntactic theories – constituency, stratificational, and transformational grammar – its organization is not dictated by these theories as individual threads, but is rather centered on four broad themes: explanatory criteria, pedagogy, knowledge transmission, and lay linguistics. The three main syntactic theories of the 1960s did not, I emphasize, exist in isolation; rather, they interacted on a nearly daily basis, influencing and challenging each other through the decade. These four themes are chosen to maximize our understanding of the syntactic theory-choice debates as dynamic and interactive events in American linguistics. Rather than considering each theory in turn, I consider their interrelationships in a variety of contexts. This narrative organization highlights the fluidity of theories and of practitioners, the interplay between research, training, and idea propagation, and the complexities of social control over knowledge dispersion.

This study opens with a background discussion of American linguistics in the early- to mid-20th century and a detailed exposition of the three syntactic theories which occupy the remainder of the work (chapter 2). This is intended to familiarize readers who have little background in linguistics with both the socio-professional and intellectual development of American linguistics prior to 1957, and with the technical apparatus of constituency, stratificational, and transformational theories. I aim to provide enough technical detail to do justice to the theories, but not so much as to overwhelm readers who are new to syntactic research. Further technical detail is provided through the rest of the study as needed. This background chapter is also intended to acquaint linguistically-sophisticated readers with the historical backdrop to the study, and to introduce the motivation for my four themes of interest.

Chapter 3 – Conditions of Explanation – explores the role of explanatory criteria in the

syntactic theory-choice debates of the 1960s. At stake are two main questions: what counts as explanation in linguistics? and, how is this decided? As well as proposing technical tools for syntactic analysis, the three linguistic theories of interest to this study also enunciated explanatory criteria for syntax. These are the conditions which, according to a given school, any syntactic theory must meet in order to be capable of explaining sentence structure. In the 1960s, these criteria ranged from accounting for specific syntactic phenomena to explaining child language acquisition to conforming to neurophysiological data. They are not the after-products of philosophers, historians, or outside commentators, but the time-of-inquiry results of discipline-internal and theory-internal priority setting. It is on these internal criteria that the theory-choice debates hinged: to convince a linguistics student to adopt a particular syntactic theory, or to convince an established linguist to change theory commitments, that linguist had to be persuaded first of the value of the new theory's explanatory criteria, and second of the theory's ability to fulfill those criteria. The decision to commit to a syntactic theory comes largely from belief in that theory's potential ability to explain syntactic structure, language, and related elements of the world at large. As such, the theory best able to dominate in the explanatory arena was best poised to gain and retain supporters.

I argue that transformational grammarians were successful at naming the dominant explanatory criteria for American syntax in the 1960s and beyond. Beginning with the publication of *Syntactic Structures* in 1957, transformational grammar raised compelling questions about language and set the stakes for syntactic explanation high. By bringing problematic data constructions, formalization, language creativity, and psychological validity to the forefront, Chomsky and his colleagues fundamentally altered the conception of explanation in linguistics. As transformationalists delivered on their goals, they created a momentum of optimism and progress – a momentum which was essential to attracting students and retaining supporters. They also placed their explanatory criteria centerstage at conferences and in writing, where they forcefully and repeatedly criticized rival theories for not meeting these criteria. Rivals were forced to respond. Soon, stratificational

and constituency grammarians were devoting as much time and effort, if not more, to fitting their theories to the transformational explanatory criteria as they were to advancing their own explanatory priorities. Stratificationalists made inroads in the late 1960s, especially in areas of simplicity metrics and syntactic power, but while their arguments forced transformational grammarians to reconsider their assumptions, it was too late to block the tide. By successfully setting the conditions for explanation in 1960s syntax, transformationalists provided their own supporters with highly significant questions to pursue and, at the same time, drained energy and momentum away from rival theories. This monopoly over explanatory criteria was central to the dominant position transformational grammar established in the American academic linguistics community.

Chapter 4 – Syntax in the Classroom – shifts focus to pedagogy and training. Following the Second World War, American academic linguistics enjoyed huge growth: universities across the country established linguistics departments, students flocked to do graduate work in syntax, and undergraduate class sizes swelled. This rapid growth brought with it a host of pedagogical problems: large numbers of students wanted to be taught at the undergraduate and graduate levels, but there were no standard courses in syntax, few syntax-oriented textbooks, and little available source material. Pedagogical stresses weighed heavily on the discipline through the 1960s. While university and student buy-in represented a bright future for academic linguistics, it also presented immediate and pressing challenges: textbooks needed to be written, courses planned, programs designed, and library collections built. These pedagogical challenges shaped American academic linguistics through the 1960s and had long-term effects on theory-choice. As linguists established a pedagogical backbone for their discipline, they influenced the training and commitments of the next generation: what linguistics students were taught was crucial to determining their later theoretical leanings. The syntactic theory which could capitalize on the pedagogical market would gain a great advantage in the theory-choice debates.

I show that transformational grammar emerged as an approach to syntax which was, from the pedagogical perspective, easier to *teach*, *learn*, and *use* than its rivals. Transforma-

tional theory captured the textbook market early, and so doing, captured a generation of young linguists. This generation was trained in the transformational paradigm and developed a transformational worldview: to them, the transformation-as-tool was a normal way of mediating between linguistic theory and syntactic phenomena. The pedagogical effectiveness of transformational grammar was enhanced by canonical examples – structured and repetitive demonstrations which were put to effective use by textbook authors. Canonical examples gave linguistics students a foothold to enter what was otherwise a complex and difficult theory. Stratificational grammar, in contrast, was not able to provide students with a simplified entrance mechanism. Finally, I show that the notational techniques of transformational grammar made that theory particularly amenable for both teaching and research. In contrast, rival theories suffered from unrevealing and overly complex notation which came to overshadow their theoretic content. Between its amenability to canonical examples and its visual appeal, the transformation-as-tool fulfilled the needs of students and teachers. Importantly, transformational grammar provided a better pedagogical tool than its rivals at a time when linguistic pedagogy was of real concern. As a result, transformational theory emerged as the most efficient mechanism for handling the rapid growth in enrollment in university linguistics programs of the 1960s.

Chapter 5 – Private Knowledge, Public Tensions – investigates knowledge transmission and the flow of ideas in 1960s American syntax. From 1957 to 1968, transformational grammar operated an underground culture: research was deliberately kept out of mainstream journals, and work was narrowly circulated in mimeograph form among a select group of insiders. Those with close connections to MIT, where the transformational school was centered, had privileged access to new research – and those outside found it difficult, and at times impossible, to access transformational work. Despite this underground culture, transformational grammar diffused across the country and, by the mid- to late-1960s, dominated American academic linguistics. In this chapter, I aim to resolve this apparent paradox: in light of its underground culture, how did transformational grammar spread across the country and gain a majority of supporters?

I argue that three factors are needed to explain this seeming paradox: the dispersion of transformational grammarians from MIT to newly-founded linguistics departments across the country, and the subsequent formation of informal knowledge transmission networks; the access provided to transformational theory through aboveground textbooks; and the oral dispersion of the theory at conferences, colloquia, and workshops. As linguistics departments were established at universities across America in the 1960s, these departments were soon staffed by young transformationalists who brought with them their connections to MIT – and their access to the underground literature controlled by that institution. Many of them shared this literature around their new academic homes, providing students with access to underground works. Further, the early dominance of transformational grammar on the pedagogical market meant that students were trained in transformational theory regardless of access to underground literature – training made possible by the publication of aboveground textbooks in the transformational paradigm. These textbooks formed an openly distributed alternative to underground knowledge, and were a key mechanism of knowledge dispersion in the 1960s. Finally, transformational grammar also spread at conferences, colloquia, and the annual Linguistic Institutes hosted by the Linguistic Society of America – events which were open to students and faculty regardless of theory affiliation. By attending lectures, even those outside of the transformational circle could gain access to new transformational research. These arguments illuminate the success of a theory built on a socio-professional culture which placed high value on private knowledge. They also highlight tensions between public and private knowledge in 1960s American linguistics.

Chapter 6 – The Debate On Other Fronts – widens the scope of investigation to subjects which influenced and were influenced by academic syntax research. In the 1960s, syntactic analysis spilled out into a variety of fields including, most prominently, language teaching (English composition, foreign language learning, and English-as-a-second-language learning), fieldwork (the study of little-known and endangered languages in the field), missionary work (proselytization, Bible translation, and literacy-oriented language analysis), and machine translation (the effort to use computers to translate automatically between

languages). I refer to this group of fields as *lay linguistics*. This chapter explores the relationship between academic syntax and lay linguistics in the 1960s and, specifically, the influence of this relationship on the syntactic theory-choice debates. I investigate the incorporation and application of syntactic theory in lay linguistics, the extent to which linguists from various syntactic schools cared about the perception of their work in lay contexts, and the efforts they took to extend their authority and influence in lay domains. While at times highly technical, the syntactic analysis of the 1960s was not isolated work, but impacted on a number of fields. This chapter recognizes the interconnections between academic and lay work.

I argue that the picture we can draw from the lay scene is one of inconsistency and practicality. In lay fields, the strong lines which existed on the academic scene between rival syntactic theories were blurred. I show that, first, lay linguists were motivated primarily by practicality and applicability, and were little concerned with theory commitment. Lay practitioners assembled what they saw as the advantages of each syntactic theory into a toolbox amenable to their work: theories which were rivals on the academic circuit were taken apart piece by piece on the lay scene and scavenged for useful parts. Second, by providing no consistent response to lay work, academic linguists effectively drew a boundary between the two contexts. Finally, I argue that the divide between academic and lay linguistics is best understood as a conceptual divide between *linguistic theories* and *linguistic tools*. Together, these arguments show that the theory-choice debates which consumed academic linguists in the 1960s were not mirrored on the lay scene. As such, lay practitioners had little influence in academic linguistic circles and, consequently, little influence on the fates of the three rival syntactic theories of the 1960s.

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By contextualizing 1960s American syntax around the themes of explanatory criteria, pedagogy, knowledge transmission, and lay linguistics, this study depicts the syntactic theory-choice debates as a dynamic and far-reaching period in American linguistics. Informed by history of science methodology, it builds on current issues by investigating the relation-

ship between theories and tools in the linguistic context and by emphasizing pedagogy as a key player in theory transmission. Further, it challenges existing literature by demonstrating the importance of the rival syntactic theories of the 1960s; the technical apparatus and tools provided by syntaticians; pedagogy and the training of linguistics students; and the transmission of linguistic knowledge. Together, my arguments explain *why* transformational grammar prevailed over alternative syntactic theories, and *how* transformational theory spread across America despite its pervasive underground culture. More broadly, this study highlights interrelationships between the theoretical, pedagogical, academic, and practical manifestations of scientific theories. The confluence of these themes provides a powerful way of investigating theory development, theory choice, and theory change.

2 A New Era for Linguistics

Over the course of the first half of the 20th century, American linguistics was transformed from a pre-professional discipline with few practitioners and little formal structure into a robust and widely-known field of academic study. Journals were created, professional societies founded, university departments and degrees inaugurated, and funding secured. Internal changes, too, pervaded the field: interests shifted from fieldwork and the description of Amerindian languages to theoretical work and the study of syntax; linguistic philosophy followed the broader movement from behaviorism to mentalism; and the rise of computers in the post-World War II years impacted on syntactic thinking and methodology. This chapter provides an overview of American linguistics from the early 20th century to the 1960s, culminating with a detailed exposition of the three syntactic theories on which this study centers. By highlighting the internal and external changes which shaped the discipline, this overview aims both to introduce readers with little background in linguistics to the relevant socio-professional, intellectual, and technical ideas, and to provide linguistically-sophisticated readers with an historical backdrop to the study.

2.1 American Linguistics: An overview

American Linguistics in the Early 20th Century

Prior to World War II, American linguistics enjoyed few of the hallmarks of a professional discipline: there were no university linguistics departments or degrees, few national meetings and conferences, and linguistics had little independence in academia. In the late 1920s,

the University of Texas at Austin's Archibald Hill recalls, linguistics was subordinate to English – something that "had to be gotten out of the way before a real study of texts could begin" – and universities typically employed "one linguist, and heaven knows, no more".[1] What little support the discipline received came from the Linguistic Society of America, a professional body formed in 1924 to provide an institutional backbone to a fledgling field of study. From the outset, the Linguistic Society successfully fostered a sense of community among American linguists – and with community came communication: a year after its inauguration, the Linguistic Society launched a journal, *Language*, which for decades dominated linguistic publishing in America. The Linguistic Society also encouraged the exchange of ideas through its twice yearly meetings (one held in the summer, and one between Christmas and the New Year), as well as the annual Linguistic Institutes. Begun in 1928, Linguistic Institutes were held every summer on a university campus, and made available courses and guest lectures to students whose home universities offered little in the way of language study. In the pre-World War II decades, linguists – that is, scholars with a particular interest in language – were found in a diverse set of disciplines: anthropology, English, literature, and classics, among others. Linguistics was seen as a tool, variously used for understanding and preserving Amerindian culture, for studying ancient dialects of Greek and Sanskrit, for analyzing verse and prose, for missionary work and proselytization, and for investigating familial connections between language groups. With the creation of the Linguistic Society, scholars working in these diverse fields could, for the first time, see their language-based studies as common and related.

The main force behind the Linguistic Society of America was the man who would lead American linguistics as the discipline came into its own in the 1930s: Leonard Bloomfield. Born in Chicago in 1887, Bloomfield was educated in Indo-European languages at Harvard, the University of Wisconsin, and the University of Chicago. He spent his career as a German instructor at the University of Illinois (1913–1921), in the Department of German

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[1]Archibald A. Hill (ed.), "How Many Revolutions can a Linguist Live Through?," in Boyd H. Davis and Raymond K. O'Cain (eds.), *First Person Singular: Papers from the Conference on an Oral Archive for the History of American Linguistics (Charlotte, N.C., 9–10 March 1979)* (Amsterdam: John Benjamins B.V., 1979), p 73.

and Linguistics at Ohio State University (1923–1940) and, finally, as Sterling Professor of
Linguistics at Yale. While his early work focused on German and Tagalog, he soon became
interested in Amerindian languages and produced groundbreaking studies of Algonquin
(spoken in the Great Lakes region) and Menominee (spoken in Northeastern Wisconsin). It
is, however, Bloomfield's contribution to the systematization of linguistics for which he is
best known. Beginning in 1914 with the publication of his *An Introduction to the Study of Lan-
guage* (Henry Holt and Co.), Bloomfield worked throughout his career to build the study of
language into a scientific discipline – one which treated natural language as a phenomenon
conducive to formal study, analysis, and understanding, similar to the phenomena studied
by physicists and chemists. Central to Descriptivism, as his program was known, was the
belief that a mechanistic and behaviorist philosophy would raise linguistics into the fold of
the natural sciences.

From a theoretical perspective, Descriptivism sought to develop mechanical procedures
which could, given proper input data, identify the phonemes (phonological, or syllable-
level, units), morphemes (morphological, or word-level, units), and syntactic (sentence-
level) units of any natural language. The linguist's first task, Bloomfield wrote, is the
"analysis of a language into distinctive sounds" – that is, into phonemes.[2] Technically,
the phonemes of a language are a finite set of discrete sounds "each of which is, for the lan-
guage absolutely uniform and absolutely distinct from the others".[3] They are determined
by isolation through minimal pairs: for example, the non-substitutability of the English
words *map* and *mat* indicates that /t/ and /p/ belong to different phonemes. However,
the English sounds [t] (as in *mat*) and [tʰ] (aspirated as in *tap*) have no effect on substitutabil-
ity, and hence both belong to the phoneme /t/ (these are called *allophones* of a phoneme).
The identification of phonemes is language specific: in Korean, for example, [t] and [tʰ] be-
long to separate phonemes, since they can be distinguished by a minimal pair. When the
linguist has completed the identification of all phonemes in the language, Bloomfield con-

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[2]Leonard Bloomfield, "Sentence and Word," *Transactions and Proceedings of the American Philological Asso-
ciation* 45 (1914), p 65.
   [3]Leonard Bloomfield, "On Recent Work in General Linguistics," *Modern Philology* 25/2 (1927), p 216.

tinues, he then turns "to the analysis of [...] the morphology and syntax of the language, its grammatical system".[4] Each of these levels of the grammar is analyzed in turn, using the same methodology as for the phonemic level. The use of minimal pairs and substitutability (often called distribution) allows the linguist to mechanically determine each level of the grammar with as little reference to semantic content as possible. As a result, Bloomfield emphasizes, Descriptivism is "a scientific process [which] abstract[s] from a series of actual speech utterances [...] their systematic patterning".[5]

Bloomfield's behaviorist philosophy manifested itself in his data methodology and his careful treatment of semantics. For Bloomfield, proper linguistic data consisted of *documentary utterances*; that is, unsolicited speech acts performed by native speakers in a natural setting and collected by means of transcription or recording. He disallowed any data obtained from intuition or by reference to mental states, arguing that mental processes "add nothing to the discussion, but only obscure it".[6] Bloomfield further asserted that in order to study meaning scientifically, linguists would require "a scientifically accurate knowledge of everything in the speaker's world" – an impossible demand which necessitated that semantics only be allowed to enter linguistic inquiry under highly controlled circumstances.[7] Bloomfield's opposition to mental constructs and his insistence on logical, mechanical procedures in linguistics followed the behaviorist and logical positivist spirit of pre-World War II American intellectualism. His conception of linguistics as a self-contained scientific discipline represents an important break between American and European linguistic traditions: whereas Descriptivism rejected the study of meaning, European linguists maintained that the study of language should be intimately connected to semiotics and semantics.

Bloomfield's Descriptivism dominated linguistic thought in America from the 1930s to the 1950s. His program was laid down in his 1933 book *Language* (Holt, Rinehart and Win-

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[4]Bloomfield, *Sentence and Word*, p 65.

[5]Bloomfield, *General Linguistics*, p 218. For detailed studies of Bloomfield, the reader is directed to Julie Tetel Andresen, *Linguistics in America: 1769–1924, a critical history* (London: Routledge, 1990), Matthews, *Grammatical Theory*, and Robert A. Hall Jr (ed.), *Leonard Bloomfield: Essays on his life and work* (Amsterdam: John Benjamins, 1987).

[6]Leonard Bloomfield, *Language* (New York: Holt, Rinehart and Winston, 1933), p 17.

[7]*Idem*, p 139. The reader is referred to Matthews, *Grammatical Theory* for further details.

ston), written while Bloomfield was a professor at Yale. *Language* soon became the standard textbook and training manual for language study in America, and it maintained its status as the go-to source for linguists on questions from fieldwork to theory to philosophical commitments into the 1950s. "[F]rom very soon after it was published, for over two decades, North American linguists assumed that others had read it and were familiar with it", wrote linguist H.A. Gleason: "[a]ccepted or not, a position set forth in *Language* could be taken as a common reference point".[8] As a shared element of training, Bloomfield's manuscript would come to define mid-20th-century American linguistics, and would be the standard against which the new linguistic theories of the 1950s and 1960s would be measured.

Between the publication of *Language* and the outbreak of the War, several hundred American scholars adopted Descriptivism, and became known as the Bloomfieldians.[9] In these years, no American universities had linguistics departments, and few had courses or programs in linguistics. As such, the Bloomfieldians – a group which would emerge as America's first generation of professional linguists – were not trained in linguistics *per se*, but in a variety of related disciplines from literature to philosophy to anthropology. "If I try to summarize the kind of education I had been given", recalled Archibald Hill (1902–1992), who studied at Stanford and Yale, "it must be said that linguistics was slighted. There were no departments or even programs with that name. I had managed to sneak in as much linguistics as I could, but I would have been deeply grateful for a lot more".[10] Stanford sociolinguist Charles Ferguson (1921–1998) trained at the University of Pennsylvania, where "there was no department of linguistics and no 'major' in linguistics, either undergraduate or graduate".[11] Studying at the Universities of Kansas and Wisconsin, Dwight Bolinger (1907–1992) only became aware of linguistics after completing his Ph.D. in 1936, when he tripped over an article by anthropological linguist Kenneth Pike – an article which was "an

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[8] Gleason Jr, *Theories in Conflict*, p 6.

[9] In the literature, this group is variously called the Bloomfieldians, the post-Bloomfieldians, and the neo-Bloomfieldians. I will consistently called them the Bloomfieldians.

[10] Hill, *Revolutions*, p 72.

[11] Charles A. Ferguson, "Long-term Commitments and Lucky Events," in Konrad Koerner (ed.), *First Person Singular III: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1998), p 45.

inspiration to make language an object of serious study instead of dabbling", and which precipitated Bolinger's career as a linguist and educationalist at Harvard.[12] The common element among this disparate training was Bloomfield's *Language*, which a generation of American linguists read "hot off the press", either formally in a classroom setting, or informally as self-study.[13]

In the pre-World War II years, American linguistics was intimately associated with anthropology. Following the pioneering work of Franz Boas, Descriptivists focused their efforts on transcribing, recording, and analyzing previously unwritten Amerindian languages. Fieldwork in Amerindian communities, Gleason recalls, was "a standard part of the apprenticeship into the linguistic profession".[14] Intense exposure to an Amerindian language was an initiation rite for a generation of linguists: among others, Charles Hockett began his career with a study of Potawatomi, C.F. Voegelin with Algonquin, and Sydney Lamb with Monachi. The application of Descriptivism as an ethnographic tool shaped the research questions asked by linguists and guided the development of a young discipline. "In the first place, no texts were at hand", Alphonse Juilland and Eugene Elliott said in 1957: "[t]he linguist had to create his object, first by recording the spoken chain of sounds furnished by some native informant, and then by analyzing this corpus to identify its constitutive elements, the meaningful sequences of sounds or forms. [...] The primary problem was that of 'breaking the code', or of recognizing the significant units".[15] With these priorities, pre-World War II American linguists concentrated not on theory, but on field methods: linguistics was a matter of elicitation, recording, and description, and its practitioners had little time for theoretical argumentation or abstract reasoning.

While the two decades leading up to the Second World War endowed American linguists

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[12] Dwight L. Bolinger, "First Person, Not Singular," in Konrad Koerner (ed.), *First Person Singular II: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1991), p 30.

[13] Charles F. Hockett, "Preserving the Heritage," in Boyd H. Davis and Raymond K. O'Cain (eds.), *First Person Singular: Papers from the Conference on an Oral Archive for the History of American Linguistics (Charlotte, N.C., 9–10 March 1979)* (Amsterdam: John Benjamins B.V., 1980), p 100.

[14] Gleason Jr, *Theories in Conflict*, p 16.

[15] Alphonse G. Juilland and Eugene Elliott, "Perspectives of Linguistic Science," in Paul L. Garvin (ed.), *Report of the Seventh Annual Round Table Meeting on Linguistics and Language Study, 1956* (Washington DC: Georgetown University Press, 1957), p 181.

with a professional association, a journal, and a burgeoning sense of community, still the discipline was small and scattered. Few academics were employed as linguists *per se*, and the discipline labored under the shadow of better-established fields. This situation was to change rapidly and dramatically during World War II, when American linguistics rose to prominence as a strategic and independent professional discipline.

American Linguistics and World War II

As hostilities broke out in Europe, and especially as the War came closer to home with the 1941 attack on Pearl Harbor, American linguists and the American military began to see eye-to-eye on the strategic importance of language. Too few Americans were familiar with languages other than English, and the education system offered few opportunities for foreign language learning – a situation which was unacceptable, linguists and the military agreed, in an era when cooperation with allies around the world was of the essence. If America was to play a central role on the world stage, it would need to bring its "linguistic isolationism" to an end.[16] As linguists were pressed into service to rectify America's language deficiencies, there emerged a stronger, tighter professional community. For the first time, American linguists worked together in large numbers on joint projects, and the study of language *per se* was seen as a dynamic and vital endeavor.

In 1939, even before America's involvement in the War, Mortimer Graves – the executive secretary of the American Council of Learned Societies – voiced the need for America and Americans to develop competence, and quickly, in foreign languages. The paucity of foreign language teaching in American schools, he argued, meant that Americans lacked the ability to communicate in, or even understand, languages other than English.[17] As a private and nonprofit federation of scholarly organizations in the humanities and social sciences, the American Council of Learned Societies had long been involved in linguistics: in

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[16]William G. Moulton, "Linguistics and Language Teaching in the United States, 1940–1960," in Christine Mohrmann, Alf Sommerfelt, and Joshua Whatmough (eds.), *Trends in European and American Linguistics, 1930–1960 (Edited on the occasion of the Ninth International Congress of Linguists, Cambridge MA, 27 August – 1 September 1962)* (Utrecht: Spectrum Publishers, 1970), p 84.

[17]*Idem*

the 1920s, the Council supported the effort of linguists to "secure an adequate record of Indian languages and dialects", and funded the first Linguistic Society of America Linguistic Institute, held at Yale in 1928.[18] With additional funding from the Rockefeller Foundation – the most significant patron of American social science and medicine between the two World Wars – Graves created the Intensive Language Program, which aimed to expand the expertise of American linguists from Amerindian languages to the potentially strategic languages of the world, and to spread this expertise through language instruction. The Intensive Language Program gained its first linguists in 1941 and, within two years, was offering "no less than 56 courses, in 26 languages, at 18 universities, involving a total of some 700 students".[19]

Graves' concerns about the lack of linguistic capacity among young Americans soon came to be shared by the American military. In the spring of 1943, the military piggybacked on the Intensive Language Program and greatly expanded its efforts in Washington and New York City. Linguists already in the draft were pulled from their duties and put to work on language-related problems, and linguists not yet involved in the War effort were brought in to the Intensive Language Program. They worked on developing language-learning materials, dictionaries, and phrase books for a plethora of languages identified by the military as being of immediate or potential importance to Allied security and intelligence. They also worked on linguistic aspects of code breaking. Within a year of the launch of the joint civilian-military effort, 15,000 members of the American armed forces were being trained in over 27 languages on more than 50 university campuses across the country.[20] This language instruction aimed to develop practical language ability in the shortest timeframe possible, for immediate use by officers heading overseas and for future use in postwar activities.

Linguists tackled this work with the same methods they had successfully employed for decades in their analysis of Amerindian languages: faced with an unfamiliar language,

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[18] Annual Meeting Proceedings ACLS, "American Council of Learned Societies Annual Meeting – 1928," in *Bulletin 7*, American Council of Learned Societies (1928), p 53.

[19] Moulton, *op. cit.*, p 85.

[20] *Idem.*

they were able "very quickly to prepare preliminary analyses [and] phonemic transcriptions", and to translate these tools into classroom materials.[21] Time and confidence were indeed of the essence: when he was assigned to teach conversational Chinese to officers en route to join General Stillwell's campaign in China, Charles Hockett had no knowledge of Chinese languages. "I had had training in linguistics and in those days we were claiming that that was enough", he wrote in a 1980 autobiographical piece: "we could learn the language faster than our linguistically unsophisticated students could, and thus keep ahead of them".[22] Closer to home, and representative of the sweeping variety of work faced by linguists during the War, Robert Hall spent the War years analyzing Melanesian Pidgin English, preparing language-learning materials for Spanish, Italian, and French, and supervising the Army's Italian language teaching program at Yale.[23]

The war effort mobilized a vast majority of working linguists in 1940s America: of the 96 participants at the Linguistic Society of America's 1944 annual meeting, approximately 80 were actively engaged in "militarily crucial work" and being paid by the American Council of Learned Societies or by the government.[24] Giants in the field, including Fred Lukoff, Morris Swadesh, Fred Householder, and Leonard Bloomfield himself, worked on languages ranging from Japanese to Norwegian to Moroccan Arabic. "It is safe to say", William Moulton concluded, that "before the [Intensive Language] Program was over, just about every trained linguist in the country, young or old, had become involved in it in one way or another".[25] As American linguists worked together towards common goals for the first time, a strong professional community began to take shape. After the War, as linguists returned to their home institutions, they retained these connections: no longer were they isolated and without a sense of common purpose.

Throughout the War, linguists faced "very heavy pressure" to produce results – and re-

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[21]H.A. Gleason Jr, *Linguistics and English Grammar* (New York: Holt, Rinehart and Winston, Inc., 1965), p 49.
[22]Hockett, *Preserving the Heritage*, p 103.
[23]Robert A. Hall Jr (ed.), "Layers of Linguistics," in Konrad Koerner (ed.), *First Person Singular II: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1991), p 177–179.
[24]Martin Joos, *Notes on the Development of the Linguistic Society of America, 1924–1950*, p 131.
[25]Moulton, *op. cit.*, p 84.

sults they did produce.[26] The 1940s were a decade of intensive growth and, in the early years of the decade, American linguists "stockpiled linguistic experience at an incredible rate".[27] For the first time, the work of American linguists was recognized and valued by outsiders: the American military and government came to see linguistics as vital to winning the War and establishing order in the postwar world, and universities took note of progress in foreign language teaching and pedagogical applications of linguistic theory. No longer second-class to anthropology or literature, linguistics began to distinguish itself on the academic scene. During the War, American linguists proved their value – and, in the following years, they became the go-to experts on language-related matters from machine translation to second language instruction to strategic linguistic analysis.

The Postwar Years

In postwar America, linguistic publications proliferated, linguistic theory took on new life, and linguists gained respect on the academic and Cold War funding scenes. With this new status came a pressing need to publish linguistic research. *Language*, the journal of the Linguistic Society of America, was no longer sufficient to cater to the growing linguistics community. By the end of the War, several more linguistics periodicals were circulating in the United States, including *Studies in Linguistics* (founded by George Trager in 1942), *Word* (founded by the Linguistic Circle of New York in 1945), and *The International Journal of American linguistics* (founded by Franz Boas in 1917, this journal ceased publication in 1939 and was reestablished by C.F. Voegelin in 1944–1945). Catering to those who saw linguistics as intimately linked to anthropology, *The International Journal of American Linguistics* focused on Amerindian languages and stressed data and description over theory. *Word*, which soon grew to rival *Language* in prestige, emphasized both European and American approaches to linguistics, and provided an outlet for European scholars who had come to America

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[26]Robert A. Hall Jr (ed.), "165 Broadway – A crucial node in American structural linguistics," *Historiographia Linguistica* 28/1 (1991), p 159.

[27]W. Freeman Twadell, "Linguistics Plus," in Richard S. Harrell (ed.), *Report of the Tenth Annual Round Table Meeting on Linguistics and Language Studies, 1959* (Washington DC: Georgetown University Press, 1959), p 147–148.

during or soon after the War. Its sponsor, the Linguistic Circle of New York, was modeled after the Société de la Linguistique de Paris, and provided a home base for, among others, Russian linguist Roman Jakobson, who fled his adopted Prague for the United States in the early 1940s, and Polish semiticist Wolf Leslau, who escaped to the United States in 1942. As such, *Word* issued an early challenge to the Bloomfieldian dominance of the American linguistics profession.

The postwar growth of American linguistics is clearly visible within the Linguistic Society of America. Membership in the Linguistic Society ballooned from 550 in 1940 to 800 in 1950 to 1800 in 1960.[28] The Society's Linguistic Institutes continued to act as key meeting places, bringing together for six to eight weeks each summer linguists who were normally dispersed across the country. The Linguistic Institutes played a central role in training young linguists, advancing theory, and fostering professional links: by attracting "many prominent linguists as teachers and visitors and [becoming] a forum for exchange of ideas among leaders in the profession and a fertile source of new advances", the Linguistic Institutes enabled the community to grow on academic, theoretical, and professional levels.[29] The range of courses offered at the Institutes was far broader than that offered at any university at the time: at the 1955 Institute held at the University of Chicago, for example, students could attend courses in Classical Nahuatl (spoken in central Mexico), Iroquoian languages, and mathematics and mechanical aids in linguistics. As Berkeley philosopher John Searle recalls, post-World War II American linguistics was a "rather cozy" discipline where practitioners assembled every summer at the Linguistic Institute in order to thrash out issues and air "family squabbles [...] in public meetings".[30] As linguistics courses became more readily available at universities in the mid to late 1950s, the Linguistic Institutes continued to provide a regular and reliable meeting place for linguists usually scattered across a vast country.

The 1950s also saw an explosion of linguistics conferences held outside the auspices of

[28]Proceedings LSA, Linguistic Society of America Annual Meetings (Language Supplement), 1957-1970.

[29]Gleason Jr, *Linguistics and English Grammar*, p 65.

[30]John Searle, "Chomsky's Revolution in Linguistics," in Gilbert Harman (ed.), *On Noam Chomsky: Critical essays* (Garden City: Anchor Books, 1974), p 8.

the Linguistic Society of America. The Georgetown Round Table Meetings on Linguistics and Language Studies were inaugurated in 1950 by Leon Dostert, the first director of the Institute of Languages and Linguistics at Georgetown University, and a giant in American machine translation. Held annually, the Georgetown Round Table Meetings were – and continue to be today – an important event on the linguistic calendar, traditionally bringing together applied and theoretical linguists for several days of discussion and debate. The most famous of the 1950s linguistics conferences were the four Texas Conferences, organized by Archibald Hill at the University of Texas at Austin between 1956 and 1960. With his status as *doyen* of the American linguistics profession in the 1950s, Hill attracted large numbers of linguists to his conferences, and organized lively and occasionally confrontational sessions. It was at the Third Texas Conference, held in 1958, that Noam Chomsky first presented his ideas on syntax to a broad public audience.

Recognition of linguistics as an independent discipline in the academic context was slower: while the first linguistics department in America was founded in 1946 at the University of Pennsylvania, in the 1950s still only a few universities employed more than a handful of linguists.[31] At the upper end, Cornell University's Division of Modern Languages, described by Charles Hockett as "in effect, a Linguistic Institute in permanent session", employed five linguists in 1946, and the University of Michigan had "a half-dozen or more linguists on staff spread through a few different departments".[32] Doctoral work remained focused on describing Amerindian languages, and hands-on fieldwork still dominated linguistic research. Changes occurred more rapidly on the textbook scene. The 1950s saw the publication of several linguistics textbooks which, together, made "the task of initiating new students to the field, both inside and outside of formal lecture courses, a far simpler and dramatically changed problem from the days when Bloomfield's *Language* had to serve

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[31]Declaring anything to be 'the first' can open a Pandora's box. While three other American universities have laid claim to the 'first' linguistics department in that country (the University of Chicago, the University of California at Berkeley, and Yale University), it was the University of Pennsylvania which was the earliest to establish a linguistics department which has been continually active from its foundation to the present day.

[32]Hockett, *Preserving the Heritage*, p 104–105, James W. Ney, "Linguistics from the Bottom Up," in Konrad Koerner (ed.), *First Person Singular III: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1998), p 134.

as the sole tool for all purposes".[33] Written by established linguists Archibald Hill (University of Texas at Austin), Charles Hockett (Cornell University), H.A. Gleason (Hartford Seminary Foundation), and Zellig Harris (University of Pennsylvania), these textbooks opened the door to university training which treated linguistics as separate from anthropology, English, and literature.

With the Second World War, linguistics funding passed from philanthropist bodies such as the Rockefeller Foundation and the Carnegie Corporation to the American military – and the following decade marked the beginning of massive government investment (both military and civilian) in linguistics. This was part of a much broader pattern of government investment in the natural and social sciences.[34] On the military side, the United States Armed Forces continued to see language capacity as a vital Cold War weapon, necessary for integrating Americans on mission outside of the United States and for developing much-desired automated Russian-to-English translation techniques. "In this war for men's minds", remarked Mortimer Graves in 1951, "obviously the big guns of our armament is competence in languages and linguistics [sic]".[35] In the civilian arena, the United States Department of State began to invest in linguistics in 1946 with the establishment of language training facilities at the Foreign Service Institute, intended to increase the foreign-language capacity of the American diplomatic corps. Henry Lee Smith Jr., George Trager, Robert Stockwell, and other established linguists accepted contracts with the Foreign Service Institute, where they were able to combine language teaching with basic linguistic research. Interest in linguistics – and, consequently, funding for linguists – ballooned after the launch of Sputnik in 1957. With the passage of the National Defense Education Act in 1958 – designed to improve the American education system in areas of mathematics, sciences, and

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[33]Eric P. Hamp, "General Linguistics – The United States in the fifties," in Christine Mohrmann, Alf Sommerfelt, and Joshua Whatmough (eds.), *Trends in European and American Linguistics, 1930–1960 (Edited on the occasion of the Ninth International Congress of Linguists, Cambridge MA, 27 August – 1 September 1962)* (Utrecht: Spectrum Publishers, 1970), p 166.

[34]For detailed studies of American government investment in the natural and social sciences through the 20th century, the reader is directed to Geiger, *op. cit.*, Stuart W. Leslie, *The Cold War and American Science: The military-industrial-academic complex at MIT and Stanford* (Columbia: Columbia University Press, 1993), and Solovey, *op. cit.*.

[35]Newmeyer, *The Politics of Linguistics*, p 56.

foreign languages – academic linguists found themselves funded by civilian arms of the United States government from the National Science Foundation to the National Institutes of Health. Established in 1950 and 1930, respectively, the National Science Foundation and the National Institutes of Health are two of the key players on the American academic funding scene, commanding a combined budget of nearly $2 billion in the mid-1960s.[36] In post-Sputnik America, government funding bodies agreed, language knowledge was no longer "a luxury for the academically talented", but was considered to be "essential for everyone".[37]

As the 1950s progressed, American linguists found their expertise increasingly valued outside of academic and government circles. Teachers and school administrators began to see a role for linguistics in language teaching, and turned to linguists as the professionals uniquely possessing the knowledge about language structure required to apply linguistic theory in the classroom. In response, linguists published textbooks directed to elementary, high school, and university teachers on English grammar, composition, and foreign-language learning. As the number of foreign students studying in American universities increased in the postwar years, linguists were further called upon to develop English-as-a-second-language programs. The recognition of linguists as the expert class on language-related matters in America represents a critical step in the emergence of linguistics as a professional discipline.

Linguistic Theory in Post-World War II America

The disciplinary changes which shaped American linguistics in the postwar years were not restricted to the status and professional identity of linguists, but also had tremendous impact on linguistic theory: the postwar decade marks the turn from the study of phonology and morphology, which had dominated American linguistics during the 1930s and 1940s, to the study of syntax, which would consume the majority of effort in the 1950s and 1960s. The defining linguistics project of the immediate postwar years was machine translation,

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[36] Herbert Roback, "Congress and the Science Budget," *Science* 160 (1968).
[37] Gleason Jr, *Linguistics and English Grammar*, p 482.

propelled and financed by an American military which desired the ability to automatically translate from Russian and German into English. Machine translation researchers from Victor Yngve at MIT to Paul Garvin at Georgetown soon realized that the difficulties "standing in the way of the development of translating machines would be more serious than the technical computer difficulties" – and the chief difficulty, they agreed, was the lack of understanding of syntax.[38] Coupled with a growing feeling among Descriptivists that the study of phonology and morphology was nearly completed and that sentence-level constructions were the next logical aspect of the grammar to tackle, interest in and demand for syntactic research grew through the 1950s. By 1955, syntactic research was well underway and a rigorous understanding of sentence structure "no longer seem[ed] an unattainable goal".[39]

The 1930s and 1940s saw great progress on phonology and morphology. Following the Bloomfieldian program, Descriptivists described and analyzed language by working from the lowest to the highest level: the analysis of any given corpus proceeded from sounds to syllables to words to sentences. Linguists were guided by the no-level-mixing constraint, which disallowed work on any particular linguistic level from making reference to higher levels – thus phonology had to be established without reference to morphology; morphology without reference to syntax; and so on. This methodological approach resulted in a natural progression towards higher levels of linguistic analysis, with syntax as a final step to be tackled only after gaining a full understanding of lower levels. If the 1930s was the decade of phonology, the 1940s shifted attention to morphology, and saw much progress on the understanding of word-level phenomena. By the 1950s, as linguists began to feel confident in their mastery of morphologic patterns, they turned their attention to syntax. In this decade, American linguists by and large chose linguistic theory – and specifically syntactic theory – as their main area of research. Indeed, historians Dell Hymes and John Fought have argued that the 1950s mark the first time that "none of the many other involve-

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[38]Victor H. Yngve, "The Dilemma of Contemporary Linguistics," in Adam Makkai and Valerie Becker (eds.), *First LACUS Forum* (South Carolina: Hornbeam Press Inc., 1974), p 4.

[39]H.A. Gleason Jr, *An Introduction to Descriptive Linguistics* (New York: Henry Holt and Company, 1955), p 139.

ments of language in other kinds of problems – historical, cultural, social – could claim the center of attention within linguistics".[40]

In the early Cold War, interest in sentence-level phenomena received a direct and practical boost from the American military's desire for machine, or automated, translation. While America requires "ready, undelayed access to scientific information written in the languages of the several scientifically creative cultures of our day", said Leon Dostert in his closing address to the 1957 Georgetown Round Table Meeting, the number of Americans capable of rapidly and accurately translating from Russian and German into English was much too small to keep pace with the scientific output of those two nations.[41] Rather than training more human resources, the chosen solution was to apply "the supremacy of the U.S. in computer technology and financial resources" to develop mechanical translation methods.[42] In one of the first uses of computers for non-numeric tasks, the American military poured funding into machine translation efforts through the 1950s and early 1960s.

As machine translation gained in prominence through the 1950s, it was pursued at universities and in the private sector. At locations from MIT to UCLA to Georgetown University, and from private companies to the nonprofit think tank RAND, researchers aimed to improve America's intelligence capabilities through automatic translation. This work was continually motivated by reported machine translation successes in the Soviet Union, including a purportedly successful English-to-Russian translation at the Institute of Precision Mechanics and Computer Technology of the USSR Academy of Sciences in 1956. The launch of Sputnik a year later sparked America's science-funding bodies into action, and guaranteed the continuation of military support for machine translation efforts for years to come.

The premise of early 1950s machine translation was to equip a computer with a set of formal rules which, when applied to an input text in language A (usually Russian or German), would produce an output translation in language B (usually English). These rules worked

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[40]Dell Hymes and John Fought, *American Structuralism* (The Hague: Mouton Publishers, 1975), p 122.

[41]Leon Dostert, "Practical Objectives of Machine Translation Research – Closing luncheon address," in Leon Dostert (ed.), *Report of the Eighth Annual Round Table Meeting on Linguistics and Language Study, 1957 (Research in Machine Translation)* (Washington DC: Georgetown University Press, 1957), p 182.

[42]Yehoshua Bar-Hillel, *Language and Information: Selected essays on their theory and application* (Reading MA: Addison-Wesley Publishing Company, Inc., 1964), p 7.

not by deciphering the meaning of the input text, but by using a lexicon and knowledge of the syntactic structure of the input language to build a corresponding output translation. Here, an understanding of syntax is critical for recognizing the various components of sentences and identifying their function. "We can consider for each language what are its major morpheme classes (and their subclasses down to some level), and what are the main combinations of these classes into its various successively larger constructions (word, phrase, clause, or the like) until we get up to its full sentences", wrote Zellig Harris in a 1954 article in *The International Journal of American Linguistics*, and "we can ask what changes would have to be made in such a structural sketch of one language in order to obtain out of it a structural sketch of the other (at the same level of detail). Such a list of changes would generate the utterances of one language out of those of the other, since the grammatical sketches of each language yield the utterances of that language (up to some level of detail), so that transferring from one sketch to the other will suffice to transfer from one set of sentences to the other".[43] It is clear that any success in machine translation would hinge on knowledge of the syntactic structure of the input and output languages, as well as a solid understanding of general syntactic processes. Accordingly, American linguists turned their attention to the development of formal rule-based syntactic theories, and to the analysis of these theories via mathematical and computer techniques. Mathematical methods – especially from modern algebra – were seen to be immensely useful because they enabled linguists to capture sentence structure as a purely formal system without resorting to meaning. While machine translation fell from fashion in the late 1960s, and while it is still today considered remarkably unsuccessful, in the 1950s and early 1960s it was a project fueled by great optimism and confidence.

Since Bloomfield, Descriptivists had been intent on raising linguistics into the fold of the natural sciences. Envious of the status and prestige of disciplines such as physics and chemistry, they worked to establish objective data collection mechanisms and formal analytical procedures. In large part because of the increased use of formalization in the study

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[43]Zellig S. Harris, "Transfer Grammar," *International Journal of American Linguistics* 20/4 (1954), p 260.

of syntax, Descriptivism had by the 1950s succeeded in developing a reputation both inside and outside the discipline as a serious scientific endeavor. "The discovery that language consists of phonemes and morphemes", wrote Claude Lévi-Strauss in 1953, could be "compared [...] to the Newtonian revolution in physics".[44] Noting the elegance of their analysis and the mathematization of their subject matter, linguists themselves proclaimed their field to be the most exact and precise of the social sciences.[45] "I became convinced that linguistics studied stable and repeatable phenomena like the phenomena found in cosmic ray physics", wrote Victor Yngve, who began his career in physics before moving to machine translation and then to linguistics, and that "the phenomena were every bit as complex and interesting, and even more important".[46] As confidence grew in the applicability of scientific methodology and norms to the study of language, linguistics became one of the first non-traditional sciences to receive funding from the National Science Foundation.

The idea behind the application of mathematics to linguistics is to consider natural language as an abstract system, or structure, to which mathematical techniques and operations can be applied in order to uncover patterns. In the 1950s, mathematics was seen as a methodological imperative to stating linguistic rules explicitly and to properly evaluating hypotheses about the structure of language. "Certain theoretical issues in linguistics [...] cannot be approached without a mathematical development of the concepts involved", wrote William Cooper later, because "where rigor is needed, so is mathematics".[47] By the middle of the decade, results in mathematical syntax began to be published. Charles Hockett's application of Markov source models to linguistic structure, Noam Chomsky's analysis of three mathematical syntax models, and Yehoshua Bar-Hillel's development of arithmetic notation for syntactic description, were at the forefront of this new branch of linguistic inquiry.[48] Mathematical linguistics first appeared as an academic subject in 1955, in the form

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[44]Quoted in Newmeyer, *Linguistic Theory in America*, p 2.

[45]Gleason Jr, *Linguistics and English Grammar*, p 51.

[46]Victor H. Yngve, "Early Research at M.I.T.: In search of adequate theory," in W. John Hutchins (ed.), *Early Years in Machine Translation: Memories and biographies of pioneers* (Amsterdam: John Benjamins Publishing Company, 2000), p 43.

[47]William S. Cooper, *Set Theory and Syntactic Description* (The Hague: Mouton, 1974), p 11.

[48]Noam Chomsky, "Three Models for the Description of Language," *IRE Transactions on Information Theory* 2 (1956), Yehoshua Bar-Hillel, "A Quasi-Arithmetical Notation for Syntactic Description," *Language* 29/1 (1953).

of a seminar at Harvard University, and in the following years over half a dozen institutions from MIT to the University of Pennsylvania followed suit. By 1960, dovetailing interests between "linguists, logicians and mathematicians" culminated in an American Mathematical Society symposium entitled *The Structure of Language and Its Mathematical Aspects*, held in New York City, which attracted prominent figures including W.V.O. Quine, Hilary Putnam, Nelson Goodman, Noam Chomsky, and Victor Yngve.[49]

The theoretical approach to syntax taken in the 1940s and 1950s was *immediate constituency grammar* (ICG). The fundamental idea of ICG is to treat sentences as hierarchical structures repeatedly divisible into (usually binary) constituents. "In analyzing a given sentence, we first isolate the immediate constituents of the sentence as a whole, then the constituents of each constituent, and so on to the ultimate constituents", wrote Yale University's Bernard Bloch, and the ultimate constituents of the sentence were typically words (or morphemes).[50] For example, the English sentence *the King of England opened Parliament* is analyzed into the immediate constituents *the King of England* + *opened Parliament*, which are then respectively analyzed into *(the + king of England)* + *(opened + parliament)*, and so on, as shown in figure 2.1.

Constituency analysis was systematized and popularized in the 1940s with the publication of four canonical works: Rulon Wells' 1947 article *Immediate Constituents* (*Language*) provided standardized analysis techniques for the theory; Eugene Nida's 1943 University of Michigan doctoral dissertation *A Synopsis of English Syntax* and Bernard Bloch's 1946 article *Studies in Colloquial Japanese II: Syntax* (*Language*) presented constituency analyses of English and Japanese syntax, respectively; and Zellig Harris' 1946 article *From Morpheme to Utterance* (*Language*) extended substitution analysis from the morphemic level to the syntactic level. Wells' article stands out as the first attempt to "replace by a unified, systematic theory the heterogeneous and incomplete methods hitherto offered for determining *immediate constituents*", and is traditionally considered the foundational enunciation of constituency

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[49] Roman Jakobson (ed.), *Structure of Language and Its Mathematical Aspects: Proceedings of Symposia in Applied Mathematics (held in New York City, 14–15 April 1960)* (Providence: American Mathematical Society, 1961), p v.

[50] Bernard Bloch, "Studies in Colloquial Japanese II: Syntax," *Language* 22/3 (1946), p 204–205.
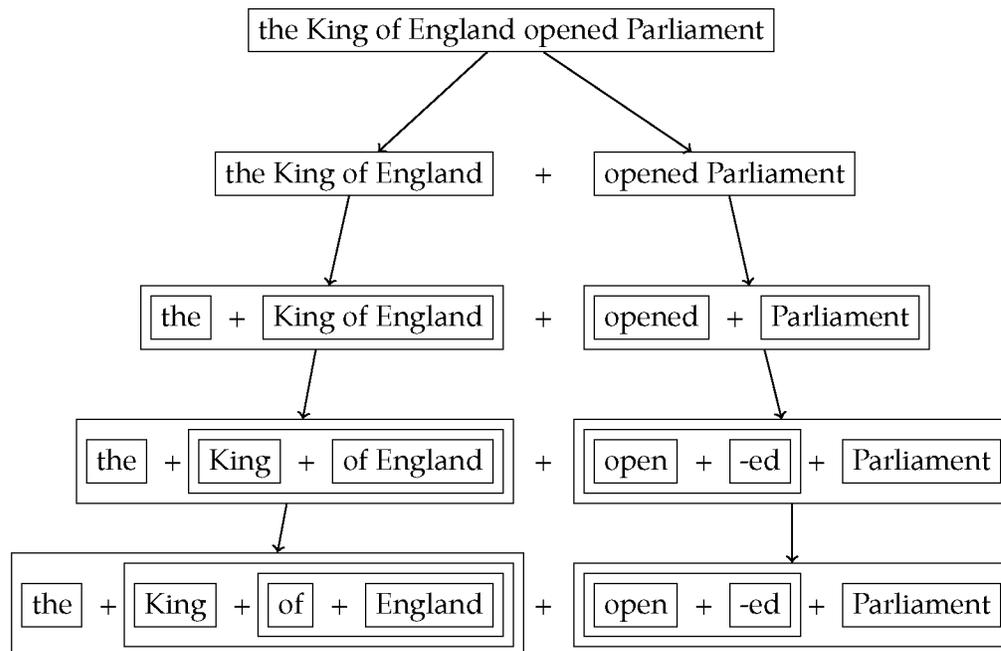
Figure 2.1: Immediate constituency analysis of *The King of England opened Parliament*

theory.[51] Nida's dissertation is also recognized as a classic source, and its extensive analysis of English syntactic constructions has been described as "one landmark on the road to a fully worked out notation of immediate constituents (ics) as a part of syntactic theory".[52]

While immediate constituency analysis was widely used in the 1950s, it faced technical problems which presented steep challenges to constituency grammarians (cf. Section 2.4). The consequent dissatisfaction with icg resulted in the development of several competing syntactic theories. By the early 1960s, two such theories – transformational grammar and stratificational grammar – had grown to challenge the dominance of icg and, by the end of the decade, transformational grammar replaced constituency analysis as the major player on the American academic linguistics scene. It is the competition between these three syntactic theories, and the theoretical, socio-professional, pedagogical, and philosophical implications of this competition, on which this study focuses.

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[51] Rulon S. Wells, "Immediate Constituents," *Language* 23/2 (1947), p 81, emphasis in original.
[52] Gleason Jr, *Theories in Conflict*, p 154.

The changes which swept through American linguistics from the early 20th century to the 1960s were immense: the discipline itself expanded from pre-professional beginnings to a mainstream academic subject, and theoretical interests evolved from fieldwork to the lower levels of grammatical structure to the study of syntax. As the 1960s opened, three syntactic theories were competing for the attention and commitment of American linguists. The rest of this chapter introduces these three syntactic theories in detail. Following an overview of the study of syntax as it was conceived in the 1960s, transformational grammar is discussed in section 2.2; stratificational grammar in section 2.3; and immediate constituency grammar in section 2.4.

Syntax in the 1960s

The syntactic theories of the 1960s – transformational, stratificational, and immediate constituency grammars – shared the broad aim of explaining the sentence structure of natural language, but varied greatly in their technical tools, goals, philosophies, and socio-professional followings. Pursued at universities across America by academics interested in machine translation, anthropology, missionary work, and theoretical linguistics itself, these theories had a competitive and often fierce interrelationship. By the end of the 1960s, transformational grammar – championed by Noam Chomsky and MIT's Research Laboratory of Electronics – had emerged on top as the most widely used syntactic theory in American academic linguistics.

The phenomenon of syntax can be conceived of narrowly as the sentence structure of a particular natural language (say, English, Hindi, or Algonquin), or widely as the overarching structure governing sentence formation in all human languages. At its most basic, the paradigmatic puzzle of syntax is to account for sentence structure in a manner which is simpler than human language itself – that is, given a language L, to characterize that language more simply than a list of all possible (or grammatical) sentences in L. In the 1960s, this puzzle was refined with the criterion of empirical adequacy: the syntactician's task was widely seen to be to "provide a theory which represents the structure that any

physical system must possess if it is to be capable of linguistic communication as we know it".[53] Under this rubric, a syntactic theory must, in its broadest conception, contain some symbolic, formal, or mathematical system which can characterize sentence structure by stipulating restrictions to which every grammatical sentence in a particular language (or in all human languages) must obey. As Zellig Harris wrote in 1966, a syntactic theory must "formulat[e] in a mathematical system precisely those properties sufficient and necessary to characterize the whole of natural language and its unique power".[54]

The fundamental questions faced in the construction of such a syntactic theory are the same as those faced by many sciences: the delineation of acceptable and unacceptable data; the choice of basic units and theoretical notation used to communicate those units; the specification of a relationship between the theory and the real-world phenomena at stake; the choice of evaluation mechanisms for variations on the theory; and the enunciation of the ultimate goals and aims of the theory. The fundamental difficulty faced by syntacticians is likewise reflected in the history of science: just as J.J. Thompson could not directly observe the atom, or Robert Milliken the electron, linguists cannot directly observe the underlying nature of syntax, whether they believe that structure to have a neural, mental, or other manifestation. And as in many other social and natural sciences, linguistic theories can be evaluated on their ability to yield accurate predictions about the system they represent, on their validity with respect to adjacent fields of study, and on their simplicity. But what exactly theoretical representations of syntactic phenomena should entail, what behaviors linguistic theories should account for, and how these theories should be evaluated, were all open to debate in the 1960s. In what follows, we look at how the three competing syntactic theories of the 1960s tackled these questions. These were the theories which defined American linguistics in a decade of upheaval, which would attract attention and funding from the American military and civilian government, and which would have enormous influence in philosophy, psychology, and pedagogy. In turn, transformational, stratificational,

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[53]Jerrold J. Katz, "Mentalism in Linguistics," *Language* 40 (1964), p 129.
[54]Zellig S. Harris, "Algebraic Operations in Linguistic Structure," in *Papers in Structural and Transformational Linguistics* (Dordrecht: D. Reidel Publishing Company, 1966), p 603.

and immediate constituency grammars are discussed first in terms of technical content, and second in terms of socio-professional development and influence on the American linguistics community.

2.2   Transformational Grammar

> Transformational theory has unquestionably been the major development in linguistics in the last decade. And understandably so, since Chomsky's ideas [...] are not a mere rephrasing or continuation of previous linguistic theories, but constitute a truly fresh and revolutionary approach to the study of language.
>
> Heles Contreras, in *The Modern Language Journal* (1967)[55]

> [I]n the opinion of this reviewer, the fact that the scientific description of natural languages presupposes the construction of a transformational theory of grammar of some kind or other is no longer open to serious doubt. All current alternative theories have been shown to be less adequate in principle or have not yet been precisely specified and, if formalized, would probably be correctly described as 'transformational'.
>
> John Lyons, in *The Philosophical Quarterly* (1966)[56]

Transformational grammar (TG), most prominently associated with the name Noam Chomsky, was developed primarily at MIT beginning in the mid-1950s. It can be viewed narrowly as a syntactic tool, or widely as a linguistic program encompassing a syntactic tool, a grammatical theory, and a philosophical stance. The syntactic tool provided by TG is the transformation (an operation which acts on one or more simple sentences to produce semantically-related sentences); the broader program includes a tripartite linguistic theory, a universalist approach to language, and a mentalist and anti-behaviorist philosophy. Transformational grammar is a widely divisive subject, and the literature surrounding the

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[55]Heles Contreras, "Review of Noam Chomsky's Aspects of the Theory of Syntax," *The Modern Language Journal* 51/2 (1967), p 110.

[56]John Lyons, "Review of Noam Chomsky's Aspects of the Theory of Syntax," *The Philosophical Quarterly* 16/65 (1966), p 394.

theory clusters around two poles: that which sees it as an extraordinarily successful theory which has allowed us to "learn [more] about the nature of language in the past 25 years than in the previous 2500",[57] and that which sees it as an "arid, sterile 'hocus-pocus' [theory] with little or no relation to linguistic reality".[58] The emotion attached to these two poles is representative of the enormous influence the Chomskyan program has exerted in American academia both inside and outside of linguistics, as well as the impact of Chomsky's linguistic, psychological, and political views on the general public. In the literature, it is propelled by the proximity of authors to the subject at hand: the vast majority of works on the history of mid-20th-century American linguistics have been written by linguists who themselves were involved in the development of transformational grammar and of rival syntactic theories.

Born in 1928 in Philadelphia, Pennsylvania, Chomsky undertook his undergraduate and graduate studies at the University of Pennsylvania, under the tutelage of the American linguist Zellig Harris. He completed his M.A. thesis in 1951 (*Morphophonemics of Modern Hebrew*) and his Ph.D. in 1955 (*Transformational Analysis*).[59] After several years as a Junior Fellow at Harvard, Chomsky arrived at MIT to take a joint appointment in the Research Laboratory of Electronics and the Modern Languages Department in 1955. Hired as a full-time faculty member, Chomsky was able to split his time between teaching and research. He taught reading courses in French and German as well as an undergraduate course on language, where he fleshed out many of his syntactic ideas.[60]

Chomsky's conception of transformational grammar was released to the American linguistics community with the 1957 publication of *Syntactic Structures* – a slim monograph published by Mouton (The Hague). While the key concept at the heart of *Syntactic Structures*, the transformation, had been developed through the early-to-mid 1950s by Harris,

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[57] Newmeyer, *Linguistic Theory in America*, p 250.

[58] Robert A. Hall Jr, "American Linguistics, 1950–1960," in *American Linguistics 1925–1969: Three essays* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1976), p 47.

[59] Chomsky's M.A. thesis was published in 1979 under the same title, and his Ph.D. thesis as part of the 1975 release of *The Logical Structure of Linguistic Theory*.

[60] Accounts of Chomsky's life are plentiful, and the interested reader is directed to Barsky, *op. cit.* and McGilvray (ed.), *op. cit.*, among others.

and then by Chomsky, it did not become widely known before the late 1950s. The impact of transformational grammar on the academic scene was soon openly recognized by both supporters and denouncers of the theory: "no work has had a greater influence upon the development of current linguistic theory", wrote English linguist John Lyons in 1966; more dramatically, American linguist Archibald Hill described the effect of Chomsky's monograph on the American linguistics community as "much what the birth of Athena must have been on the Olympians".[61] In 1965, Chomsky presented a second version of transformational grammar in his *Aspects of the Theory of Syntax* (MIT Press), described in Ved Mehta's *New Yorker* article as the "New Testament" and by American linguist Frederick Newmeyer as the "Bible of our field".[62] Those less enthusiastic about transformational grammar called *Aspects* the "constitution of the revolutionary regime in power".[63] The following sections describe the technical content of these two presentations of transformational grammar, as well as their influence on the American linguistics scene.[64]

Transformational Grammar: Theory

As presented by Chomsky, transformational grammar stakes out a new program of inquiry for linguistics, and specifically for syntax. Chomsky rejects Bloomfieldian mechanical grammar building and anti-mentalism, introduces new aims for linguistic theory and an anti-behaviorist framework, and – most importantly for this study – develops a powerful syntactic tool: the transformation. Where the Descriptivists promoted an incremental study from phonology to morphology to syntax, Chomsky saw sentence-level phenomena as the core of grammatical study; where Bloomfield warned against appealing to unobservable entities, Chomsky saw mental capacities as essential to a full understanding of

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[61]Lyons, *Review of Chomsky*, p 393, Hill, *Promises and Limitations*, p 8.

[62]Ved Mehta, "John is Easy to Please," in Ved Mehta (ed.), *John is Easy to Please: Encounters with the written and the spoken word* (New York: Farrar, Straus and Giroux, 1971), p 175, Newmeyer, *Linguistic Theory in America*, p 92.

[63]Murray, *Theory Groups*, p 431.

[64]The theoretical discussion of transformational grammar presented in this chapter, as the theoretical discussions of stratificational and immediate constituency grammars, are designed to familiarize the reader with the theories in question; for fully detailed treatments of the grammars, the reader is referred to the original works.

language; where the Descriptivists valued data-collection, Chomsky advocated hypothesis testing and theory evaluation; and where the Bloomfieldians used immediate constituency theory to analyze syntactic constructions, Chomsky introduced a more powerful analytical tool. This linguistic program was enunciated gradually over a decade, and *Syntactic Structures* itself – the topic at hand – covered only the theoretical elements of this program.

For Chomsky, a *language* is a set of sentences, each finite in length, built out of a finite alphabet. A *grammar* is a device which generates sentences (that is, which generates a subset of a language). Given a language L, the goal of syntax is to construct grammars which generate all and only the grammatical (or, well formed) sentences of L. More broadly, Chomsky argues, linguists should be interested in "determining the fundamental underlying properties of successful grammars" – or, linguists should aim to develop universal syntactic theories which make no appeal to individual languages.[65] In later manifestations of transformational theory, these ideas would be explicitly underpinned by the belief that all human languages share common structural principles, and that these principles are a function of innate characteristics of the human brain.

It is important to distinguish Chomsky's conception of *grammars* and *linguistic theories*. While grammars are language-specific, linguistic theories concern all human languages. They define both a class of grammars for the space of potential natural languages, and an *evaluation procedure* for that class of grammars: given a language L, two grammars of L, and a corpus from L, the evaluation procedure should determine which grammar better captures the corpus, based on criteria discussed below. This conception of linguistic theory rejects the Bloomfieldian requirement that a linguistic theory be equipped with a discovery procedure for grammars (that is, a mechanical method for building a grammar based on a given corpus). It is "unreasonable" to expect linguistic theories to provide grammar-building procedures, Chomsky argued: such procedures are simply too complex to be realistically considered feasible.[66] "[B]y lowering our sights to the more modest goal of developing an evaluation procedure for grammars", he continued, "we can focus attention more clearly

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[65] Noam Chomsky, *Syntactic Structures* (The Hague: Mouton de Gruyter, 2002), p 11.
[66] *Idem*, p 52–53.

on really crucial problems of linguistic structure and we can arrive at more satisfying answers to them".[67]

Perhaps the most interesting aspect of *Syntactic Structures* is the identification of criteria for selecting one grammar over another within a given linguistic theory. Chomsky proposes two main criteria: first, grammars must generate *all and only* the grammatical sentences of the language in question (external adequacy) and, secondly, the fundamental units and structures of the grammar must be language-independent (generality, or universality). If two grammars satisfy the external adequacy and generality criteria equivalently, selection then depends on the relative "simplicity" of the grammars (internal adequacy).[68] A final adequacy condition imposed on grammars requires that all grammars provide an isomorphism between constructional homonymity and meaning ambiguity. This condition forces grammars to account for ambiguous syntactic constructions such as *old men and women* and *they are flying planes*, each of which has two possible meanings.[69] The implications of these criteria for the rise of transformational grammar are discussed in detail in Chapter 3.

At the core of *Syntactic Structures* is the determination of the linguistic theory best able to capture natural language sentence structure. Chomsky introduces three increasingly complex potential theories – finite state grammars, phrase structure grammars, and transformational grammars – and argues that, of these, only transformational grammars can adequately capture syntactic structure.[70] Phrase structure grammars (PSGs) are a formalization of immediate constituency grammars – that is, of the grammatical model used by Bloomfieldians in the 1940s and 1950s.[71] This formalization is a dual-structure grammar comprising a phrase structure (or syntactic) component and a morphophonemic component. The phrase structure component consists of initial symbols and rewrite rules which,

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[67]*Idem.*

[68]*Idem,* p 53.

[69]*Idem,* p 87.

[70]Finite state grammars have little bearing on this study and, as such, will not be discussed here. The reader is referred to *Idem* for a first-hand account, and to Lyons, *Noam Chomsky,* p 57ff for a second-hand account.

[71]Phrase structure grammars were not universally accepted as being accurate formalizations of immediate constituency grammars. This is discussed in detail in Chapter 3.

when repeatedly applied, can result in the derivation of a sentence. This can be represented graphically in terms of rewrite rules, or by a derivational tree, as shown in figure 2.2. A phrase structure grammar meets the condition of external adequacy if the set of sentences it generates are exactly the grammatical sentences of the language under investigation. The morphophonemic component then converts the output of derivations into strings of phonemes. A typical morphophonemic rule converts the infinitive form of a verb (for example, *take*) and the past tense morpheme (+*past*) into the past tense form of the verb (*took*), as shown in figure 2.3.

The key argument in *Syntactic Structures* – the argument which would come to define transformational grammar in the 1960s – is that phrase structure grammars are incapable of adequately capturing sentence structure. These grammars, Chomsky argues, are "extremely complex, *ad hoc*, and 'unrevealing'", and they are unable to "state many real generalizations and regularities or to account for many facts about English structure which are intuitively obvious to any native speaker".[72] Phrase structure grammars, he continues, fail to adequately account for syntactic constructions such as conjunctions (*John and Mary*), active-passive pairs (*John loves Mary* and *Mary is loved by John*), discontinuous constituents (*John picked up the book* and *John picked the book up*), and ambiguous sentences (*They are flying planes*). On this basis, Chomsky argues that a more powerful linguistic theory is needed. This theory is, of course, transformational grammar. "A great many of these difficulties [with phrase structure theory] can be eliminated", he concludes, "if we extend our concept of linguistic structure to include a new level of transformational analysis".[73]

Transformational grammars are tripartite grammars comprising a phrase structure component, a transformational component, and a morphophonemic component. The phrase structure and morphophonemic components are similar to those defined above for PSGS – the innovative aspect of TG is the introduction of intervening transformations. A transformation is a rule which operates on one or more input strings and converts them to an out-

[72] Chomsky, *Syntactic Structures*, p 34, Noam Chomsky, "A Transformational Approach to Syntax," in *Third Texas Conference on Problems of Linguistic Analysis in English, 9–12 May 1958* (Austin: The University of Texas, 1962), p 135.

[73] Chomsky, *A Transformational Approach*, p 135.

Sentence

The girl eats the apple.

Rewrite Rule Representation

$$
\begin{aligned}
S &\rightarrow NP + VP \\
NP &\rightarrow Det + N \\
VP &\rightarrow V + NP
\end{aligned}
$$

Derivational Tree Representation
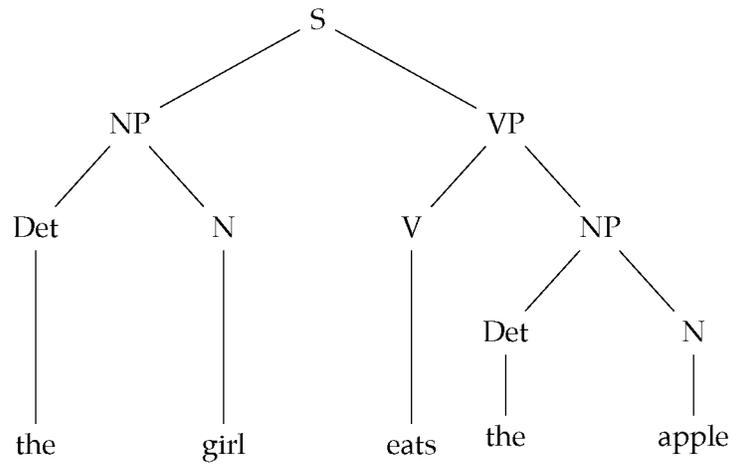
S Sentence
Det Determiner
N Noun
NP Noun Phrase
V Verb
VP Verb Phrase

Figure 2.2: Phrase structure component of the grammar: rewrite rules and derivational trees

$$\text{take} + [+\text{past}] \rightarrow /took/$$

Figure 2.3: Morphophonemic component of the grammar, adapted from *Syntactic Structures*, p 32.

put string with a new constituent structure. Transformations can add or delete morphemes, or rearrange the order of morphemes. For example, the canonical passive transformation T_{PASS} takes as input the active form of a sentence and generates as output the corresponding passive. This is shown below in simplified form (figure 2.4).[74] Here, the active sentence *John loves Mary* is generated by the phrase structure component of the grammar. To generate the corresponding passive, *Mary is loved by John*, the transformation T_{PASS} is applied. As demonstrated below, T_{PASS} rearranges the elements of the input string, adds the morpheme /by/, and modifies the form of the verb to provide a new constituent structure for the passive sentence. In a similar manner, TG generates negative sentences from positive ones (*John ate the apple* $\rightarrow T_{\text{NEG}} \rightarrow$ *John did not eat the apple*), interrogatives from declaratives (*John bought the book* $\rightarrow T_{\text{WHQ}} \rightarrow$ *what did John buy?* or *who bought the book?*), and nominalizations from noun-adjective constructions (*the boy is tall* $\rightarrow T_{\text{NOM}} \rightarrow$ *the tall boy*).[75]

Syntactic Structures defines two varieties of transformations: obligatory and optional. The set of sentences obtained by applying obligatory transformations to the output derivations of the phrase structure component is called the *kernel* of the grammar. Thus, every sentence in a language either belongs to the kernel or is derived from kernel sentences via one or more optional transformations. In the active-passive example, the active sentence is generated by phrase structure rules (PSRS) and hence belongs to the kernel, while the passive counterpart is derived from the kernel via an optional transformation. Importantly, there is no one-to-one relationship between kernel and non-kernel sentences; rather, a single kernel sentence can act as the basis for a number of transformations. For example, the kernel sentence *John bought a book at the university* can be transformed via the optional transformation T_{WHQ} (colloquially known as the WH-question transformation) into any of the

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[74]In the examples of transformational theory presented in this study, I leave out technicalities such as tense and auxiliaries, which are not essential for our purposes.

[75]A list of the transformations introduced in 1957 appears in Appendix II of Chomsky, *Syntactic Structures*.

Input:

```
                    S
            NP             VP
            |         V         NP
            N         |         |
            |       loves       N
          John                  |
                              Mary
```

Transformation: ⬇ T_PASS

Output:

```
                    S
            NP             VP
            |         V         PP
            N         |      P       NP
            |     is loved   |       |
          Mary              by       N
                                     |
                                    John
```

S Sentence
N Noun
NP Noun Phrase
P Preposition
PP Prepositional Phrase
V Verb
VP Verb Phrase
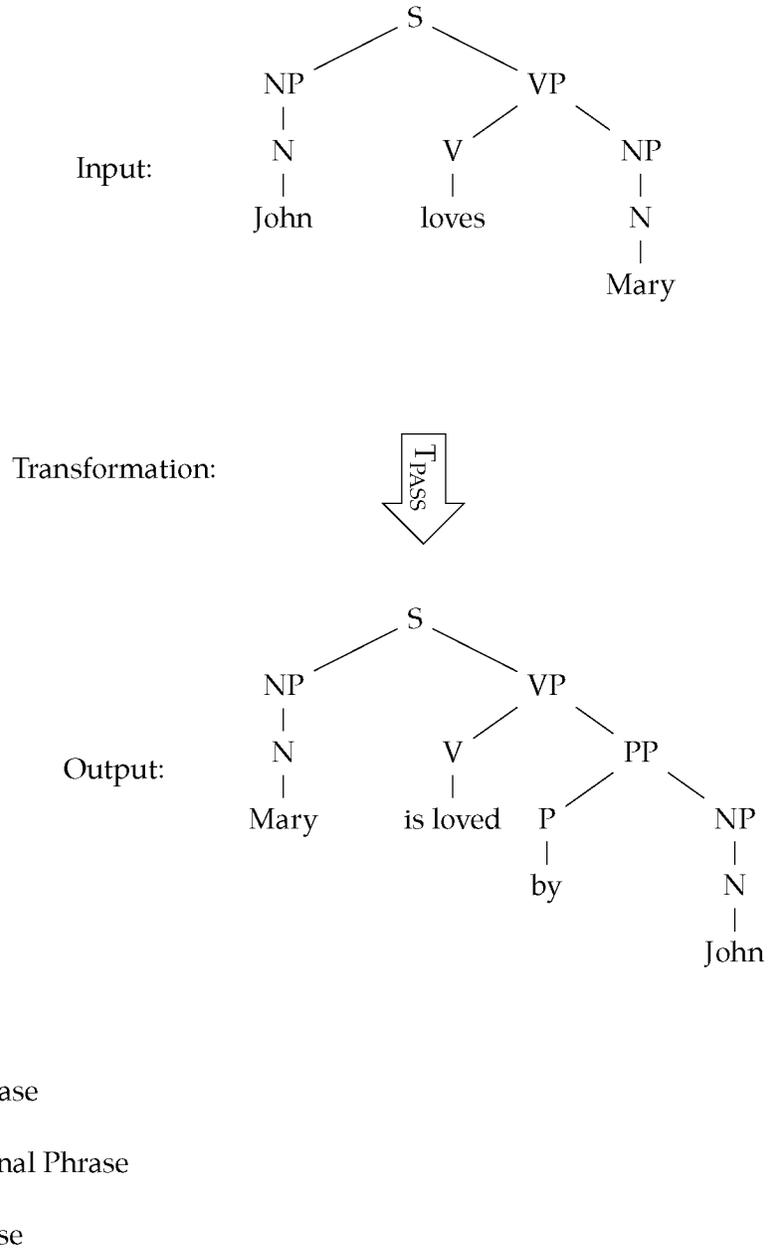
Figure 2.4: The passive transformation

sentences *What did John buy at the university?*, *Where did John buy a book?*, *Who bought a book at the university?*, and *Who bought what where?*. Clearly, the size of the kernel can be adjusted by varying the classification of transformations as obligatory or optional. The optimal kernel, Chomsky argued, is obtained when (1) all kernel sentences can be generated by simple phrase structure rules, and (2) every sentence in the language can be derived from a kernel sentence via optional transformations. The decision of whether to generate a particular structure in the kernel (that is, by phrase structure rules and obligatory transformations) or through optional transformations is based on simplicity: optional transformations are preferred when they can capture a large number of apparently independent structures. For example, actives are generated in the kernel and their corresponding passives are generated via an optional transformation since passivization is easily handled with a transformation, but is difficult to handle with phrase structure rules. Debate over the size and purpose of the kernel, and the relationship between kernel sentences and semantics, would become front-and-center in the late 1960s and 1970s.

The chief motivation for transformational rules, Chomsky argued through the 1960s, was their ability to capture problematic syntactic constructions. There are many types of sentences, he wrote, that "cannot be generated in a natural and economical way by a constituent-structure grammar but that are, nevertheless, related systematically to sentences of simpler structure. Transformations express these relations. When used to generate more complex sentences [...] from already generated simpler ones, transformations can account for aspects of grammatical structure that cannot be expressed by constituent-structure grammar".[76] Perhaps the paradigmatic example of systematic sentence relations is the active-passive construction. As described above, transformational theory links active-passive pairs through the transformational component: the passive counterpart is derived from the active sentence by means of the transformation T_{PASS}, establishing a relationship between the two sentences. This ability to capture what Chomsky termed "intuitively ap-

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[76]Noam Chomsky and George A. Miller, "Introduction to the Formal Analysis of Natural Languages," in R. Duncan Luce, Robert R. Bush, and Eugene Galanter (eds.), *Handbook of Mathematical Psychology* (New York: John Wiley and Sons, Inc., 1963), p 299–300.

parent" connections between sentences would be central to the success of transformational grammar in the 1960s.[77] This argument grew into the *raison d'être* of transformational theory, and was used repeatedly in articles, lectures, and textbooks to promote the theory. In his 1964 syntax textbook, for example, University of Texas at Austin linguist Emmon Bach argues that transformational grammar "overcomes many of the formal and empirical defects of the phrase-structure grammars" and that transformational theory is able to account for syntactic constructions which are treated by constituency theory in "enormously complicated [...] clumsy or artificial" manners.[78] Proponents of transformational grammar were careful to recognize that Chomsky had provided no proof of the inherent inability of phrase structure grammars to fully capture all the sentences of a natural language, but emphasized that any such grammar would be extremely complex – so complex, in fact, that it would be impossible to work with.

Together, these arguments were persuasive: by explicitly linking active sentences with their passive counterparts, declarative statements with their associated wh-questions, and nominal phrases with their associated nominalizations, transformational theory impressed linguists with its ability to handle what John Lyons has called the "deeper connexions" between sentences.[79] Even linguists who rejected the broad transformational program considered the technical capacities of the theory to be useful. Yale University's Paul Newman, for example, rejected the "philosophical and psychological claims" of Chomsky's program, but still considered the transformation to be the most practical syntactic tool for the description of previously unknown languages.[80] In his 1967–1968 study of Tera (a language of the Gombe area of Nigeria), Newman chose to work with transformations because they "permit[] the expression of significant generalizations hidden below the surface structure of the language".[81]

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[77]Chomsky, *A Transformational Approach*, p 135.

[78]Emmon Bach, *An Introduction to Transformational Grammars* (New York: Holt, Rinehart and Winston, Inc., 1964), p 65–67, 168.

[79]John Lyons, *Introduction to Theoretical Linguistics* (Cambridge: Cambridge University Printing House, 1969), p 247.

[80]Paul Newman, *A Grammar of Tera: Transformational syntax and texts* (Berkeley: University of California Press, 1970), p 4.

[81]*Idem.*

The classical version of transformational grammar described above persisted from its introduction in 1957 until 1965, when it was modified by Chomsky's second major work, *Aspects of the Theory of Syntax* (hereafter, *Aspects*).[82] *Aspects* introduces Chomsky's anti-behaviorist philosophy and his arguments for the innateness of language for which he is well known in philosophical, psychological, and public circles. This monograph is best understood as a collection of four themes: methodology, philosophy, grammar adequacy, and syntactic theory. We will discuss these in turn.

Aspects: Methodology

Aspects opens with a passage which has come to epitomise Chomsky's linguistics program: "Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogenous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, and shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance".[83] On this basis, Chomsky draws a distinction between *competence* (the native speaker-listener's internal, tacit knowledge of his language) and *performance* (the actual use of language in real situations).[84] Performance is affected by, among other factors, underlying competence, memory limitations, physical and mental disabilities, distractions, etc. The first key methodological point in *Aspects* states that linguists must study competence, not performance; or, that grammars should aim to capture "the ideal speaker-hearer's intrinsic competence".[85] Performance factors, Chomsky argues, are irrelevant to the task of constructing grammars, and vice versa – a belief which would be challenged by rival syntactic theories through the 1960s.

An immediate consequence of the competence-performance distinction is the deconflation of the concept of grammaticality. Under the *Aspects* rubric, a sentence from a

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[82] The central ideas in *Aspects of the Theory of Syntax* were presented orally before 1965, but first appeared in print in 1965.

[83] Noam Chomsky, *Aspects of the Theory of Syntax* (Cambridge MA: MIT Press, 1965), p 3.

[84] *Idem*, p 4. The competence-performance distinction was first introduced in Chomsky's 1962 address to the Ninth International Congress of Linguists.

[85] Chomsky, *Aspects*, p 4.

given language is said to be *acceptable* if it is "perfectly natural and immediately compre-
hensible without paper-and-pencil analysis, and in no way bizarre or outlandish".[86] This
concept belongs to performance. A sentence is said to be *grammatical* if it is permitted by
a native speaker-listener's competence (that is, if it is permitted in the absence of memory
restrictions, semantic content, physical state, and so on). For example, the sentence *I shot
the lion that loved the gazelle that loved the hyena that loved the warthog that loved the hippo that
loved the crocodile...* is a perfectly grammatical English sentence, but – if continued for a long
time – would not be considered acceptable, since it surpasses human memory limitations.
While the notions of competence and grammaticality clearly refer to mental capacities, it
is important to emphasize that transformational grammar does *not* attempt to model the
mental state of the speaker-listener; rather, it attempts to "characterize in the most neutral
possible terms the knowledge of the language that provides the basis for actual use of lan-
guage by a speaker-hearer".[87] This restriction would come to form a significant part of the
theory-choice debates of the 1960s (cf. Chapter 3).

Coupled with the desire to study competence, the belief that speaker-listeners are not
aware of their underlying linguistic competence creates methodological challenges for
transformational grammarians. "[N]o adequate formalizable techniques are known for ob-
taining reliable information concerning the facts of linguistic structure", Chomsky wrote
in 1965 – and hence there are "very few reliable experimental or data-processing proce-
dures for obtaining significant information concerning the linguistic intuition of the na-
tive speaker".[88] Chomsky proposed that linguists use both actual speech occurrences and
introspective reports to gather data. Through the 1960s, transformational grammarians
chose introspection as their main data-collection methodology. This is in stark contrast
with Bloomfieldian linguistics, which considered mental processes to be outside the realm
of linguistic science, and which obtained all data from corpora (in later years, corpora aug-
mented with specific elicited speech acts as required). In the Bloomfieldian worldview,

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[86] *Idem*, p 10.
[87] *Idem*, p 9.
[88] *Idem*, p 19.

linguists have no means of accessing or analyzing the "psychological correlates of facts", and hence "[t]he native speaker's feeling about sounds or about anything else is inaccessible to investigation by the techniques of linguistic science, and any appeal to it is a plain invasion of the linguist's proper function".[89] Transformational grammarians, in contrast, regularly judged the acceptability and unacceptability of sentences by appealing to their own intuition. The ubiquity of introspection among transformational grammarians, and the emphasis on utterances potentially producible by a native speaker, was a fundamental shift from the 1950s data methodology, and representative of both Chomsky's adherence to a mentalistic philosophy and the broader shift in the social sciences away from behaviorism.

Aspects: Philosophy

The aspects of *Aspects* which have had the most influence outside of linguistics are its philosophical commitments: the rejection of behaviorism and operationalism, the shift from discovery procedures to hypothesis testing, and commitment to the innateness of language knowledge. "The central fact to which any significant theory of language must address itself", Chomsky argued, is that "a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them. Most of our linguistic experience, both as speakers and hearers, is with new sentences; once we have mastered a language, the class of sentences with which we can operate fluidly and without difficulty or hesitation is so vast that for all practical purposes (and, obviously, for all theoretical purposes), we can regard it as infinite".[90] The philosophical framework of the second version of TG was designed precisely to account for language creativity and acquisition. In this context, the concept of universal grammar emerged centerstage: linguists should aim, Chomsky emphasized, not simply to develop grammars for individual languages, but to develop "a universal gram-

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[89]Bernard Bloch and George L. Trager, *Outline of Linguistic Analysis* (Baltimore: Linguistic Society of America, 1942).

[90]Noam Chomsky, "Current Issues in Linguistic Theory," in Jerry A. Fodor and Jerrold J. Katz (eds.), *The Structure of Language: Readings in the philosophy of language* (New Jersey: Prentice-Hall, Inc., 1964), p 50.

mar that accommodates the creative aspect of language use and expresses […] deep-seated regularities".[91] Such a universal grammar would necessarily be based on innate capacities and strategies for language. Children must be equipped with, first, "a linguistic theory that specifies the form of a grammar of a possible human language" and, second, "a strategy for selecting a grammar of the appropriate form that is compatible with the primary linguistic data".[92] Without this innateness assumption, Chomsky argued, it would be impossible to account for the uniformity, rapidity, and universality of language acquisition in the face of the degeneracy and incompleteness of the primary data available to children (this has become known as the *poverty of the stimulus argument*). This framework ties together linguistic competence, language acquisition and creativity, and linguistic universals. Language, Chomsky concluded, reflects not one's experience but one's innate structures – the core of the anti-behaviorist position which would come to be intimately linked to transformational theory.

Chomsky's philosophy is built explicitly in opposition to behaviorism, as manifested in mid-20th-century American psychology and, specifically, in Bloomfieldian linguistics. The behaviorist position, Chomsky argues in *Aspects* and his later *Cartesian Linguistics* (1966), assumes that children learn language as a result of conditioning and drill: children are endowed with a simple, non-language-specific device which provides "a preliminary analysis of experience" and, beyond this, language is learned "by application of the available inductive principles to this initially analyzed experience".[93] Under this framework, language is essentially entirely a function of the child's experience of observable input. This philosophical position, Chomsky argues, is fundamentally incapable of providing an adequate explanation of human language since it "fail[s] totally to come to grips with the 'creative' aspect of language use, that is, the ability to form and understand previously unheard sentences [and fails] to appreciate the degree of internal organization and the intricacy of the system of abstract structures that has been mastered by the learner, and that is brought to

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[91] Chomsky, *Aspects*, p 6.
[92] *Idem*, p 25.
[93] *Idem*, p 48.

bear in understanding, or even identifying utterances".[94] Behaviorism – and with it the Bloomfieldian linguistic program – cannot in principle, Chomsky concludes, account for the poverty of the stimulus. Instead, a mentalist position is needed: a position which recognizes that children are endowed with innate, language-specific structures which enable them to acquire language quickly and easily without overt teaching or study.

Chomsky's promotion of mentalism was part of a much broader movement in American social science which led, by the early 1960s, to the collapse of the behaviorist paradigm. Logical positivism and operationalism – briefly, the beliefs that statements which can not be empirically verified are meaningless, and that all scientific concepts should be defined via measurable operations – underpinned behaviorism in the early to mid 20th century. In American linguistics, these manifested themselves as, first, the methodological principle that only observable behavior (in the case of language, speech and writing) was permissible as data and, second, the theoretical assumption that general linguistic theories would emerge from detailed descriptions of data. Chomsky's rejection of mechanical grammar building, or linguistic discovery procedures, is implicitly a rejection of these two tenets: by introducing hypothesis testing and theory evaluation, and by opening the door to unobservable entities, Chomsky removed many of the constraints which had limited earlier linguistic inquiry and ushered American linguistics into the growing neo-empiricist framework. Chomsky entered these philosophical debates publicly in 1959 with the publication of his scathing review of B.F. Skinner's *Verbal Behavior* in *Language* – a review whose influence on the decline of behaviorism is widely recognized.[95]

The rejection of behaviorism within transformational grammar was central to attracting students to that theory in the 1960s – among them, philosopher Jerrold Katz. The failure of constituency grammars to handle "the full range of facts about linguistic structure", Katz

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[94]Chomsky, *Current Issues*, p 113.

[95]George Miller, "The Cognitive Revolution," *Trends in Cognitive Science* 7/3 (2003), Apostolos Georgopoulos, The Top 100 Works in Cognitive Science (Millennium Project, Center for Cognitive Sciences, University of Minnesota). The reader interested in the fall of empiricism and behaviorism in mid-20th-century philosophy of science more broadly is directed to Carl Hempel, *Philosophy of Natural Science* (Englewood Cliffs: Prentice Hall, 1966), David S. Palmero, "Is a Scientific Revolution Taking Place in Psychology?," *Science Studies* 1 (1971) and Miller, *Cognitive Revolution*.

wrote in a 1964 article in support of mentalist linguistics, "is due to the failure of such theories to concern themselves with mental capacities, and events, and processes".[96] By refusing to consider mental constructs, he continued, constituency grammars are "unable to handle many kinds of facts that [transformational grammars] handle easily and naturally".[97] For Katz, as for many others who went to work with Chomsky in the 1960s, the appeal of transformational grammar rested as much on its philosophical framework as on the technical apparatus of the theory. As Chomsky's anti-behaviorism gained prominence through the 1960s, and as the characterization of linguistic competence became central to transformational theory, the technical aspects of Chomsky's program became part and parcel of a larger set of beliefs about the human mind.[98]

Aspects: Grammar adequacy

Under the *Aspects* rubric, linguistic theories must be endowed with two properties: first, they must define a class of externally adequate grammars covering all natural languages (that is, grammars which generate structural descriptions for all and only the well-formed sentences of each language) and, second, they must contain a method of evaluating alternative grammars based on the primary linguistic data which would be available to a child.[99] That is, linguistic theories must be able to account for the innate predisposition of children to acquire language. Common linguistic features were seen to be "universal properties of language", explainable only in terms of innate capacities.[100] Further, linguistic theories must use simplicity considerations to evaluate alternative grammars. On the surface, this is an intuitive idea: given two externally adequate grammars for language, the simpler one is said to be superior. The devil, however, is in the details: transformational grammarians had a difficult time establishing a definition of simplicity, designing methods for measur-

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[96]Katz, *Mentalism in Linguistics*, p 127.

[97]*Idem.*

[98]The reader is directed to Noam Chomsky, *Cartesian Linguistics: A chapter in the history of rationalist thought* (New York: Harper & Row Publishers, 1966) and Noam Chomsky, "Language and Responsibility," in Noam Chomsky (ed.), *On Language* (New York: The New Press, 1977).

[99]To avoid confusion in Chapter 3, I am deliberately not using Chomsky's definitions of strong and weak generative capacity, and of descriptive and explanatory adequacy.

[100]Chomsky, *Aspects*, p 35.

ing the simplicity of grammars, and comparing simplicity results. As we will see, while the notion of simplicity was integral to *Aspects*, it proved particularly complex to implement and would become one of the weakest parts of transformational theory (cf. Chapter 3).

Aspects: Syntactic theory

Finally, *Aspects* made several technical modifications to transformational theory as presented in *Syntactic Structures*. The most important of these is the specification of *deep structure* and *surface structure*. The identification of deep and surface structures allows transformational theory to separate the grammar into three main components: the syntactic, semantic, and phonological. The syntax portion of the grammar (the phrase structure rules) first generates a structure, or phrase marker, for each sentence. With one caveat, this phrase marker is a deep structure. The deep structure then enters the semantic component of the grammar and receives a semantic interpretation (that is, the semantic component "relates the structure generated by the syntactic component to a certain semantic representation").[101] The deep structure is also mapped by transformational rules into a surface structure, which is given a phonetic interpretation by the phonological component of the grammar (that is, the phonological component "relates a structure generated by the syntactic component to a phonetically represented signal").[102] Critically, deep and surface structures are (usually) not identical (Figure 2.5). For example, the phrase marker *John ate the apple* would act as the deep structure for the surface structures *John ate the apple, Did John eat the apple?*, and *John did not eat the apple*, among others. The caveat mentioned above is that a phrase marker can only be a deep structure if it underlies some well-formed surface structure. In this way, the transformational rules act as a filter which permits only certain phrase markers to qualify as deep structures.

The other technical modifications presented in *Aspects* are less important for the purposes of this study. Briefly, phrase structure rules were forced to apply recursively, so that all embedding occurs in the phrase structure component. As such, the transformational

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[101]*Idem*, p 16.
[102]*Idem*.
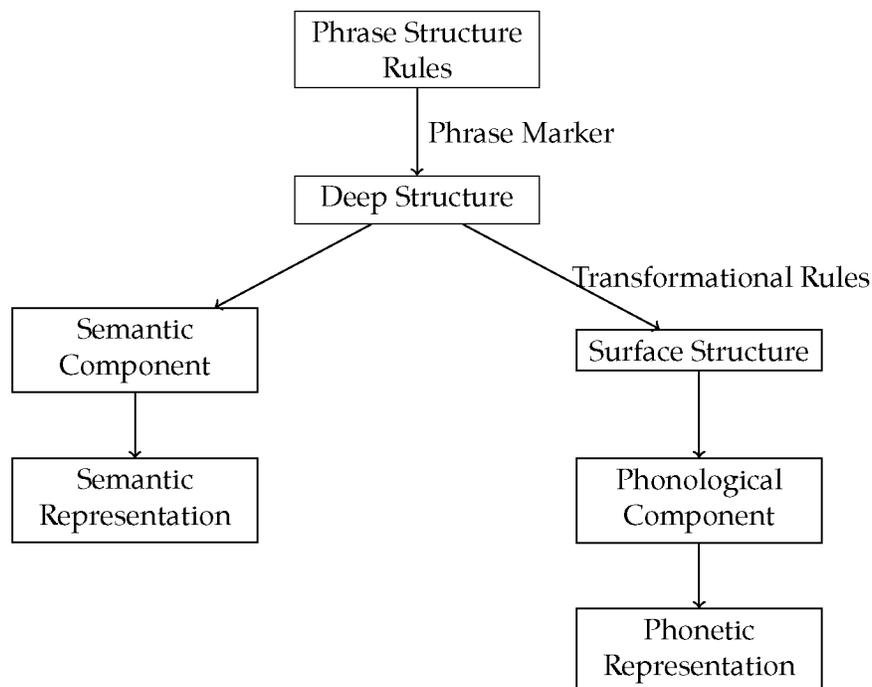
Figure 2.5: Relationship between deep structure, surface structure, and transformations in *Aspects of the Theory of Syntax*

component was restricted to singulary transformations (that is, transformations which take as input a single string). By restricting recursion to the phrase structure component, the transformational component becomes solely interpretive, guaranteeing semantic stability under transformational operations.

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There has been much debate in the literature about the extent to which Chomsky's linguistics program, as introduced between 1957 and 1965, represented a real break from what had come before. Transformational grammarians presented their program as "not a mere rephrasing or continuation of previous linguistic theories, but [...] a truly fresh and revolutionary approach to the study of language" – a view which was upheld by advocates of TG for decades.[103] The epithet of the era – the 'Chomskyan Revolution' – with its attendant atmosphere of abrupt, disruptive change, has been used in publications from *Language* to *The Nation* to *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, as well

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[103]Contreras, *op. cit.*, p 110.

as in popular histories of TG and linguistics textbooks. While the transformational grammar program certainly brought sweeping changes to American linguistics, its originality has traditionally been overstated. Recently, the historical record has been corrected by accounts which emphasize that the transformation, seen as a linguistic tool, originated not with Chomsky but with his teacher Zellig Harris, who introduced transformations in the context of discourse analysis in the early 1950s.[104] P.H. Matthews' work, for example, provides a detailed investigation of the relationship between Harris' and Chomsky's intellectual development, including the evolution of transformations through the 1950s.[105] Transformational grammar "was, indeed, far-reaching, but neither as sudden nor as thorough as commonly alleged", wrote Gleason in 1988, and "much of the old remained, covertly incorporated into the new".[106] As will be emphasized in the coming chapters, the depth of novelty ascribed to TG was due in large part to the representation by transformational grammarians of the Bloomfieldian linguistics program and, more broadly, the community of constituency grammarians, as out-dated and old-fashioned.

One area in which Chomsky's theory made a novel contribution was in its emphasis on theory and, in particular, on formalization, mathematization and notation (cf. Chapter 4). Proponents and opponents of transformational grammar agree that Chomsky's key contribution to linguistics was his "insistence upon formal theory in linguistic work", and that his mathematization of syntax was a "breakthrough of the first importance in linguistic science".[107] This included the elaboration of rewrite rule notation, the more fundamental insistence on a rule-based approach to linguistic science, the explicit mathematical description of finite state and phrase structure grammars, and the Chomsky hierarchy. While working linguists paid only minimal attention to the mathematical underpinnings of this work, the emphasis on formalization and notation had broad consequences for American linguistics. An entire generation of linguists was "bitten by the theory bug", Emmon Bach

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[104]Zellig S. Harris, "Introduction to Transformations," in *Papers in Structural and Transformational Linguistics* (Transformations and Discourse Analysis Papers 15: D. Reidel Publishing Company, 1956), Zellig S. Harris, "Co-occurrence and Transformation in Linguistic Structure," *Language* 33/3 (1957).

[105]Matthews, *Grammatical Theory*.

[106]Gleason Jr, *Theories in Conflict*, p 59.

[107]David H. Kelly, "Changing Emphases in the Study of Language," *The English Journal* 55/8 (1966).

recalls, and, indeed, in the years after the introduction of TG, the basis of an American doctoral dissertation in linguistics shifted from fieldwork to theory.[108] The enunciation of Chomsky's program set new standards for the research questions worth pursuing, the presentational structure of research results, and the entrance requirements for the profession.

Transformational Grammar: Development and influence

Transformational grammar was nurtured and developed at MIT's Research Laboratory of Electronics, where Chomsky arrived for his first academic job in 1955. As the successor to the War-time Radiation Laboratory, which was instrumental in the development of microwave radar, the Research Laboratory of Electronics enjoyed high prestige and funding through the 1950s and 1960s. In those decades, the Laboratory's work focused on microwave, physics, and electronics research, but the interdisciplinary nature of the Laboratory – also a vestige from the War – opened naturally to the study of acoustics, machine translation, computer science, and, of course, linguistics. "In the intellectual milieu of Cambridge", Chomsky recalls from his arrival, "there was a great impact of the remarkable technological developments associated with World War II. Computers, electronics, acoustics, mathematical theory of communication, cybernetics, all the technological approaches to human behavior enjoyed an extraordinary vogue. The human sciences were being reconstructed on the basis of these concepts. It was all connected".[109] It was in this stimulating and exciting environment that Chomsky would build his syntactic ideas into a theory which soon would dominate the American academic linguistics scene.

Historians of American academia have singled out the Research Laboratory of Electronics as unique in its capacity to stimulate interdisciplinary work and to permit members to focus on research over teaching.[110] Through the 1950s and 1960s, researchers at the Laboratory were free to follow their interests, whether they be basic or applied science, with the understanding that the military – the holder of the purse strings – could command the Lab-

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[108] Emmon Bach, Interview with Emmon Bach in London (UK), conducted by Janet Martin-Nielsen, 2008.

[109] Chomsky, *Language and Responsibility*, p 128.

[110] Geiger, *op. cit.*

oratory should its capabilities be required by domestic or international events. Chomsky and his colleagues recall with admiration and reverence the research climate they enjoyed in the post-World War II decades: transformational theory was developed in a "stimulating interdisciplinary environment" where the "atmosphere is uniquely conducive to discovery" and linguists were "free to do the work that interested [them]".[111] The Research Laboratory of Electronics, Chomsky continues, was endowed with a "general spirit of encouragement for innovation [which] made it possible for linguistics to flourish [...] in a way that for us at least would have been virtually out of the question elsewhere".[112]

This atmosphere persevered with the establishment of the MIT Department of Linguistics in 1961. Prior to that year, MIT doctoral students in linguistics were granted Ph.D.s in electrical engineering, since there existed no official linguistics department. "We were able to develop our program at MIT", Chomsky wrote in his 1977 *Language and Responsibility*, "because, in a sense, MIT was outside the American university system. There were no large departments of humanities or the related social sciences at MIT. Consequently, we could build up a linguistics department without coming up against problems of rivalry and academic bureaucracy. [...] That permitted us to develop a program very different from any other and quite independent".[113] In fact, MIT had established a School of Humanities and Social Studies in 1950 which, over the next six years, gained responsibility for economics, English, history, and international studies, among other subjects. It was under this school that Chomsky and his colleagues established a linguistics department in 1961. This came amidst a flurry of other new graduate programs in political science (1958), psychology (1960), and philosophy (1963). Chomsky's recollection of the lack of bureaucratic rivalry in face of the establishment of a linguistics department, then, speaks more to the carryover of the prestigious, stimulating, and research-oriented atmosphere of the Research Laboratory of Electronics than it does to the actual makeup of MIT at the time.

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[111]Noam Chomsky, "Introduction," in *The Logical Structure of Linguistic Theory* (New York: Plenum Press, 1975), p 2, John Robert Ross, *Infinite Syntax!* (New Jersey: Ablex Publishing Corporation, 1986), p xv, Chomsky, *Language and Responsibility*, p 132.

[112]Chomsky, *Language and Responsibility*, p 133.

[113]*Idem*, p 134.

The prestige of MIT and of the Research Laboratory of Electronics attracted a large group of strong graduate students to the linguistics program: in 1959, MIT's linguistics group consisted of only three members (Morris Halle, Roman Jakobson, and Chomsky), but by 1967 the MIT linguistics department employed more than 20 academic and research staff, as well as over 30 graduate students.[114] The graduate students who came to MIT to work on linguistics in the late 1950s and 1960s came from a wide variety of disciplines, bringing with them expertise in a range of natural and social sciences. Among them were Robert Lees (chemistry), Jerry Fodor and Jerrold Katz (philosophy, Princeton), James McCawley (mathematics, University of Chicago), and Barbara Hall Partee (mathematics, Swarthmore College). Approximately 30 graduate students received Ph.D.s in linguistics from MIT in the 1960s, working on languages ranging from West Scandinavian to Menomini to Sanskrit to Russian. This group was particularly motivated, intelligent, and enthusiastic about the possibilities offered by transformational grammar. They would be essential to the promotion of transformational theory and they would form the core of new linguistics departments across America (cf. Chapter 5).

In the American research climate, however, students – regardless of their strength – are not enough to ensure the success of a scientific theory. Funding is essential to such success and, in this area too, transformational grammar benefited from its association with the well-funded Research Laboratory of Electronics. From its inception, MIT linguistics was heavily supported by the American military and civilian government organizations, including the Army (Signal Corps), the Navy (Office of Naval Research), the Air Force (Office of Scientific Research and Operations Applications Laboratory, Air Research and Development Command), the National Science Foundation, the National Institutes of Health, and the Social Science Research Council. "Ever since the Second World War, the Defense Department has been a main channel for the support of the universities, because Congress and society as a whole have been unwilling to provide adequate public funds", Chomsky said in a 1971 *New York Times* interview, and "[l]uckily, Congress doesn't look too closely at the Defense

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[114]MIT Research Laboratory of Electronics, Massachusetts Institute of Technology, *Quarterly Progress Report*, (1967), p 273.

Department budget, and the Defense Department, which is a vast and complex organization, doesn't look too closely at the projects it supports – its right hand doesn't know what its left hand is doing".[115] For MIT linguists, this resulted in a windfall: through the 1960s, "more than half the MIT budget came from the Defense Department, but this funding at MIT is a book keeping trick", continued Chomsky: "[a]lthough I'm a full-time teacher, MIT pays only thirty to fifty percent of my salary. The rest comes from other sources – most from the Defense Department. But I get the money through MIT".[116] MIT graduate students also benefited from this generous military support – support which Frederick Newmeyer, Stephen Murray, James McCawley, and others credit with being instrumental to the success of transformational theory. Transformational grammar acquired its dominant position, McCawley argued, "faster than it deserved to" because of heavy military investment.[117] Critics of transformational grammar, including Konrad Koerner, argue that transformational grammarians used this funding for "proselytizing purposes", and that it gave them an unfair and unwarranted advantage over rival theories.[118]

As philosophical and psychological correlates of linguistic theory grew in importance through the 1960s, the research objectives of the MIT linguistics group shifted in parallel. In 1959, two years after the enunciation of transformational grammar, this group saw as its central task "the development of a generative theory of language. The theory will attempt to integrate all that is known about language and to reveal the lawful interactions among the structural properties of different languages as well as of the separate aspects of a given language, such as its syntax, morphology, and phonology. The search for linguistic universals and the development of a comprehensive typology of languages are primary research objectives".[119] After the elaboration of the second version of TG, with its emphasis on mentalism and language acquisition, these research objectives were expanded to include

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[115] Mehta, *op. cit.*, p 193.

[116] *Idem.*

[117] James D. McCawley (ed.), "¡Madison Avenue, Si, Pennsylvania Avenue, No!," in James D. McCawley (ed.), *Adverbs, Vowels, and Other Objects of Wonder* (Chicago: The University of Chicago Press, 1979), p 233.

[118] Koerner, *The 'Chomskyan Revolution'*, p 167.

[119] MIT Research Laboratory of Electronics, Massachusetts Institute of Technology, *Quarterly Progress Report*, (1959), p 186.

not only the development of a theory of generative grammar, but also the use of this theory as "a basis for the study of cognitive processes".[120] Finally, by 1971, the MIT linguistics group saw the study of language as an explicit "effort at mapping the mysteries of the human mind".[121] Soon after its introduction in *Syntactic Structures*, the transformation was no longer an autonomous tool for syntactic analysis, but was inextricably linked to a program which aimed to make fundamental discoveries about the human mind. Transformational grammarians extended their influence from linguistics to a variety of social science disciplines, and the theory was debated in psychological, philosophical, and cognitive science circles.

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Beyond the borders of MIT, transformational grammar quickly gained a reputation as it was transmitted via conferences, seminars, and underground literature. The theory was center stage at conferences from the Third Texas Conference on Problems of Linguistic Analysis in English (organized by Archibald Hill at the University of Texas at Austin in 1958) to the 1960 Symposium in Applied Mathematics hosted by the American Mathematical Society, and from the Ninth International Congress of Linguists (Cambridge, Massachusetts, 1962) to the annual Georgetown Round Table Meetings on Linguistics and Language Study. Transformational theory reached students from across America at the annual Linguistic Institutes, where Chomsky's 1964 presentation of deep and surface structures in Indiana drew particularly large crowds. These public presentations invariably provoked discussion about the new theory – discussion which, often times, resulted in convincing linguists of the merits of TG. Robert Stockwell, for example, entered the 1958 Texas Conference a firm believer in phonological syntax and exited, after lengthy debate over the "real subject matter of linguistics", firmly committed to transformational theory.[122] This theory transmis-

[120] Noam Chomsky and Morris Halle, "Research Objectives," *Research Laboratory of Electronics (Quarterly Progress Report, Massachusetts Institute of Technology)* 88 (1968), p 283–285.

[121] Morris Halle, "Research Objectives," *Research Laboratory of Electronics (Quarterly Progress Report, Massachusetts Institute of Technology)* 100 (1971), p 177–178.

[122] Archibald A. Hill (ed.), *Third Texas Conference on Problems of Linguistic Analysis in English, 9–12 May 1958* (Austin: The University of Texas, 1962), Robert P. Stockwell, "From English Philology to Linguistics and Back Again," in Konrad Koerner (ed.), *First Person Singular III: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1998).

sion consistently presented the transformation as the only technically adequate syntactic tool and mentalism as the only philosophically significant foundation for language study, and implied the inseparability of the technical from the philosophical.

Three early textual presentations stand out as particularly effective at gaining adherents to transformational grammar. In 1957, the year of publication of *Syntactic Structures*, Robert Lees released an essay review of the monograph in *Language*. Lees had begun his education in chemistry, but his studies were soon interrupted by the outbreak of World War II. When he resumed his university education in 1947, he chose to study linguistics, and in 1950 received his M.A. from the University of Chicago. Lees arrived at MIT in 1956 to work with Victor Yngve and the Research Laboratory of Electronics machine translation group, and soon thereafter began to work closely with Chomsky on TG. His 1959 Ph.D. dissertation, *The Grammar of English Nominalizations*, a study of noun-formation rules under the transformational rubric, was one of the first linguistics doctorates to be granted by MIT. Lees soon became one of the foremost advocates of transformational grammar and one of its most outspoken defenders. His career later led him to found the linguistics departments at the University of Illinois and at Tel Aviv University.

Syntactic Structures, Lees argued in 1957, was "one of the first serious attempts on the part of a linguist to construct within the tradition of scientific theory-construction a comprehensive theory of language which may be understood in the same sense that a chemical [or] biological theory is ordinarily understood by experts in those fields".[123] With this thesis, Lees sought to forge a direct link between scientific status and Chomsky's formalization of linguistics, to elevate linguistics from the social to the natural sciences, and to convince readers of *Language* of TG's novelty and credibility. Unlike older approaches to linguistics, he continued, transformational grammar was not "a mere reorganization of the data into a new kind of library catalog, nor another speculative philosophy about the nature of Man and Language", but rather "a rigorous explication of our intuitions about language in terms of an overt axiom system, the theorems derivable from it, explicit results which

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[123]Robert B. Lees, "Review of Noam Chomsky's Syntactic Structures," *Language* 33/3 (1957), p 377.

may be compared with new data and other intuitions, all based plainly on an overt theory of the internal structure of languages".[124] This emphasis on the scienticity of Chomsky's linguistics program came at a time when many American social scientists were struggling to demonstrate the rigor and objectivity of their work – a struggle which would, through the 1960s, determine government funding and academic status. Published in the premier American linguistics journal of the times, Lees' review was widely distributed and read. For many, it constituted their first introduction to transformational grammar and, with its forceful conclusions and implicit support from Bernard Bloch, then the editor of *Language*, the review was instrumental in bringing positive attention to Chomsky's newly published manuscript and, more broadly, to transformational grammar.

The simultaneous release of Jerrold Katz's *Mentalism in Linguistics* and Paul Postal's *Constituent Structure* in 1964 also added strong ammunition to the transformational program. Published in *Language*, Katz's article made the case for mentalist linguistics by arguing that any behaviourist linguistic theory would necessarily be incapable of accounting for communication and language acquisition. Katz – who had trained in philosophy at Princeton – gave a philosopher's backing to the Chomskyan argument that transformational grammar was inherently more powerful than immediate constituency grammar. Elegantly argued and concisely stated, Katz's article soon came to be the premier philosophical statement of support for TG. Postal's monograph similarly aimed to support transformational theory, but assumed a different angle of inquiry. Postal's argument is two-fold: first, he shows that several syntactic theories (including immediate constituency grammar and stratificational grammar) are equivalent to phrase structure grammar and, second, he argues that phrase structure theory is "inadequa[te] vis-à-vis the theory of transformational grammar".[125] *Constituent Structure* was, officially or unofficially, required reading at universities across America, and was celebrated by transformationalists as giving the final stamp of proof to Chomsky's arguments. Supporters of rival syntactic theories, however, denounced Postal

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[124]*Idem*, p 377–378.

[125]Paul M. Postal, *Constituent Structure: A study of contemporary models of syntactic description* (Bloomington: Indiana University, 1967), p v.

for misrepresenting those theories, and thus for incorrectly equating them with phrase structure grammar (cf. Chapter 3). Part of the effectiveness of Postal's manuscript was its wide distribution, which meant that, for many young linguists, it provided their only knowledge of syntactic theories such as stratificational grammar.

The signature of 1960s transformational grammar was the elaboration of technical details. "*Syntactic Structures* is programmatic", Gleason wrote in 1988, and "[i]t sorely needed to be supplemented by a full-scale, detailed treatment of a significant segment of the grammar of the language".[126] This treatment was first provided by Lees' 1959 doctoral dissertation, which studied the creation of new nouns. Lees proposed that nominalizations such as *that cold cup of coffee you left there* are transformations of base sentences such as *you left that cold cup of coffee there*.[127] Lees' study represents the first large-scale transformational analysis of a linguistic construction, and is often pointed to as supplying the hard data missing in *Syntactic Structures*.[128] Such data was considered essential to defending transformational grammar, since it "meant that opponents of the theory had the burden of responding to (and finding alternatives to) highly detailed analyses of many central syntactic phenomenon in English".[129] With Lees' work, TG had planted a stake – an analysis of empirical language data which would have to be "refined or rejected by anyone who would further our understanding of the phenomena involved" regardless of their theoretical commitments.[130] The lasting importance of this work is clear from ongoing demand: it was reprinted five times between 1960 and 1968.

A plethora of technical studies followed Lees' work. German word order, indirect objects, pronouns, relative clauses, and elementary transformations were all investigated between 1960 and 1964.[131] Together, these works provided detailed transformational analyses

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[126]Gleason Jr, *Theories in Conflict*, p 61.

[127]Robert B. Lees, *The Grammar of English Nominalizations*, 5th edition (Bloomington: Indiana University, 1968).

[128]Jerrold M. Sadock and Anthony L. Vanek (eds.), *Studies Presented to Robert B. Lees by his Students* (Edmonton: Linguistic Research, Inc., 1970), Newmeyer, *Linguistic Theory in America*.

[129]Newmeyer, *Linguistic Theory in America*, p 45.

[130]Sadock and Vanek (eds.), *op. cit.*, p xi–xii.

[131]Emmon Bach, "The Order of Elements in a Transformational Grammar of German," *Language* 38 (1962), Robert B. Lees, "The Promise of Transformational Grammar," *The English Journal* 52/5 (1963), Carlota S. Smith, "Determiners and Relative Clauses in a Generative Grammar of English," *Language* 40/1 (1964), Zellig S.

of problematic syntactic constructions in English and other languages, supplying TG with a large analytic database. The key technical works capping off this period are Katz and Fodor's 1963 *The Structure of a Semantic Theory (Language)* and Katz and Postal's 1964 *An Integrated Theory of Linguistic Descriptions* (MIT Press).[132] Both works attempt to integrate semantics into transformational grammar. Ex-Princeton philosophers Jerrold Katz and Jerry Fodor provided the first transformational study of semantics and, while their work was not widely accepted, it brought attention to the lack of semantic analysis in Chomsky's writings. Shortly thereafter, Katz and Postal proposed that singulary transformational rules should have no semantic effect, or, that transformations should be meaning-preserving – a view which became known as the Katz-Postal hypothesis. These early semantic studies instigated wide-spread work on the relationship between transformational grammar and the study of meaning, and the debates which sprang from them set in motion the Semantics Wars which would consume American academic linguistics from the late 1960s to the early 1970s. This aspect of the history of American linguistics is outside of the scope of this study; the reader is directed to John Goldsmith and Geoffrey Huck's *Ideology and Linguistic Theory: Noam Chomsky and the deep structure debates* (Routledge, 1995) for more detail.

In addition to technical studies, the 1960s also saw the development of a large number of partial transformational grammars – that is, transformational descriptions of the syntax of individual languages (or portions thereof). Postal's 1962 Yale dissertation, *Some Syntactic Rules in Mohawk* (published in 1979 by Garland), for example, provides a transformational analysis of Mohawk syntax. Similar work was done for Modern Standard Arabic (Frank Anshen and Peter A. Schreiber, 1968), Spanish (William W. Cressey, 1968), Samoan (Ross Clark, 1969), Tera (Paul Newman, 1967–1968), and many other languages. Together, technical studies and partial grammars proved the applicability of transformational theory to the analysis of problematic syntactic constructions, and showed that the theory could account broadly for the syntax of a variety of languages.

Harris, "The Elementary Transformations," in *Papers in Structural and Transformational Linguistics* (Dordrecht: D. Reidel Publishing Company, 1964).

[132] Jerrold J. Katz and Jerry A. Fodor, "The Structure of a Semantic Theory," *Language* 39/2 (1963), Jerrold J. Katz and Paul M. Postal, *An Integrated Theory of Linguistic Descriptions* (Cambridge MA: The MIT Press, 1964).

Finally, transformational grammar arrived on the university textbook scene in 1964 with the publication of Emmon Bach's *An Introduction to Transformational Grammars* (Holt, Rinehart, and Winston, Inc.). Initially written for a course at the University of Texas at Austin, Bach's book soon became a common teaching tool at universities across America. It provided professors with a standardized and sanitized version of TG appropriate for both undergraduate and graduate education, complete with problem sets and exercises. The ability of transformational grammar to capture the pedagogical market – made possible by the many TG-oriented textbooks of the 1960s – was central to the success of that theory over rivals (cf. Chapter 4).

By 1965, transformational grammar was the most common approach to syntax in American academic linguistics. Prior to that year, authors of journal articles on TG were regularly compelled to explain and/or defend the transformational approach, including in articles whose primary purpose was the description of the syntax of a relatively unknown language in transformational terms.[133] By 1965, however, TG was "widely accepted as valid", and journal articles no longer provided explanatory overviews or justified the use of transformations.[134] Post-1965 journal articles also shifted the debate from the validity of the philosophical underpinnings of TG to technical applications of the transformation-as-tool. These journal trends show that, by the mid-to-late 1960s, transformational grammar was seen by a large majority of the American linguistics community as the correct technical and philosophical approach to syntax, and that the crux of debate had shifted to internal technical problems with the theory. As English linguist John Lyons noted in 1970, "[r]ight or wrong, Chomsky's theory of grammar is undoubtedly the most dynamic and influential; and no linguist who wishes to keep abreast of current developments in his subject can

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[133]Werner Winter, "Transforms without Kernels?," *Language* 41/3 (1965).

[134]Mary L. Foster, "Review of Joseph E. Grimes' Huichol Syntax," *International Journal of American Linguistics* 32/3 (1966), John J. Quinn, "An Additional Subordinate Clause Transformation," *Journal of English Linguistics* 1 (1967), Ross Clark, "Some Verbless Sentences in Samoan," *Oceanic Linguistics* 8/2 (1969), Robert M.W. Dixon, "Relative Clauses and Possessive Phrases in Two Australian Languages," *Language* 45/1 (1969), Jeffrey S. Gruber, "Look and See," *Language* 43/4 (1967), Emmon Bach, "Have and Be in English Syntax," *Language* 43/2 (1967), Paul C. Doherty and Arthur Schwartz, "The Syntax of the Compared Adjective in English," *Language* 43/4 (1967), William W. Cressey, "Relative Adverbs in Spanish: A transformational analysis," *Language* 44/3 (1968), John C. McKay, "Some Generative Rules for German Time Adverbials," *Language* 44 (1968), Winter, *op. cit.*.

afford to ignore Chomsky's theoretical pronouncements".[135]

2.3 Stratificational Grammar

> The stratificational baby is here. Full term, red-faced, and squalling. It gives every indication of growing into as Tartarish a Turk as anyone could wish.
>
> John Algeo, in *Linguistics: Where do we go from here?* (1969)[136]

> [Stratificational grammar] is superior to any theory that cannot encode, cannot decode, or that claims isomorphism between the two; it is superior to any theory without a workable simplicity metric, or to one that is not provably consistent; and it is superior to all theories in which linguistic categories (agent, subject, noun phrase) are undefined primitives.
>
> William Sullivan, in *Syntax and Linguistic Semantics in Stratificational Theory* (1980)[137]

The most successful rival syntactic theory to transformational grammar in the 1960s was stratificational grammar (SG), whose development has been spearheaded over the past half-century by American linguist Sydney Lamb. The premise of stratificational theory is three-fold: it maintains that linguistic structure is comprised uniquely of a network of relation-ships; that distinct levels (or strata) can be identified within this network; and that linguistic theory ought to explain both the production and comprehension of language. While strat-ificational grammar never achieved the prominence of TG, during the 1960s and 1970s it enjoyed a moderate success in the United States and in Canada, and was one of the few vi-able rivals to transformational theory. "The importance of Lamb's work", Charles Hockett stated in his 1964 Presidential Address to the Linguistic Society of America, is that "it is the only currently active line of research reasonably independent of Chomsky".[138] As we

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[135] Lyons, *Noam Chomsky*, p 1–2.

[136] John Algeo, "Linguistics: Where do we go from here?," *The English Journal* 58/1 (1969), p 111.

[137] William J. Sullivan, "Syntax and Linguistic Semantics in Stratificational Theory," in Edith A. Moravcsik and Jessica R. Wirth (eds.), *Current Approaches to Syntax* (New York: Academic Press, 1980), p 326.

[138] Charles F. Hockett, "Sound Change," *Language* 41/2 (1965), p 197.

will see, stratificationalists developed their own technical and philosophical approach to linguistics – an approach which challenged the premises of transformational grammar and which offered an alternative to linguists unhappy with the Chomskyan program.

Born in 1929 in Colorado, Lamb pursued peripatetic undergraduate studies at Yale, first in a pre-medical program, then switching to mathematics, and finally settling on economics. His undergraduate years coincided with the Korean War and with the threat of the American draft. Seniors at Yale were deferred from the draft until graduation but, Lamb recalls, "the outlook beyond that day was not inviting".[139] Along with other students, Lamb was encouraged to study Russian in his final semester – an act designed to guide Yale graduates into Army intelligence instead of the infantry. This was his first introduction to linguistics, a field he had "never heard of until that semester".[140] "What I liked most about studying foreign languages", Lamb wrote in a 1998 autobiographical piece, was that "it seemed to provide pertinent information for one of my favorite problems in philosophy – how the problem of thinking works".[141] This problem would come to be central to Lamb's work as a linguist over the following half-century. As he prepared to graduate in 1951, Congress passed a bill deferring seniors who intended to continue to graduate school from the Korean War draft. Seeing graduate education as "a more exciting prospect than that of going into combat in Korea", Lamb chose to enroll in linguistics.[142] "At the time, I knew nothing about what the practitioners of linguistics actually did", he remembers, "but given the alternatives I was willing to take a chance".[143] From 1951 to 1958 Lamb studied at Berkeley – the only school where he had not missed the application deadline. For his doctoral dissertation, he analyzed Monachi (an Uto-Aztecan language spoken in the region south of Yosemite National Park) as part of Mary Haas' Survey of California Indian Languages. It was this work, combined with the teaching of Berkeley linguist Frank Whit-

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[139]Sydney M. Lamb, "Linguistics to the Beat of a Different Drummer," in Konrad Koerner (ed.), *First Person Singular III: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1998), p 105.
[140]*Idem.*
[141]*Idem.*
[142]*Idem*, p 106.
[143]*Idem.*

field, which led Lamb to the fundamental idea of stratificational grammar: the existence of multiple strata within linguistic structure. Stratificational theory was also motivated by Lamb's work on Russian-to-English machine translation, which he conducted at the Berkeley Computer Center from 1956 to 1964. Lamb left Berkeley in 1964 and returned to his alma mater, having been offered a position by Bernard Bloch. He worked at Yale for the remainder of the decade, nurturing, developing, and promoting stratificational grammar.

Stratificational grammar follows not in the tradition of American linguistics, but of European thought: its theoretical apparatus is motivated by the work of Danish linguist Louis Hjelmslev (1899–1965). The father of glossematics and a leader of the Copenhagen School of Linguistics, Hjelmslev's work is not widely known in America.[144] Lamb was exposed to his ideas while at Berkeley by Frank Whitfield, who in 1953 had translated Hjelmslev's *Prolegomena to a Theory of Language* into English. Lamb described Hjelmslev, who he met in Indiana in 1964, as "a brilliant and farsighted scholar, whose [...] contributions to linguistic theory, years after their publication, have yet to be fully appreciated by the commonwealth of linguists".[145] Lamb was attracted to Hjelmslev's conception of language as "nothing but a *system of relationships*" – the kernel which, combined with Lamb's notational innovations, would grow into stratificational grammar.[146]

Stratificational Grammar: Theory

In the second half of the 20th century, the label *stratificational grammar* has been applied to a variety of linguistic theories which share a set of theoretical assumptions but which differ in detail. Here, we restrict our discussion to the primary version of SG used in the 1960s: the work led by Sydney Lamb, which has its classical enunciation in his 1966 *Outline of Stratificational Grammar* (Georgetown University Press). For an outline of other versions

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[144]The interested reader is directed to Louis Hjelmslev, *Prolegomena to a Theory of Language* (Baltimore: Indiana University Publications, 1961) and Francis J. Whitfield, "Louis Hjelmslev," *Language* 42/3 (1966).

[145]Sydney M. Lamb, "Epilegomena to a Theory of Language," *Romance Philology* 19/4 (1966), p 531.

[146]Herman Parret, "Interview with Sydney M. Lamb (conducted in New Haven, CT, on 6 November 1972)," in *Discussing Language: Dialogues with Wallace L. Chafe, Noam Chomsky, Algirdas J. Greimas, M.A.K. Halliday, Peter Hartmann, George Lakoff, Sydney M. Lamb, Andre Martinet, James McCawley, Sebastian K. Saumjan, and Jacques Bouveresse* (The Hague: Mouton, 1974), p 179, emphasis in original.

of SG, the reader is directed to Ilah Fleming's 1969 review article, *Stratificational Theory: An annotated bibliography*, published in the *Journal of English Linguistics*.[147]

Perhaps the clearest and most succinct explanation of 1960s stratificational grammar is given in Peter Reich's 1968 report to the Yale Linguistic Automation Project. Reich explains stratificational theory by contrasting it with transformational theory: "[I]n producing a particular utterance [in TG], one applies, or attempts to apply, rules to the string one is building up one at a time, in linear sequence. After applying all of the rules, or perhaps a subset of them, one may have to reapply the rules or a subset of them again and again until no further changes can be made to the string. If at the end of this process the string consists solely of terminal symbols, one has produced a grammatical utterance. [...] In sharp contrast with this approach is the theory developed by Lamb, in which the system underlying natural language behaviour is formalized as a network of logical elements, or relationships, which communicate with one another using a small set of discrete symbols. There are no rules in a stratificational grammar, nor are there symbols in the usual sense of the term. One can, of course, describe the network of relationships in terms of a set of formulas consisting of symbols, which stand for lines in the network, and operators, which stand for nodes in the network. In fact we do this in order to input networks to the computer. However the basic form is the network form. One insight of Lamb's formulation is that the use of symbols and rules specifying operations on these symbols is not necessary to the description of the system underlying natural language data. This insight is important, because it brings us a small step closer to understanding how the system underlying language might be stored and used in the brain".[148]

As Reich indicates, the first tenet of stratificational grammar states that language consists of a *system of relationships*. This tenet underlies the most innovative theoretical assumption of stratificational theory: that linguistic structure (or, more accurately, the network of relationships which makes up linguistic structure) *contains no items at all*. Rather, stratification-

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[147]Ilah Fleming, "Stratificational Theory: An annotated bibliography," *Journal of English Linguistics* 3 (1969).
[148]Peter A. Reich, "Symbols, Relations, and Structural Complexity," in Adam Makkai and David G. Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973), p 92–93.

alists argue, "if all the information pertaining to a morpheme (likewise any other linguistic unit) is accounted for by network connections to all of the components of that information, then the symbol that was being used to represent that morpheme becomes redundant. It can be erased with no loss of information. Where that symbol was, before being erased, there is just a point of the network, connecting to all of that information."[149] Hence traditional linguistic items (or elements) such as phonemes, morphemes, and clauses are considered in sg to have no external existence beyond the identification of a unique network structure. "We are able to distinguish any linguistic element from any other linguistic element by virtue of the different connections it has", Lamb explained, "and therefore we have represented what it is not by virtue of what it consists of, but by what it is connected to".[150] Take, for example, the traditional morpheme *dog*. "To characterize [dog]", Lamb explained in a 1972 interview, "we can give a complete list of its properties as a morpheme of the language. In the first place it has a phonological form which we can represent as *d* followed by *o* followed by *g*. In the second place it has a certain grammatical function which can be summarized as 'noun'. In the third place it has a certain meaning or several meanings. These meanings we will characterize by connecting *dog* to one or more concepts (and for the moment we leave aside the question of the nature of a concept). We connect it to whatever concepts it needs to be connected to, within the conceptual structure. We can indicate its grammatical properties by connecting this element *dog* to the noun position within the syntax. [...] Now I have completely characterized that morpheme in terms of connections which it has to these various locations, conceptual, grammatical and expression. This totality of the properties *dog* is represented by those connections – therefore *dog* as an element has no existence in addition to that. There would be no information added by having a symbol or a label 'dog'; in other words *dog* emerges as a line or a node which has connections to specific points in the network. In drawing a network it is of practical value to put a label next to the line; it makes it easier to read, but we recognize that those

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[149]Sydney M. Lamb, "Translation and the Structure of Language," in W. John Hutchins (ed.), *Early Years in Machine Translation: Memories and biographies of pioneers* (Amsterdam: John Benjamins Publishing Company, 2000).

[150]Parret, *Interview with Lamb*, p 198.

labels are not a part of the structure".[151] In stratificational grammar, linguistic items are used simply as identifiers or labels to ease our ability to discuss and manipulate language structure; it is only the network which has real theoretical significance.

The second tenet of stratificational grammar states that a set of structural levels, or *strata*, can be identified within the system of relationships which underlies language – hence the name of the theory. Strata correspond very roughly to traditional linguistic levels (that is, phonological, morphological, syntactic, and semantic levels). Each stratum plays a role in determining the sentences which belong in a language. One of the central ways in which stratificational theories can differ is in terms of the number of strata they ascribe to language. Here, we focus on the four-strata model used by, among others, Lamb and David Lockwood.[152] Extensions to higher-strata versions of SG, including the more common six-strata variation, are straightforward. (The correct number of strata in linguistic structure, stratificationalists argue, can be determined "only by empirical investigation", and is not necessarily constant across all human languages.[153]) In the four-strata version, the strata are the phonemic, morphemic, lexemic, and sememic. Each stratum consists of an inventory of EMEs and its tactics. In the four-strata version, the relevant EMEs are phonemes, morphemes, lexemes, and sememes; or, as some stratificationalists refer to them, P-EMEs, M-EMEs, L-EMEs, and S-EMEs. The tactics of a stratum specifies how the EMEs of that stratum can combine with each other on that stratum. That is, on each stratum, the tactics specifies permissible (or, grammatical) combinations of EMEs: the semotactics (the tactics of the sememic stratum) specifies "the (infinite) set of well-formed sememic networks for a language", and the lexotactics specifies the (infinite) set of grammatical and sensical sentences of the language (that is, the lexotactics is concerned with "syntactic behavior and the various [allowed] syntactic constructions").[154] The morphotactics specifies the well-

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[151]*Idem*, p 196ff.

[152]Sydney M. Lamb, "The Sememic Approach to Structural Semantics," *American Anthropologist* 66/3 (1964), David G. Lockwood, *Introduction to Stratificational Linguistics* (New York: Harcourt Brace Jovanovich, Inc., 1972).

[153]Sydney M. Lamb, *Outline of Stratificational Grammar* (Washington DC: Georgetown University Press, 1966), p 1.

[154]Sydney M. Lamb, "Stratificational Linguistics as a Basis for Machine Translation," in Adam Makkai and David G. Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973), p 51, David G. Lockwood, "The Problem of Inflectional Morphemes," in Adam Makkai and David G.

formed morphological realizations of the language (that is, the morphological realizations of the grammatical and sensical sentences of the language), and the phonotactics specifies the well-formed phonemes of the language. Combinations of EMEs always exist on the same stratum as the EMEs themselves. For example, on the lexemic stratum we have both lexemes (for example, *dog, wood, dogwood, ...*) and permissible combinations of lexemes (that is, phrases). It is important to emphasize that EMEs are not linguistic units; rather, they are "points in the total network of relationships, and their labels are assigned as a matter of convenience, having no status in the theory whatsoever".[155] The labels chosen to represent the EMEs, whether familiar words or representational symbols, have no bearing on the content of the theory, but "do contribute to its readability [and are] reference points to aid the linguist in discussing this system".[156]

Stratificational grammar is commonly associated – both positively and negatively – with Lamb's network diagrams, which use a combination of lines and nodes to depict tactic structure and inter-stratal relationships. There are eight types of nodes based on the primitive pairings AND/OR, ORDERED/UNORDERED, and UPWARD/DOWNWARD (where upward refers to the directionality towards higher strata, or, towards meaning, and downward refers to the directionality towards lower strata, or, towards sound). These eight nodes are shown in figure 2.6; their functionality is explained in later examples. These nodes – or, more accurately, the relationships described by these nodes – are the only primitives in stratificational grammar, and "[a]ll linguistic functives found in the model – subject, object, clause, agent, etc. – whatever their importance, are derived from these relationships".[157] We now turn to examples.

Given a language L, the stratificational representation of a sentence S from L involves all four strata. On each individual stratum, S is represented by a set of EMEs ordered by the tactics of the stratum. The diagrams below demonstrate the phonemic, morphemic, and

Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973), p 197.

[155] Lockwood, *Stratificational Linguistics*, p 26.
[156] *Idem.*
[157] Sullivan, *Stratificational Theory*, p 304.

UPWARDS UNORDERED AND		UPWARDS ORDERED AND	
DOWNWARDS UNORDERED AND		DOWNWARDS ORDERED AND	
DOWNWARDS UNORDERED OR		DOWNWARDS ORDERED OR	
UPWARDS UNORDERED OR		UPWARDS ORDERED OR	
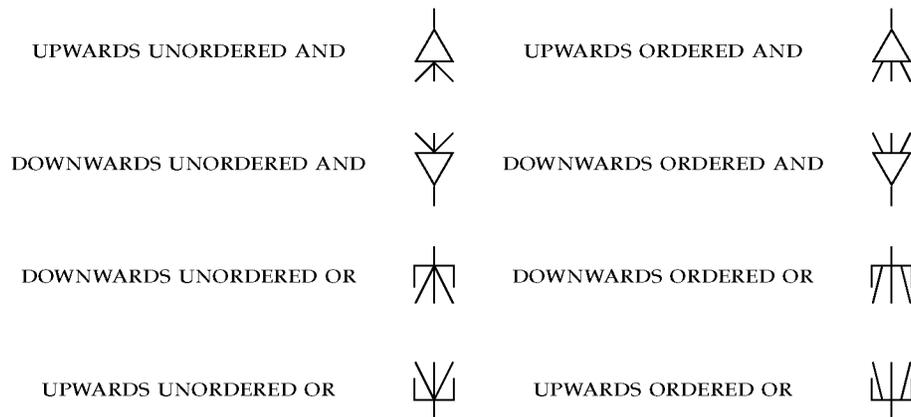
Figure 2.6: Stratificational grammar: nodes

lexemic strata (figures 2.7, 2.8, 2.9). The sememic stratum, not shown here, represents the semantic content of language constructions. On the phonemic stratum, the phonotactics identifies a subset of the well-formed phonemes of English by specifying permitted feature combinations. Here, the nodes immediately below the phonemes are UNORDERED–AND nodes, since each phoneme is produced by the simultaneous pronunciation of its composing features. The morphotactics specifies well-formed morphemes, including what are colloquially called 'words' as well as grammaticality features such as tense and plurality markers. In figure 2.8, the ORDERED–AND nodes immediately below *dog* and *boy* ensure that the composing phonemes are properly ordered. Finally, the lexotactics specifies well-formed sentences. The simplified model of the English declarative clause in figure 2.9 specifies sentences of the form NOUN PHRASE–VERB–NOUN PHRASE and NOUN PHRASE–COPULA–ADJECTIVE, with optionality provided by the uppermost UNORDERED–OR node. Thus the sentences *John read the book*, *Mary is dirty*, and *the book is red* are all specified by figure 2.9.

The final component of stratificational grammar – the realizational portion – links adjacent strata.[158] Realizational rules attach "to every well-formed structure on some stratum one or more accompanying structures on the adjacent levels" in such a way that units of neighboring strata are not in a simple one-to-one relationship.[159] Formally, realiza-

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[158] In early versions of stratificational grammar, this was called the *representational portion*.

[159] Alexander T. Borgida, "Formal Aspects of Stratificational Theory," in Michael Paradis (ed.), *Fourth LACUS Forum* (South Carolina: Hornbeam Press Inc., 1977), p 391.

b   d   p   t   m

w   △   △   △   △   △   n

Labial   Voiced   Closed   Apical   Nasal
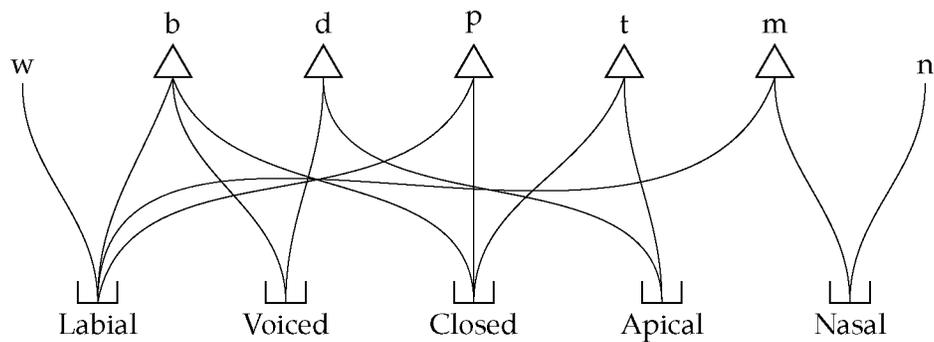
Figure 2.7: Phonotactics (adapted from Lamb 1971)

boy   dog

-d   -s

△   △

o   b   g   d   y   s
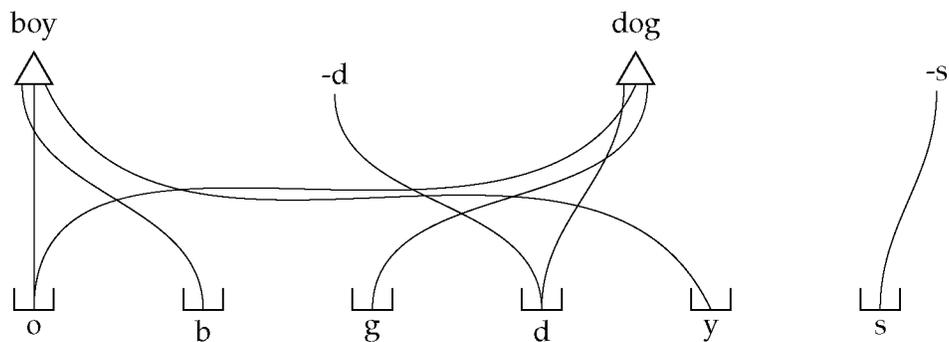
Note: -d is the past tense marker, -s is the plurality marker.

Figure 2.8: Morphotactics (adapted from Lamb 1971)

tional rules consist of a realizate (an element of a given stratum *S*) and a set of subrules, each of which specifies an environment of *S*-stratum elements and the realization (on a neighboring stratum) of the realizate in that environment. The standard stratificational model includes eight types of realization: simple realization, diversification, neutralization, composite realization, portmanteau realization, zero realization, empty realization, and anataxis. Neutralization, for example, describes the situation in which a single element on a stratum can be realized by several different elements on the next higher stratum. Figure 2.10 demonstrates neutralization at the lexemic-sememic stratal boundary: the string *old men and women* on the lexemic stratum can be realized either as *women and old men* or as *old men and old women* on the sememic stratum. Composite realization occurs when two
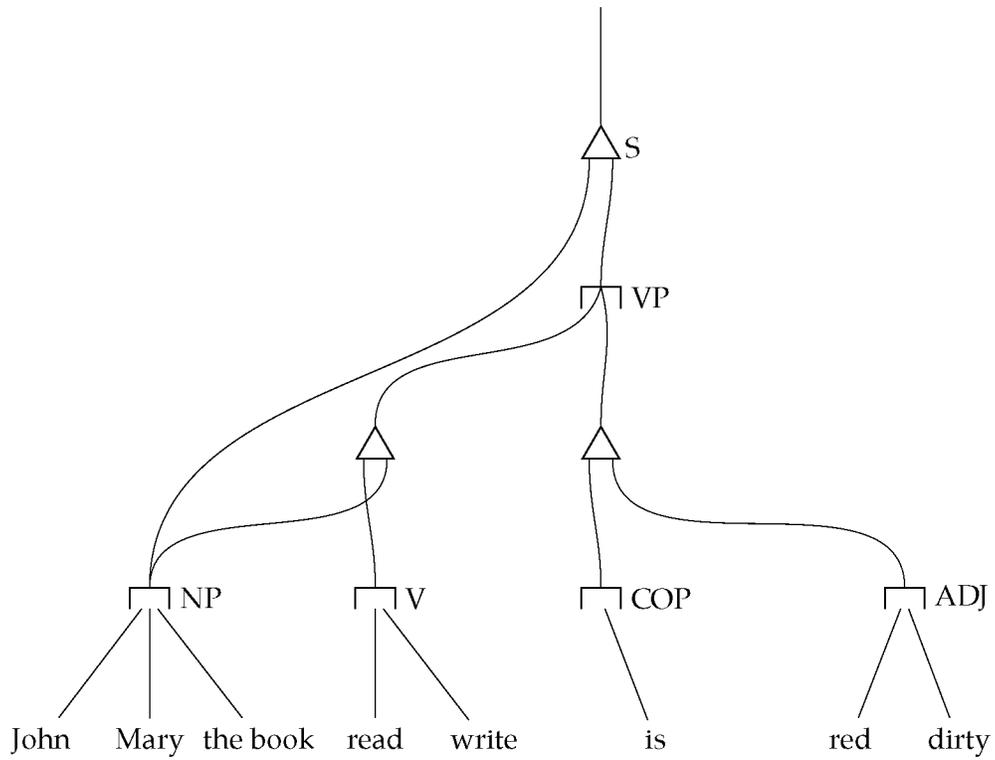
S Sentence
ADJ Adjective
COP Copula
NP Noun Phrase
V Verb
VP Verb Phrase

Figure 2.9: Lexotactics (adapted from Davis 1973)

Sememic
Stratum women and old men · · · old men and old women

Lexemic
Stratum the old men and women
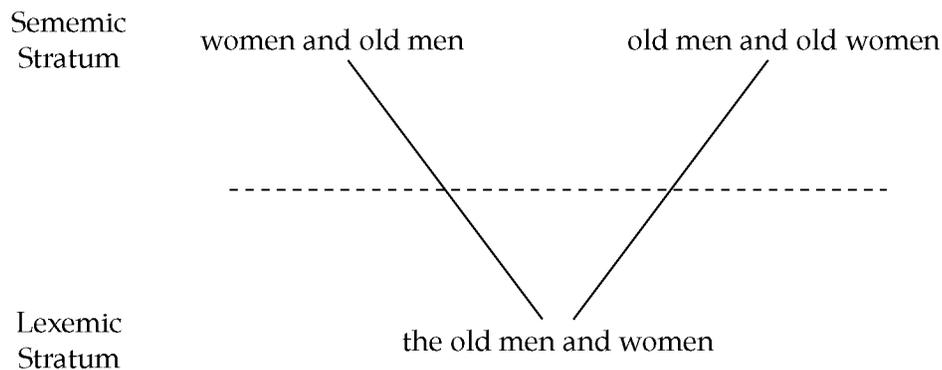
Figure 2.10: Neutralization at the lexemic-sememic stratal boundary

or more elements from a stratum are merged on the next higher stratum. For example, the morphemes /wood/, /peck/, and /-er/ compositely realize the lexeme /woodpecker/ on the lexemic stratum (figure 2.11). The eight types of realization are depicted graphically in figure 2.12. In the 1960s, stratificationalists were careful to emphasize that realization is a relationship, and not a process. Together with the belief that items form no part of linguistic structure, this rejection of process-accounts forms the foundation of the stratificational view of language as a system of relationships. All told, the strata, tactics, and realizational rules allow language structure as a whole to be expressed in terms of a single network with phonetic features at the lower end (representing the boundary between speech and the language network) and semantic features at the upper end (representing the boundary between semantic awareness and the language network), and with the network itself consisting of nothing but a complex pattern of lines and nodes.

Whereas the divide between syntax (namely, the study of sentence structure) and other levels of the grammar can fairly easily be made in transformational and immediate constituent grammars, this division is less clear in stratificational grammar. The term *tactics*, as used in sg, "has the same Greek roots as the term 'syntax', referring to arrangements", and was originally chosen by Charles Hockett to refer to "that part of the structure which is concerned with arrangements at whatever level".[160] With respect to syntax, under strat-

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[160] Parret, *Interview with Lamb*, p 193.

Lexemic
Stratum

woodpecker

Morphemic
Stratum

/wood/   /peck/   /-er/
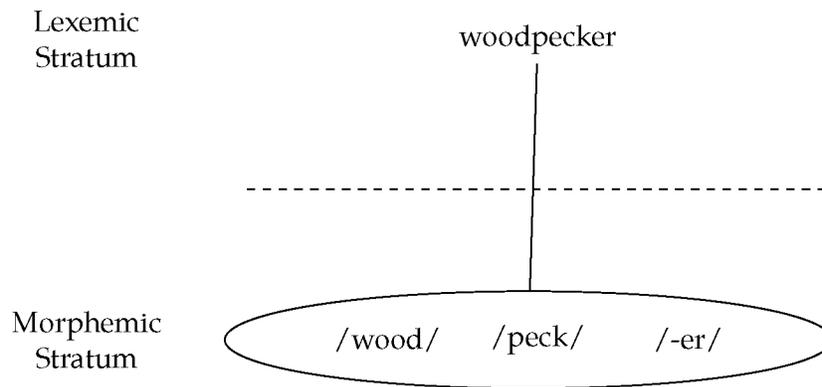
Figure 2.11: Composite realization at the morphemic-lexemic stratal boundary

ificational theory, "language has several syntaxes" and, in particular, every stratum has its own syntax.[161] The multiple syntaxes of stratificational grammar are, of course, the semotactics, lexotactics, morphotactics, and phonotactics, as described above. Thus, unlike standard grammatical theories which use "one 'syntax' to take care of all features of arrangement", SG spreads responsibility for arrangements over the entire grammatical spectrum.[162] Nonetheless, it is clear that the lexotactics – which is primarily concerned with phrasal structure – corresponds most closely to the standard idea of syntax; indeed, stratificationalist David Bennett describes the lexotactics as "roughly equivalent to syntax in the traditional sense", and Lamb describes the sentence as "primarily a lexotactic unit".[163] The lexemic stratum will accordingly be of most interest in this study.

In the lexotactic diagram (figure 2.9), it is important to notice that, for any declarative clause of the form SUBJECT–VERB–PREDICATE (to use traditional terminology) specified by this network pattern, the subject and the predicate both come from the same node (the noun phrase, or NP, node). This is a classic demonstration of the relational network basis of SG: subjects and predicates are distinguished only by their relationships to other elements in the grammar or, more accurately, to other nodes. As emphasized above, stratificational

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[161]*Idem.*

[162]Lockwood, *Inflectional Morphemes*, p 197.

[163]David C. Bennett, "English Prepositions," in Adam Makkai and David G. Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973), p 278, Parret, *Interview with Lamb*, p 217.

Simple Realization

Portmanteau Realization

Diversification

Zero Realization

Neutralization

Empty Realization

Composite Realization

Anataxis
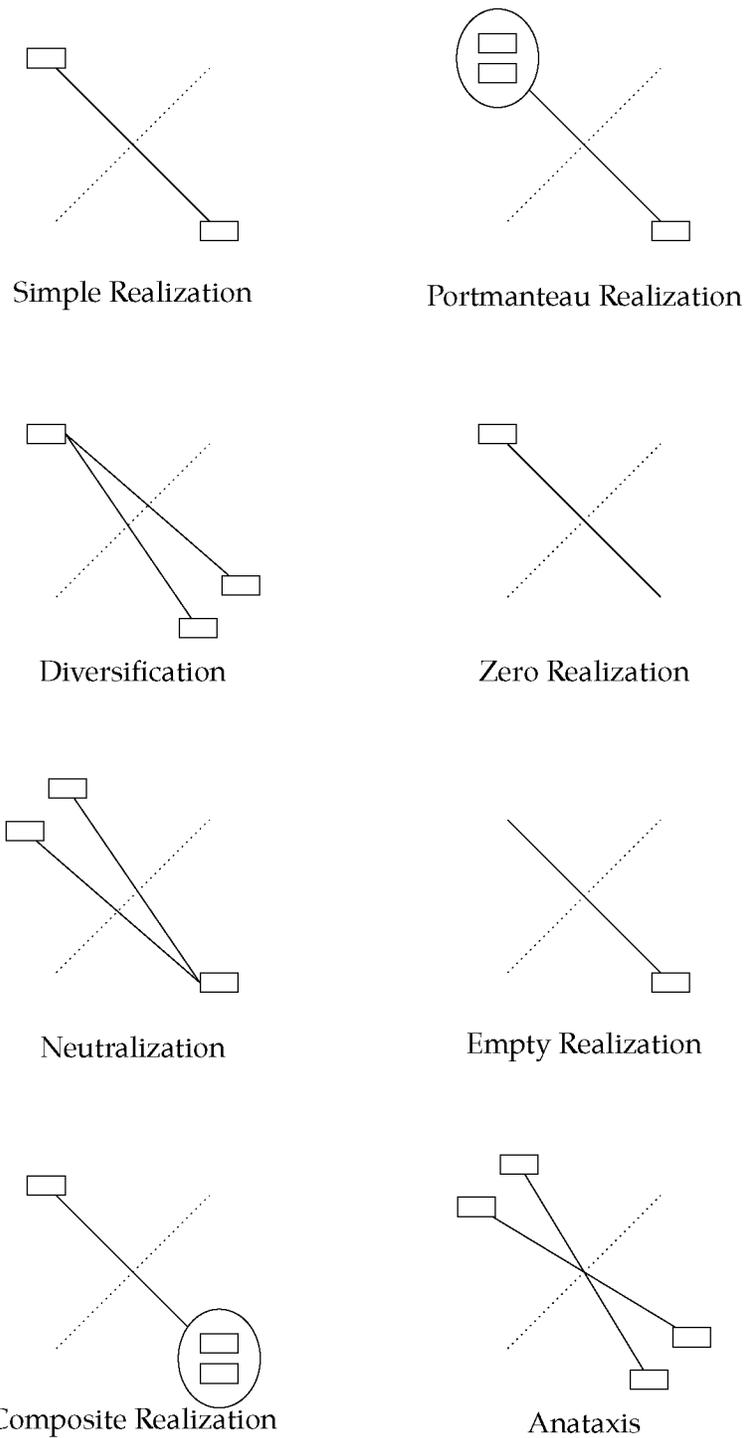
Figure 2.12: Types of realization (adapted from Makkai 1972)
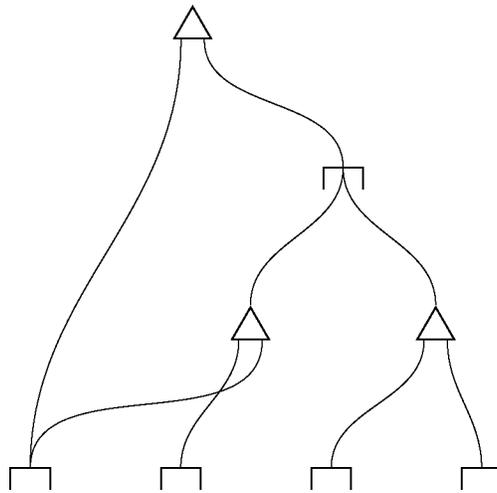
Figure 2.13: Lexotactic pattern (adapted from Davis 1973)

theory considers all units and labels to be superfluous: only the network structure itself carries meaningful content. Figure 2.13 below shows the same lexotactic pattern as figure 2.9, but with all labels removed – the network pattern which, in stratificational terms, fully captures the (heavily simplified) English declarative clause.

One of the most striking features of stratificational grammar is its graphic notation system. When Lamb first encountered Hjelmslev's work in the early 1950s – and, in particular, the Danish linguist's rejection of linguistic items – he did not fully appreciate the idea, "partly because Hjelmslev had provided no demonstration, nor a notation system to allow that assertion to be made concrete for people's observation".[164] It was English-Australian linguist Michael Halliday who provided Lamb with the necessary link between Hjelmslev's theoretical assumption and a fruitful notation system. In 1964, when Lamb was "stumbling along with rough and ready diagrammatic sketches", Halliday visited the United States and showed Lamb his notation system for systemic networks.[165] "With two or three simple modifications to Halliday's network notation", Lamb recalls, "I had the essentials of relational network notation".[166] This notation system soon became widely-used

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[164] Lamb, *Different Drummer*, p 119.
[165] *Idem*, p 118.
[166] *Idem*.

in the stratificational community. Lamb's notation would come to be both a triumph and a hindrance for stratificational grammar: to proponents of the theory, it represented an economical method of demonstrating and manipulating the network structure of language, while critics called it an overly complex and convoluted set of techniques (cf. Chapter 4).

Central to the idea of *explanation* in 1960s syntax was the ability to handle problematic syntactic constructions including active-passive pairs and ambiguous sentences. No syntactic theory of that decade could afford to ignore these constructions and, indeed, stratificational grammarians devoted time and effort to demonstrating the capacities of their theory in this respect (cf. Chapter 3). The essential feature of sg relevant to these constructions is the division of labor between the various strata, which allows pairs of constructions to be "identical or highly similar on one stratum, [but] significantly different on some other stratum".[167] For active-passive pairs, for example, the lexotactics specifies both the active and passive versions, while the next higher stratum (the semotactics) specifies an "underlying form [which is] distinct from both of these and in itself neither active nor passive", thus ensuring that both forms are preserved in the network.[168] Cases of ambiguity similarly rely on stratal distinctions. For phonologically identical pairs of sentences such as *the sun's rays meet* and *the sons raise meat*, the two phrases are identical on the phonemic stratum but distinct on the lexemic stratum. This "overlapping on some stratum of texts that are distinct on higher strata", John Algeo argued, can be extended to account for all homonymy constructions.[169]

Through the 1960s, stratificational grammarians saw their syntactic theory as a genuine alternative to transformational grammar and immediate constituency grammar, and their confidence was high. Stratificational theory, Gleason argued at the 1964 Georgetown Round Table Meeting, "gives a more intuitively satisfying picture of language organization than any other proposal yet made".[170] Proponents of the theory argued that they

[167]John Algeo, "Stratificational Grammar," *Journal of English Linguistics* 3 (1969), p 4–5.

[168]Sydney M. Lamb, "Review of Noam Chomsky's Current Issues in Linguistic Theory and Aspects of the Theory of Syntax," *American Anthropologist* 69/3 (1967), p 411.

[169]Algeo, *Stratificational Grammar*, p 4–5.

[170]H.A. Gleason Jr, "The Organization of Language: A stratificational view," in C.I.J.M. Stuart (ed.), *Report of the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Studies, 1964* (Washington

succeeded in several areas where rival theories failed: explaining language as a communicative device (that is, explaining both the production and comprehension of language), accounting for language change (for example, the introduction of a new vocabulary term or metaphor), and explaining supra-sentence phenomena (such as idioms, poetry, and discourse). These are discussed in detail in Chapter 3; importantly, however, they are all based on the stratificational commitment to a multi-level, or stratified, view of language. No other linguistic theory, stratificationalists argued, maintained such a view. Lamb describes constituency grammars and early transformational grammars as "*single level* views of linguistic structure".[171] In the *Syntactic Structures* version of transformational theory, he argues, "one takes essentially a surface structure and then applies the transformation upon it to form another surface structure" – a process which does not recognize a functional separation of levels.[172] With the explicit definition of deep and surface structures in *Aspects of the Theory of Syntax*, stratificationalists revised their assessment of the theory, but still did not identify transformational grammar as stratified. Lockwood called the *Aspects* model "quasi-stratificational", arguing that deep structure was "largely a rearrangement of surface elements", whereas a truly stratified view of language recognizes the involvement of "different sets of elements as well as basically different types of arrangements".[173] Lamb was less accommodating, arguing that deep structure was "not on a really separate level in the sense that we, in stratificational grammar, speak of a separate level", because of the nature of transformational rules: these rules, Lamb continues, "take[] a representation and convert[] certain parts of the representation into another form; [they] convert[] one representation into another representation of essentially the same kind. So, there is no real break between the levels of deep structure and surface structure corresponding to the real break that we find in a stratificational framework between, say, the sememic stratum and the lexemic stratum".[174] While stratificationalists were intent on distinguishing themselves from

DC: Georgetown University Press, 1964), p 95.
[171] Parret, *Interview with Lamb*, p 181, emphasis in original.
[172] *Idem*, p 182.
[173] Lockwood, *Stratificational Linguistics*, p 268.
[174] Parret, *Interview with Lamb*, p 183.

transformational and immediate constituency grammarians in terms of their adherence to a stratified account of language, they did not see their theory as the only possible manifestation of this view: they also recognized Hjelmslev's glossematics and Halliday's systemic grammar as multi-level linguistic theories.

Stratificational Grammar: Development and influence

In the late 1950s and 1960s, stratificational grammar was developed primarily at Berkeley, Yale, and the University of Michigan. After Lamb left California for Yale in 1964, Berkeley – far removed geographically from the transformational hub in Massachusetts – remained oriented towards stratificational theory. Lamb's appointment at Yale brought SG to the forefront at that school, and it also enabled Lamb to supervise students at Michigan, which soon grew into a third stratificational center. Lamb's theory also enjoyed support from SIL International and the Hartford Seminary Foundation, two organizations which provided linguistics training for missionaries.

Many of the first generation of stratificationalists were involved with the Yale Linguistic Automation Project, inaugurated in 1966, which developed computer-based performance models of language. Well-supported by the National Science Foundation, the Automation Project provided funding to Lamb's graduate students. Most prolific among these students were Adam Makkai, who completed his doctoral dissertation on idiom structure and stratificational theory at Yale under Lamb in 1965 and subsequently built his career at the University of Illinois at Chicago; Peter Reich, a graduate student at the University of Michigan who became Lamb's chief assistant at the Linguistic Automation Project, and who developed the first computer simulation of language networks; Ilah Fleming, also a graduate student at the University of Michigan and Automation Project researcher who developed a model of SG suitable for writing descriptions of languages which was subsequently widely used by American missionaries; and David Lockwood, who, having been introduced to SG by Lamb at the 1965 Linguistic Society of America Linguistic Institute, received his Ph.D. in 1966 from the University of Michigan and subsequently taught at Michigan State Uni-

versity.[175] These four students became vocal advocates of SG, publishing defenses of the theory and training the second generation of stratificational grammarians. However, despite Lamb's National Science Foundation grants at Berkeley and Yale, transformational grammarians received much more government funding from both military and civilian sources through the 1960s.

At the Hartford Seminary Foundation, H.A. Gleason tailored stratificational theory to fit the needs of missionary work. Founded in the late 19th century and endowed with a university charter, the Hartford Seminary Foundation offered preparatory courses for missionaries in linguistics, religion, and cultural subjects. Through the 1950s and 1960s, the Foundation offered masters and doctoral level programs in linguistics. Under Gleason's influence, the theoretical approach to linguistics adopted by the Foundation in those decades was that of stratificational grammar. Born in 1917, Gleason wore two hats in his professional career: he served as a missionary in India and in Appalachia and, in the 1940s, as a pastor in Virginia; also, after receiving a doctorate in religion from the Hartford Seminary Foundation in 1946, he worked as a linguistics professor at the Seminary in the 1950s and 1960s and, subsequently, at the University of Toronto, where he spent the final two decades of his career.[176]

Gleason saw stratificational theory as the only linguistic theory of the 1960s which was suitable for missionary work. Transformational and immediate constituency grammars were not suitable for such work, he argued, because the missionary's task requires an approach which "deals with texts, not isolated sentences" and which is interested primarily in "the structure of discourses [and] only secondarily of sentences".[177] The stratificational model developed at the Hartford Seminary Foundation placed minimal emphasis on sentence structure, focusing primarily on larger units of discourse, and attempting to "encompass whole texts".[178] When Gleason left Hartford for the University of Toronto in 1967, the Seminary wound down its linguistics program. In his 1988 memoir, Gleason reiterated his

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[175] Lamb, *Different Drummer*, p 125.
[176] William Forrest, "Obituary: H. Allen Gleason Jr. 1917–2007," *Linguist List* 18 (2007).
[177] Gleason Jr, *Theories in Conflict*, p 129.
[178] *Idem*, p 132.

commitment to stratificational grammar, asserting that "I still think of linguistic problems in a Stratificational framework [and] I still believe that in certain fundamental points it is superior to any other theory I know".[179]

A second link between stratificational grammar and missionary work was provided by SIL International.[180] Founded in Arkansas in 1934, SIL is a Christian organization which "studies, documents, and assists in developing the world's lesser-known languages", and offers courses in linguistics for missionary students, furloughed missionaries, and Bible translators.[181] Along with its sister organization, the Wycliffe Bible Translators, SIL has long been a player on the American linguistics scene. Under the leadership of Kenneth Pike (1912–2000), SIL was represented at the Linguistic Society of America and in American academic linguistics, particularly at Michigan. Between its foundation in 1934 and 1960, SIL trained over 4500 missionaries and related workers in linguistics, and actively worked on more than 200 languages from around the world.[182] Indeed, SIL is credited as being the foremost producer of descriptive linguistic studies in America through the 20th century. These studies were frequently published in mainstream American linguistics journals, including *Language*, *Word*, and *The International Journal of American Linguistics*.

Through the 1960s and early 1970s, SIL linguists often employed stratificational grammar, contributing to its theoretical development and to the publication of partial grammars under its framework. Akin to the Hartford Seminary Foundation, SIL found in stratificational grammar a theoretical framework amenable to its missionary-oriented goals. With its emphasis on idiom, metaphor, and textual content, stratificational theory was particularly useful for translation and proselytizing purposes. SIL linguists applied stratificational grammar to "the analysis of discourse and semantics" and used Lamb's network diagrams to analyze participant-action discourse relationships.[183] Several linguists straddled the SIL

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[179]*Idem*, p ix.

[180]SIL originally stood for the Summer Institute of Linguistics; it should not be confused with the summer Linguistic Institute offered annually by the Linguistic Society of America.

[181]SIL International Homepage, SIL.

[182]Moulton, *op. cit.*, p 107.

[183]Calvin R. Rensch, "The Contributions of SIL in Linguistics," in Ruth M. Brend and Kenneth L. Pike (eds.), *The Summer Institute of Linguistics: Its works and contributions* (The Hague: Mouton, 1977), p 95.

and academic worlds: Ilah Fleming, for example, contributed an annotated bibliography of work in stratificational grammar (1969), and Leonard Newell's stratificational study of English was published as an appendix to Lamb's *Outline of Stratificational Grammar* (1966). The relationship between missionary work and stratificational theory, and its ramifications for the theory-choice debates of the 1960s, is explored in detail in Chapter 6.

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From its enunciation in the mid-1950s until 1964, stratificational theory was transmitted primarily by oral presentation and personal contact. Lamb presented his theory to the Berkeley Linguistics Group in 1956, at the Linguistic Society of America's 1959 annual meeting and 1961 Christmas meeting, to the Cornell and Yale Linguistics Clubs in 1962, and at the 1964 Georgetown Round Table Meeting on Linguistics and Language Study – an array of conferences and locations which promoted stratificational theory on both coasts and reached a large number of linguists. Most prominently, Lamb lectured at the 1965 Linguistic Institute, held at the University of Michigan, where he gained several students who would become standard-bearers of the theory, including David Lockwood and Peter Reich.[184] Lamb put his theory on paper in 1962, with the first draft of *Outline of Stratificational Grammar*. This manuscript, however, only circulated in multilithed form through the Berkeley bookstore and the University of California Machine Translation Project: outside of Berkeley, it was "not freely available".[185] It wasn't until 1964 that SG appeared in mainstream publications: in that year, Lamb's *The Sememic Approach to Structural Semantics* appeared in *American Anthropologist*, and Lamb and Gleason each had their papers to the 15th annual Georgetown Round Table Meeting published in the widely-circulated proceedings of the conference (respectively, *On Alternation, Transformation, Realization, and Stratification*, and *The Organization of Language: A stratificational view*).[186]

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[184]Lamb, *Different Drummer*, p 120.

[185]Lamb, *Outline of Stratificational Grammar*, p iii, F.R. Palmer, "Review of Paul Postal's Constituent Structure: A study of contemporary models of syntactic description," *Foundations of Language* 1 (1965).

[186]Lamb, *Sememic Approach*, Sydney M. Lamb, "On Alternation, Transformation, Realization, and Stratification," in C.I.J.M. Stuart (ed.), *Report of the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Studies, 1964* (Washington DC: Georgetown University Press, 1964), Gleason Jr, *Organization of Language*.

The keystone of SG – an elongated and formal version of Lamb's *Outline of Stratificational Grammar* – was published by Georgetown University Press in 1966, four years after it was first sketched.[187] This monograph contains the first in depth printed presentation of Lamb's graphic notation system, and consequently is largely devoted to a detailed explanation of both the stratificational characterization of language structure and its particular notation technique. The book also contains a small selection of exercises meant to familiarize the reader with both the theory and its notation. It was this manuscript which was, for a decade after its publication, the public face of stratificational grammar in America. However, by Lamb's own description, the 1966 *Outline* was "a rather hasty interim document" written in a rush in the six weeks preceding the 1966 Linguistic Institute held at UCLA – qualities which would hinder the reputation of the book and, consequently, the theory.[188] Reviews of and commentaries on Lamb's *Outline* commonly complained that it was neither complete nor well-written; R.D. Huddleston noted that it was hindered by its "brevity and paucity of exemplification", and F.W. Palmer called it "very disappointing" for not providing a stand-alone account of stratificational theory.[189]

The mid- to late-1960s and early 1970s saw an explosion in the publication of technical studies and partial grammars in the stratificational framework. While in these years transformational grammarians were tackling specific grammatical constructions, stratificationalists had a much broader range of interests, from idiom structure to synonymy to poetry, and from nursery rhymes to metaphors to number systems.[190] At the peak of the stratificational focus on supra-sentence constructions came Leonard Newell's analysis of a tape-recorded dinner conversation between a mother, father, 10-year-old daughter, and

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[187] Lamb, *Outline of Stratificational Grammar*.

[188] *Idem*, p iii, Lamb, *Different Drummer*.

[189] R.D. Huddleston, "Review of David G. Lockwood's Introduction to Stratificational Linguistics (1972)," *Journal of Linguistics* 9/2 (1973), p 350, F.R. Palmer, "Review of Sydney Lamb's Outline of Stratificational Grammar (1966)," *Journal of Linguistics* 4/2 (1968), p 287.

[190] Adam Makkai, *Idiom Structure in English* (The Hague: Mouton, 1972), Adam Makkai, "The Transformation of a Turkish Pasha into a Big Fat Dummy," in Adam Makkai and David G. Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973), Bennett, *op. cit.*, David C. Bennett, "English Prepositions: A stratificational approach," *Journal of Linguistics* 4 (1968), Geoffrey Sampson, *Stratificational Grammar: A definition and example* (The Hague: Mouton, 1970).

12-year-old son.[191] This interest in supra-sentence phenomena was characteristic of strat-ificational grammar in the 1960s: proponents of the theory emphasized in writing and in practice that their approach was explicitly designed to account not only for sentences, but also for larger texts including "paragraphs, narratives, sonnets, five-act tragedies, epics, and the *Encyclopaedia Britannica*, any text that has formal unity".[192] Whereas transformational grammarians "seem unable to look beyond the sentence", stratificationalists argued, sg provided a broader approach to language – one which worked with and tried to explain grammatical and literary phenomena beyond the boundary of the sentence.[193] These works, along with stratificational studies of more traditional linguistic constructions, appeared in, among others, the Chicago Linguistic Society Papers, *Language, American Anthropologist*, the European-based *Journal of Linguistics*, and doctoral dissertations. In these years, however, stratificational grammar was underrepresented in the main American linguistics journals in comparison to transformational grammar. Between 1960 and 1970, *Language* published over 35 articles in the transformational paradigm, and only two in the stratificational paradigm. In the same decade, *The International Journal of American Linguistics* published three times as many transformational articles as stratificational ones. This lopsided representation was, as discussed below, actively protested by the stratificational community.

Even as stratificational grammarians increased their market share through the late 1960s, they were clearly lagging behind transformational grammar in terms of publications and research, and this discrepancy was weakening their theory. In a 1969 article largely favorable to sg, the University of Florida's John Algeo complained that "there is as yet no extensive grammar of any language written in stratificational terms", concluding that "[t]he theory itself is far from fully developed".[194] "There has been little in print on which to base an appraisal of [Lamb's] ideas", commented transformationalist Emmon Bach in 1965

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[191]Leonard E. Newell, "Stratificational Analysis of an English Text," in Sydney Lamb (ed.), *Outline of Stratificational Grammar* (Washington DC: Georgetown University Press, 1966).

[192]Algeo, *Stratificational Grammar*, p 7.

[193]*Idem.*

[194]*Idem*, p 1–2.

– a situation which might have been rectified by the 1966 publication of Lamb's *Outline of Stratificational Grammar*, had it been better written. Lamb has long recognized that his oral presentation skills – he describes his lectures as "less than crystal clear" – did not make up for the lack of written sources.[195]

Particularly telling is the complete lack of stratificational textbooks during the 1960s. This paucity of publications was only rectified in the early 1970s with the release of two books geared towards the classroom. The first, David Lockwood's 1972 textbook *Introduction to Stratificational Linguistics* (Harcourt Brace Jovanovich, Inc.), provided the first formal pedagogical tools for SG. Previously, pedagogical materials for stratificational grammar had been limited to unpublished teaching materials and exercises written by Lamb and Reich.[196] Lockwood's text grew out of a course he had been offering at Michigan State University since 1968. "This book is intended to fulfill the need for an introduction to the stratificational theory of language", he wrote in the preface, and "[s]ince relatively few linguists are presently equipped to teach a course in stratificational linguistics even with the aid of such a textbook, this work is further intended to be adaptable for self-instruction by the linguistic scholar and advanced graduate student wishing to familiarize himself with the general principles and specific applications of stratificational linguistics. The book is designed to provide a thorough introduction to the fundamental principles of the theory and its application to a considerable range of specific linguistic problems. Such specific treatments have been few in number, unfortunately, in the earlier stratificational literature".[197] The text was received with great enthusiasm within the stratificational community, and was considered to provide "the most comprehensive account today of stratificational grammar".[198] However, the eight-year time lag between the publication of the first transformational textbook and the first stratificational textbook would be instrumental to the success of the former theory (cf. Chapter 4).

The second major stratificational work of the early 1970s was an anthology entitled *Read-*

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[195]Lamb, *Different Drummer*, p 103–4.
[196]Sydney M. Lamb, *Personal Communication with Janet Martin-Nielsen*.
[197]Lockwood, *Stratificational Linguistics*, p v.
[198]Huddleston, *op. cit.*, p 349.

ings in Stratificational Linguistics, edited by Adam Makkai and Lockwood, and published by the University of Alabama Press in 1973.[199] With its collection of 16 articles, several of which had previously appeared only in "relatively inaccessible places" such as technical reports of the Yale Linguistic Automation Project, this anthology took a large step towards rendering stratificational grammar more accessible to the American linguistic community.[200] The editors hoped that "this work will answer the frequently heard demand for a single volume which will explain the workings of stratificational linguistics to the uninitiated, and which can serve as a textbook for graduate or advanced undergraduate courses dealing with the comparison of modern linguistic theories and stratificational grammar specifically", and asserted that the anthology fulfilled an "urgent need" in the profession.[201] Together, the textbook and the anthology brought about a new respect for stratificational grammar: "As of 1972", wrote William Sullivan, the claim "that the stratificational viewpoint has not been extensively articulated in print [...] can no longer be considered valid".[202]

Nonetheless, stratificational grammarians still felt that their theory was unfairly underrepresented in mainstream American linguistics. Together with others outside of the transformational establishment, they "felt that their views were not being adequately represented by the existing associations and their journals".[203] "[T]he natives are restless", wrote Indiana linguist Fred Householder, describing supporters of non-transformational linguistic theories who, by the early 1970s, were fed up with their status as second-class citizens.[204] This restlessness came to a head in 1974 when the non-TG community came together to "set up their own shop", resulting in the formation of the Linguistic Association of Canada and

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[199]Adam Makkai and David G. Lockwood (eds.), *Readings in Stratificational Linguistics* (Alabama: The University of Alabama Press, 1973).

[200]Alan H. Sommerstein, "Review of Adam Makkai and David G. Lockwood's (eds.) Readings in Stratificational Linguistics," *Journal of Linguistics* 10/2 (1974), p 313.

[201]Makkai and Lockwood (eds.), *Readings in Stratificational Linguistics*, p v.

[202]William J. Sullivan, "Review of Adam Makkai and David G. Lockwood's (eds.) Readings in Stratificational Linguistics," *American Speech* 49 (1974), p 146.

[203]Bolinger, *First Person*, p 37.

[204]Fred W. Householder, "Review of Adam Makkai and Valerie Becker Makkai (eds.) The First LACUS Forum 1974," *Language* 54/1 (1978), p 170.

the United States (LACUS).[205] As "a counter-fashion, a community of independent work-ers outside the dominant TG paradigm", LACUS provided an outlet for stratificationalists across North America.[206] Spearheaded by linguists Adam Makkai (University of Illinois at Chicago), Valerie Becker Makkai, and Dwight Bolinger (Harvard), LACUS opened doors for the stratificational community in two ways: it published a journal, *Forum Linguisticum*, and held an annual forum which provided a conference setting for the exchange of ideas and resulted in the complete publication of papers and proceedings each year. The location of the forum alternated each year between universities in Canada and the United States, emphasizing the particular importance of Canadian linguistics to the stratificational com-munity. Stratificational grammar holds a prominent place in LACUS Forum proceedings of the 1970s and 1980s and, indeed, LACUS provided a primary publication outlet for strati-ficational grammarians in those decades. LACUS was careful not to explicitly espouse any particular theoretical framework, but its publication cycle clearly favors stratificational the-ory. While LACUS provided a much-needed outlet for stratificational grammarians, it was not sufficient to enable them to challenge the dominance of transformational grammar.

2.4   Immediate Constituency Grammar

> [D]uring that period in American linguistic theory, the notion of immediate
> constituents had cast a spell over us. Whenever our attention turned to syntax
> the IC [immediate constituent] frame came to mind.
>
>   Charles Hockett, in *Approaches to Syntax* (1997)[207]

Immediate constituency grammar (ICG) was the premier syntactic theory in America in the 1940s and 1950s. As phonology and morphology gave way to the study of syntax, ICG provided a theoretical framework which dominated the study of sentence structure for two decades. Through the 1960s and early 1970s, as constituency grammar was eclipsed

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[205]Bolinger, *First Person*, p 37.

[206]J. Peter Maher, "The Transformational-Generative Paradigm: A silver anniversary polemic," *Forum Linguisticum* 5/1 (1980), p 5.

[207]Charles F. Hockett, "Approaches to Syntax," *Lingua* 100 (1997), p 160.

by other syntactic theories, it retained a large number of supporters across America and continued to play a key role in machine translation, fieldwork, and language teaching, but would not regain its former position as America's foremost syntactic theory. Constituency grammar is of interest here because of its dominance in the 1940s and 1950s; because it became a baseline theory against which rivals were measured; and because it retained an important influence in particular areas through the 1960s and beyond.

Ground-breaking studies by Wells, Bloch, Nida and Harris in the 1940s (cf. section 2.1) set the stage for the rise of constituency grammar in the 1950s. These works were read and discussed by the American linguistics community as a whole, and formed the foundation for the first large-scale analysis of syntactic structure in the United States. By the late 1940s, constituency theory had been systematized and began to be extensively applied, and through the 1950s the theory was refined into a broadly-applicable set of syntactic methods which slowly shed their adherence to the Bloomfieldian program. In these decades, optimism was high that the application of constituency analysis would lead eventually to a full understanding of syntax – an optimism fueled by both the success of Descriptivism on lower linguistic levels and the Second World War legacy of American linguists. In the 1960s, however, as Chomsky's arguments against the Bloomfieldian philosophy gained acceptance, proponents of ICG shifted their philosophical views away from behaviorism and anti-mentalism while still retaining the theoretical core of constituency grammar. The emergence of rival syntactic theories forced constituency grammarians to develop and modify their theory in order to meet new technical and philosophical expectations. In these years, unlike transformational and stratificational grammars, ICG had no prominent figure-head at its helm; rather, it was developed and pursued by a disperse community of linguists working at a wide variety of institutions.

Immediate Constituency Grammar: Theory

The fundamental motivation for constituency analysis is the intuition that sentences cannot be analyzed simply as linear strings of words or morphemes, or, that sentences have

an inherent hierarchical structure. "[T]he native speaker hears the sentence not as a linear string of morphemes", Charles Hockett wrote in his 1958 textbook *A Course in Modern Linguistics*, but "in depth, automatically grouping things together in the right way" – and thus sentence analysis must also assume a "hierarchical structure [or] immediate constituent structure".[208] This technique aims to divide a given sentence, or sequence of words, into constituents in such a way so as to reveal something about "the complexity of the sentence" and, more broadly, the complexity of language itself.[209] As described in Bernard Bloch's classic 1946 study of Japanese syntax, the process is to "first isolate the immediate constituents of the sentence as a whole, then the constituents of each constituent, and so on to the ultimate constituents – at every step choosing our constituents in such a way that the total number of different constructions will remain as small as possible. We regard the analysis of the sentence (the syntactic analysis) as complete when further analysis would reveal only constructions different in kind from all the constructions established up to that point. An element that emerges from the analysis as an ultimate constituent of a sentence is typically a word".[210] Importantly, following the Bloomfieldian tradition, constituency analysis rejects the use of semantic criteria for determining syntactic structure: constituents are determined not on the basis of meaning, but of distribution and co-occurrence.

The first systematic treatment of immediate constituents appeared in Rulon Wells' 1947 article *Immediate Constituents*, published in *Language*. A linguist and philosopher at Yale, Wells aimed to "replace by a unified, systematic theory the heterogeneous and incomplete methods hitherto offered for determining immediate constituents", and thereby provide the theory with a standard set of descriptive and analytic tools.[211] In Wells' account, constituents are determined on the basis of co-occurrence: two sequences of sounds are said to be co-occurrent if they occur "in the same environments even though they have different internal structures", or, if some (usually contiguous) set of words is syntactically equivalent

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[208]Charles F. Hockett, *A Course in Modern Linguistics*, 1st edition (New York: The MacMillan Company, 1958), p 149–150.

[209]Curtis W. Hayes, "Syntax: Some present-day concepts," *The English Journal* 56/1 (1967), p 91.

[210]Bloch, *Japanese Syntax*, p 204–205.

[211]Wells, *op. cit.*, p 81.

to a smaller (usually contiguous) set of words.[212] For example, the set of words *unmarried man* is syntactically equivalent (that is, substitutable for) the set of words *bachelor* in a variety of constructions and expressions. The fundamental aim of ICG, Wells argued, is "to analyze each utterance and each constitute into maximally independent sequences – sequences which, consistently preserving the same meaning, fit in the greatest number of environments and belong to focus-classes with the greatest possible variety of content".[213] Wells' article provided linguists with tools for systematically conducting immediate constituency analysis. His article was widely seen as a significant step towards the creation of a full constituency theory and, in the following years, a great many American linguists spent time "digesting his insights and experimentally applying his theory of IC [immediate constituent] analysis to various languages".[214] Applications of Wells' methodology built confidence in the procedures of ICG and in the ability of constituency analysis to cap off the Descriptivist study of language. Within a few years, "the notion of immediate constituents had cast a spell over us", Hockett recalls, and "[w]henever our attention turned to syntax the IC frame came to mind".[215]

While highly-regarded, Wells' article was by no means final and debate over the proper way of dividing strings of morphemes into constituents continued through the late 1940s and 1950s. At the heart of this debate was the lack of a fully-worked out mechanical method for uniquely determining constituency cuts. Wells established preliminary procedures for choosing between various possible constituency analyses – procedures which, for example, determined that the construction *the King of England* should be divided into immediate constituents as *the + king of England*, and not as *the King + of England*. In his 1969 review article, the University of Wisconsin's John Street identifies several criteria used in the 1950s "for preferring this or that cut": internal cohesion (preference is given to strings of morphemes which function as cohesive units), independence (preference is given to strings

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[212]*Idem*, p 88.

[213]*Idem*.

[214]John C. Street, "Methodology in Immediate Constituent Analysis," in Irmengard Rauch and Charles T. Scott (eds.), *Approaches in Linguistics Methodology* (Madison: The University of Wisconsin Press, 1969), p 90.

[215]Hockett, *Approaches to Syntax*, p 160.

which occur in a wide variety of constructions), and simplicity (preference is given to simple strings of morphemes).[216] These criteria were by no means necessary nor sufficient: in some cases they conflict with each other, and in others they fail to provide a satisfactory analysis. Taken together, however, they were seen to be "not simply vacuous artifacts of analysis", but to provide an understanding of "something real and useful in the structure of language".[217]

The 1940s and 1950s saw no standard notation for constituency theory. Diagrammatic techniques varied between linguists and between applications, and included, among others, tree diagrams, boxed constituency diagrams, and vertical line diagrams (shown below). The choice of techniques was based most often on personal preference: particular linguists preferred particular notation systems for tasks such as classroom teaching, textbook writing, syntactic analysis, and presentation of research results. There was little discussion about diagramming techniques, about the relative merits of the various systems, or about the relationship between notation and theory. In these years, diagrams were thought of "as little more than aids to presentation or heuristic tools", and were rarely if ever considered to contain implicit theoretical assumptions.[218] Linguists were focused "in the language not in the notation" – a remnant of the Bloomfieldian program, which emphasized fieldwork and description over theory and analysis.[219] As rival syntactic theories introduced more sophisticated ideas about notation in the 1960s, however, constituency grammarians were forced to reconsider the role and status of the diagramming techniques (cf. Chapter 4).

The four diagrams below show constituency analyses of the sentence *the old man who lives there has gone to his son's house*. The vertical line diagram (figure 2.14) indicates the 'height' of constituency divisions by the thickness of the vertical lines. Easy to typeset and taking up little space on the page, these diagrams were popular for published works in the 1940s and 1950s. The same information is presented in the grouping diagram (figure 2.15), where constituents are successively grouped from smallest to largest. The boxed constituency

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[216]Street, *op. cit.*, p 91–94.

[217]*Idem*, p 91.

[218]Gleason Jr, *Theories in Conflict*, p 38.

[219]*Idem*.

The | old ¦ man ‖ who | lives ¦ there ▌ has ¦ gone ▌ to ‖ his ¦ son's | house

Figure 2.14: Vertical line diagram

The old man who lives there has gone to his son's house

Figure 2.15: Grouping diagram

The	old	man	who	lives	there	has	gone	to	his	son's	house
The	greybeard		who	survives		went		to	that		house
The	greybeard		surviving			went		to	Boston		
The	survivor					went		there			
He						went					

Figure 2.16: Boxed constituency diagram
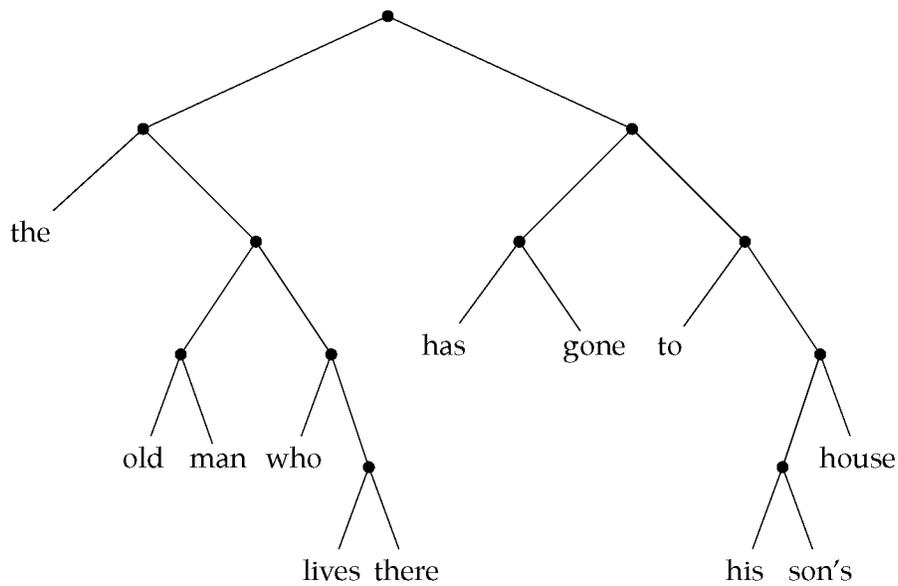
Figure 2.17: Tree diagram (figures 2.14 to 2.17 adapted from Gleason 1955)

diagram (figure 2.16) shows explicitly how constituents are identified on the basis of the substitutability of a number of morphemes for a larger number of morphemes. This diagramming technique was popular for pedagogical purposes, because it clearly shows the analytic process used to arrive at the final constituent structure. Finally, the tree diagram (figure 2.17) represents constituency cuts in terms of tree branches, where the topmost split marks the "most fundamental cleavage in the utterance".[220] This type of diagram became popular in the 1960s in both immediate constituency and transformational grammars (cf. Chapter 4). These examples make clear Zellig Harris' identification of the fundamental difference between constituency and transformational theory: in ICG, he said, the key question is "How is [a sentence] composed out of smaller stretches?", while in TG it is "From what other sentence or sentences can it be said to have been transformed?".[221]

A point of pride for constituency grammarians in the 1940s and 1950s was their ability to handle ambiguous constructions such as *old men and women*, which has the two potential meanings *women and old men* and *old men and old women*. In his 1947 article, Wells accounts for this ambiguity with two different constituency analyses, shown in figure 2.18: the first is of the form NOUN/NOUN PHRASE + AND + NOUN/NOUN PHRASE, and the second is of the form MODIFIER + NOUN PHRASE where the noun phrase itself is of the form NOUN/NOUN PHRASE + AND + NOUN/NOUN PHRASE.[222] This analysis uncovers the structural nature of the construction, Wells notes, by "reveal[ing] a formal difference correlated with the semantic one".[223] It is important to note this appeal to semantic content, or meaning: while constituency grammar could be "developed up to a certain point without a consideration of meaning" in the spirit of Bloomfieldian linguistics, cases of ambiguity often required a consideration of the semantic characteristics of the construction.[224]

The technical content of constituency theory is not particularly complex. However, it is important to understand how this technical content was wedded to a philosophical frame-

[220]Gleason Jr, *Descriptive Linguistics*, p 130.

[221]Zellig S. Harris, "The Transformational Model of Language Structure," *Anthropological Linguistics* 1 (1959), p 28.

[222]Wells, *op. cit.*, p 96.

[223]*Idem*, p 93.

[224]*Idem*.

Semantic
interpretation   *women and old men*   *old men and old women*

Structural
representation

```
              NP                              NP
      ┌───────┼───────┐              ┌────────┴────┐
     NP      AND      NP            MOD           NP
    ┌─┴─┐              │             │         ┌───┼───┐
  MOD   N              N            old    N  AND    N
    │   │              │                   │         │
   old men          women                men      women
```

```
              NP                              NP
      ┌───────┼───────┐              ┌────────┴────┐
     NP      AND      NP            MOD           NP
                                             ┌────┼────┐
                                             N  AND    N
```

MOD Modifier
N Noun
NP Noun Phrase
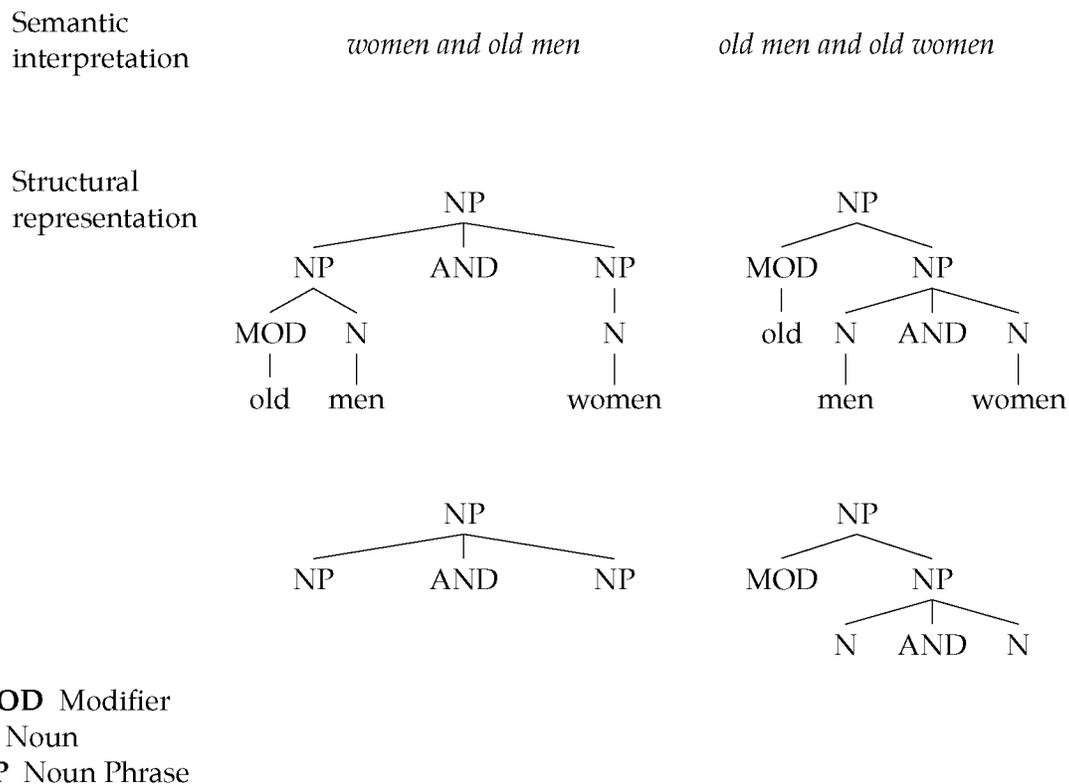
Figure 2.18: Constituency analysis of *old men and women*

work, and how this relationship changed between the 1940s and the 1960s. When it was first elaborated, ICG was firmly set in Bloomfieldian Descriptivism – a program centered on fieldwork and the study of Amerindian languages, and which subscribed to a behaviorist philosophy. As the years progressed, and as the philosophical underpinnings of Descriptivism collapsed, constituency grammarians shed their early philosophical beliefs and developed a syntactic theory in line with the new mentalism which pervaded American psychology, cognitive science, and linguistics in the 1960s. This progression manifested itself in constituency theory with an emphasis switch from compatibility with the Bloomfieldian framework to tackling specific technical problems in the theory. This progression can best be seen in a chronological series of ICG textbooks from 1951 to 1958, and through the technical work of the 1960s.

The first of these textbooks, Zellig Harris' *Methods in Structural Linguistics* (University of

Chicago Press, 1951), represents the epitome of the Bloomfieldian program. As a standard university textbook through the 1950s, it had "great intellectual impact" on American linguistics.[225] Harris codified linguistic methodology, set out formal research methods for the development of linguistic discovery procedures, and made explicit the intuitive practices used by linguists in their everyday work. The core of the text contains a set of operations which, if performed on an input corpus, yield a distributional statement of the utterances in the language.[226] This statement consists of a "compact representation" of elements and the rules governing their relative distribution for each level of language (phonology, morphology, ..., utterances).[227] The aim of this analysis, Harris asserts, is to gain more information about language than can be obtained from a "mere list of sounds and forms".[228] Importantly, while Harris emphasizes the use of corpus data, he is interested in corpora which are capable of representing the language as a whole: the linguistic analysis of a corpus, he writes, "becomes of interest only if it is virtually identical with the analysis which would be obtained in like manner from any other sufficiently large corpus of material taken in the same dialect".[229]

Harris' textbook is firmly set in the Bloomfieldian philosophy: he rejects appeals to meaning to identify or classify elements, forbids linguists from allowing intuition or beliefs about language to influence data, and assumes that general linguistics principles will emerge from the collection of observed facts. Meaning and intuition are often used as shortcuts or heuristics, he argues, but can always be replaced by rigorous distributional and methodological alternatives. Importantly, Harris is not concerned with the concept of a 'best' grammatical structure for language: he aims to build descriptively accurate structures for natural language, but argues that there can be many such structures for any given language (or corpus) – structures which he describes as linguistically identical but logically different.

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[225]Hymes and Fought, *op. cit.*, p 142.
[226]The distribution of a linguistic element is the set of all environments in which that element occurs.
[227]Zellig S. Harris, *Structural Linguistics* (Chicago: The University of Chicago Press, 1951), p 198.
[228]*Idem.*
[229]*Idem*, p 13.

While *Methods in Structural Linguistics* focuses on phonology and morphology, syntax is by no means absent from the book. Harris' syntactic methodology is the reverse of typical constituency analysis: he does not proceed "by first dividing utterances into large syntactic sections and subdividing these into smaller morphologic ones"; rather, he "begins with morphemes, investigates their syntactic function, and builds up from them to ever larger morpheme sequences having identical syntactic status".[230] This textbook paints syntax as an intimate part of the Bloomfieldian program, and as the final step in the Descriptivist study of language. This wedding of constituency sentence analysis and Bloomfieldian philosophy would, as we will see, gradually loosen over the next two decades.

Designed for an introductory course at the Hartford Seminary Foundation, H.A. Gleason's 1955 textbook *An Introduction to Descriptive Linguistics* (Henry Holt & Co.) was developed "primarily as preparation for the language problems faced by new missionaries in the foreign field, but also [for] students who are starting preparation for specific linguistic work – analysis, translation, teaching, reading education, etc.".[231] In this mid-decade text, syntax is given a prime place of importance. For Gleason – who, in 1955, had not yet committed himself to stratificational grammar – the process of syntactic analysis "is largely one of finding successive layers of ics [immediate constituents and describing] the relationships which exist between ics" – an analysis which, akin to Harris, is based on substitution.[232] Importantly, Gleason considers stress and intonation patterns to be essential parts of syntactic analysis (a theoretical approach known as *phonologic syntax*).[233] "Syntax has long been one of the least satisfactorily handled aspects of the structure of languages", he argues, because of the "failure to give sufficient attention to stress and intonation or to the equivalent features of the language concerned".[234] Particularly interesting is Gleason's appeal to these suprasegmentals to handle syntactically ambiguous constructions. Ambiguity, he writes in agreement with Wells, "rests in different ways of organizing the

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[230]*Idem*, p 262.

[231]Gleason Jr, *Descriptive Linguistics*, p vi.

[232]*Idem*, p 133.

[233]In the 1950s, phonologic syntax was promoted by, among others, Henry Smith, Archibald Hill, and George Trager. It fell from fashion after the Second Texas Conference.

[234]Gleason Jr, *Descriptive Linguistics*, p 139.

words into constituents" – but these ambiguities, he continues, are chiefly restricted to the written form since "in speech these relationships are clearly marked by stress and intonation".[235] In spoken language, he concludes, "stress and pitch contours are additional ics of constructions in which they occur" and, indeed, necessary to a full syntactic analysis.[236] This emphasis on suprasegmentals is a holdover from the Bloomfieldian years and, by the 1960s, would be largely dropped by constituency grammarians. As rival syntactic theories came on the scene, research questions became more specific and more technical, forcing supporters of ICG to move away from their commitment to suprasegmental features and to adopt new methods of explanation (cf. Chapter 3).

Constituency grammar took a large step away from the Bloomfieldian paradigm with the publication of Charles Hockett's 1958 textbook *A Course in Modern Linguistics* (The Macmillan Company), intended for university-level introductory linguistics courses. As the last major ICG work written before Chomsky's transformational grammar became well-known, Hockett's text represents the culmination of constituency theory as the dominant syntactic paradigm in America. The text includes lengthy, detailed discussions of syntax, and provides a constituency analysis of major constructions from conjunctions to prepositional phrases. Hockett's presentation of syntax balances on the threshold between Descriptivism and the new emerging approach to linguistics. Like Gleason, he considers intonation as a potential constituent, and argues that suprasegmentals are essential to determining constituent cut location. However, Hockett distances himself from the Bloomfieldian program in two respects: first, he continues his rejection of the Descriptivist views of mental constructs, methodology, and meaning which he first enunciated in his 1955 book *A Manual of Phonology* (Waverly Press). This manual, a survey of phonological theory and methods based on immediate constituency principles, argues that since language structure is not directly observable, all non-arbitrary human behavior should be admissible as linguistic evidence – including speech, observation of a subject's behavior, and intuition about language. Hockett further assumes that humans are equipped with a neural apparatus responsible

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[235] *Idem*, p 138.
[236] *Idem*.

for language and argues that the ultimate aim of linguistics should be to understand the functioning of this apparatus – a philosophical stance at odds with Bloomfieldian tenets. Second, Hockett rejects the Bloomfieldian quest for a mechanistic solution to language, arguing that linguists must empathize with their subject matter: since "we know of no set of procedures by which a Martian, or a machine, could analyze a [linguistic] system", he argues, linguists must bring human qualities to their study.[237] Hockett's presentation of constituency theory in a new philosophical framework represents the first of many major modifications made by constituency grammarians in the late 1950s and 1960s.

As rival syntactic theories emerged in the late 1950s, their proponents accused constituency grammarians of not being able to adequately handle problematic syntactic constructions such as ambiguities, conjunctions, active-passive pairs, and discontinuities. As these constructions gained a central place in linguistic explanation (cf. Chapter 3), constituency grammarians felt pressured to concentrate their efforts on technical questions. By the early 1960s it was clear that in order to remain a viable theory, ICG would have to be modified to enable it to capture constructions which were troublesome under traditional constituency assumptions. The result was a plethora of technical modifications which aimed to both maintain the fundamentals of constituency grammar and increase the analytic power of this approach. An early example of such a modification is Hockett's conjunction proposition, put forward in his 1954 article *Two Models of Grammatical Description* (*Word*). The conjunction construction is epitomized in phrases such as *apples and oranges*, or, as we will represent it, A CONJ B. Through the 1940s and 1950s, there was general consensus among linguists that constituency analysis should involve only binary cuts. This was particularly problematic for conjunctions, where neither of the possible binary cuts (A | CONJ B and A CONJ | B) captured the intuitive belief that A and B ought to be on the same level. Hockett's solution was to identify the conjunctive element (in this case, *and*) as a flag, or marker. The conjunctive element is thus not part of a constituent *per se*, but rather acts as a "marker of construction in which nearby forms stand".[238] This solution can clearly

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[237]Charles F. Hockett, *A Manual of Phonology* (Baltimore: Waverly Press, Inc., 1955).
[238]Charles F. Hockett, "Two Models of Grammatical Description," *Word* 10 (1954), p 214.

be extended to cover related problematic constructions including triple conjunctions and or-constructions. Through the 1960s, as constituency grammarians were forced to improve their theory in order to refute the accusations that ICG could not adequately handle syntactic analysis, such modifications to basic constituency theory became increasingly common. However, these modifications failed to impress opponents of ICG, who likened them to "astronomical epicycles" and described them as "chewing-gum and bailing-wire repairs on a failing system".[239] This dispute is considered in detail in Chapter 3.

Through the 1960s, ICG was criticized not only for its technical deficiencies, but also for its association with the Bloomfieldian philosophical framework. This second identification was, however, misrepresentative: as our survey of textbooks shows, while constituency theory formed an intimate part of Bloomfieldian linguistics in the 1940s and 1950s, it gradually lost this association and, by the 1960s, there was a clearly identifiable school of constituency theory which accepted the technical basis of ICG but was no longer wedded to the older philosophy. The constituency theory of the 1960s was independent of behaviorism and anti-mentalism; it had emerged as a technical approach to syntax compliant with the new philosophical norms of the era. Still, opponents of constituency theory continued to conflate that theory and Bloomfieldian philosophy, and to argue that constituency analysis was inherently flawed by its behaviorist underpinnings. This argument – based more on rhetoric than on fact – was particularly effective at winning adherents to transformational grammar (cf. Chapter 3).

Immediate Constituency Grammar: Development and influence

For over a decade after World War II, ICG was the syntactic theory of choice for linguists, anthropologists, and machine translators alike. It dominated textbooks and university-level linguistics training, and was an integral part of doctoral dissertations. It was particularly valued for its descriptive capacities, and was seen to "provide[] the investigator with a comprehensive system which he may apply to all constructions, and by which he can determine

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[239]Gilbert H. Harman (ed.), *Personal Communication with Janet Martin-Nielsen*, Gleason Jr, *Theories in Conflict*, p 40.

all the pertinent data".[240] As rival syntactic theories came into their own in the early 1960s, they attracted a large following of young linguists – those working on their doctorates and those beginning their academic careers. The older generation, including Archibald Hill, Charles Hockett, and Robert Hall, vocally rejected transformational grammar, calling its ascendance in the 1960s "tragic".[241] This generation bemoaned the separation of anthropology and linguistics which followed from the increased emphasis on formalization in transformational grammar. "[L]inguistics without anthropology is sterile; anthropology without linguistics is blind", wrote Hockett in his autobiographical contribution to *First Person Singular* – a quip which captures the reaction of a generation which had been trained to see the study of language as an integral part of the study of culture, and which had devoted much time and effort to recording and analyzing dying Amerindian languages. Trained in anthropology and linguistics at Yale in the late 1930s, Hockett is responsible for early linguistic work on the Potowatomi language. The new linguistics, he argued, was too quick to ignore what had come before and to reject the work of the 1940s and 1950s outright: "We have currently", Hockett wrote in 1980, "in our ranks a large number of young people, many of them very bright, from beginning students up to and including a few full professors, who know nothing of what happened in linguistics before 1957, and who actually believe (some of them) that nothing did happen".[242] Robert Hall added to these concerns a rejection of "all the accompanying unscientific Chomskyan dogmas, such as the innateness of language structure, the subjection of linguistics to psychology and philosophy, and the existence of 'deep structure' ".[243] Sustained through a series of articles and books over several decades, Hall's scathing criticism of transformational grammar represents one of the more extreme reactions from the older generation of American linguists. However, while this dissenting generation worked with and contributed to ICG through the 1950s, they did not all retain their commitment to constituency theory in the 1960s. Hockett and Gleason, both of whom were adamantly opposed to transformational grammar and both of whom

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[240] Eugene A. Nida, "The Analysis of Grammatical Constituents," *Language* 24/2 (1948), p 177.
[241] Hockett, *Preserving the Heritage*, p 105.
[242] *Idem.*
[243] Hall Jr, *Layers of Linguistics*, p 184.

had in the 1950s been fully supportive of ICG, turned to stratificational grammar.[244] Others continued to use constituency theory, but focused on problems far removed from the syntax of the 1960s. Dwight Bolinger, for example, applied constituency theory to intonation, and Hill to studies of literature. Thus while the older generation by and large did not accept transformational theory, neither did they remain wholly committed to constituency grammar.

Those who continued to support constituency grammar through the 1960s formed a diverse and disperse community of machine translators, literaturists, fieldworkers, missionaries, and others who valued the descriptive capacities and simplicity of the theory. Importantly, constituency theory had no equivalent to the support structures provided for transformational and stratificational grammars by Chomsky at MIT and Lamb at Yale, respectively. While these theories enjoyed institutional support and had clearly identifiable leaders and centers of promotion, constituency grammar had no clear location for the gathering of graduate students and adherents, no strong leadership, and no identifiable point for the emanation of ideas. As linguistics departments became increasingly common at American universities through the 1960s, supporters of ICG – who were spread over a variety of disciplines from anthropology to machine translation – became separated from the academic linguistic mainstream. While key to the anthropological linguist's toolbox in the 1950s, in the following decade the tools provided by constituency analysis had lost their importance in academic linguistics *per se*. By the end of the 1960s, when American linguistics had grown into a formalized and theoretically-oriented discipline, the descriptively-oriented constituency theory and its supporters became seen "not as an adversary but as an antiquity".[245] The marginalization of ICG from the American academic linguistic scene is discussed in detail in Chapter 6.

The advent of the 1960s saw the emergence of three types of work in constituency grammar: those which made full use of constituency theory (that is, those which assumed that

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[244]Hockett's commitment to stratificational grammar ended in 1968; Gleason maintained his commitment until the end of his career.

[245]Bolinger, *First Person*, p 38.

basic ICG provided an adequate syntactic theory for linguistic research), those which intro-
duced technical modifications to ICG in order to improve the technical capabilities of the
theory, and those which used constituency theory for particular applications but did not
necessarily consider the theory adequate on a broader scale. The latter two of these types
greatly overshadowed the first, and marked a significant change from the 1950s, when con-
stituency theory had been regularly and often unquestioningly used for language descrip-
tion and analysis.

<center>* * *</center>

The most intellectually stimulating work on constituency theory in the 1960s occurred in
machine translation. In an era when transformational grammar seemed to eclipse ICG on
so many fronts, machine translation stood out as an area in which constituency grammar
maintained a high level of value. Through the 1960s, machine translation groups at MIT, the
University of Texas at Austin, Georgetown University, and other institutions chose consti-
tuency-based grammars for their work. At MIT, Gilbert Harman's *Generative Grammars With-
out Transformation Rules: A defense of phrase structure*, published in *Language* in 1963, set
the stage for machine translators to adopt constituency theory.[246] Written while Harman
worked in the machine translation group at MIT's Research Laboratory of Electronics, the
article defends constituency grammar against the criticisms put forward by Chomsky. The
chief technical criticism of ICG, elaborated in *Syntactic Structures* and *Aspects of the Theory
of Syntax*, was that constituency grammars could not handle problematic syntactic con-
structions such as active-passive pairs, ambiguities, and discontinuities. Whereas TG linked
active-passive pairs with a common deep structure, transformational grammarians argued
that constituency theory provided no such relationship and hence treated this construc-
tion with "serious loss of insight and naturalness".[247] Similarly, while TG accounted for the
two different semantic interpretations of sentences such as *flying planes can be dangerous* by
attributing to each a separate kernel sentence (*planes fly* and *someone flies planes*), transforma-

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[246]A similar version of this article, authored by Harman and Victor Yngve, appeared in the machine trans-
lation section of the Research Laboratory of Electronics Quarterly Progress Report in 1963.

[247]Robert P. Stockwell, "The Transformational Model of Generative or Predictive Grammar," in Paul L.
Garvin (ed.), *Natural Language and the Computer* (New York: McGraw-Hill Book Company, Inc., 1963), p 26.

tional grammarians argued that constituency theory had no mechanism for characterizing this type of ambiguity. Harman aimed to prove this argument incorrect by developing a modified constituency grammar which "generates exactly the set of sentences generated by a fairly large transformational grammar written by Noam Chomsky".[248] His modified grammar, presented in the 1963 article, was designed to handle discontinuous constituents and was motivated by the work of Victor Yngve, the leader of the MIT machine translation group. Harman concluded that modified constituency grammars can do all the work of transformational grammars, and hence that TG has "no advantage" in the syntactic arena.[249]

Harman's work motivated Wayne Tosh's machine translation team at the government-funded University of Texas at Austin Linguistic Research Center to develop their own modifications of constituency grammars. Founded in 1961 by historical linguist Winfred Lehmann (1916–2007), who also played a vital part in the creation of the University of Texas at Austin's linguistics department, the Linguistic Research Center was a sister project to the machine translation group at Georgetown. The division of labor was along language lines: while Tosh and his colleagues worked on German-to-English translation, the Georgetown group worked on Russian-to-English translation. At the Linguistic Research Center, Tosh developed a modified constituency grammar which was able to handle tricky syntactic phenomena including agreement and government.[250] Despite the adoption of constituency grammar by machine translators – even in the unlikely milieu of MIT, where transformational grammar reigned – this work made little impact on the academic linguistic scene (cf. Chapter 6).

Harman took a strong stance on constituency grammar, arguing that modified constituency theories were fully capable of matching the explanatory power of rival theories and were "at least as well motivated as the transformational [theory]".[251] If we can label Harman's argument the *strong ICG argument*, then in the 1960s a significant number of

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[248]Gilbert H. Harman, "Generative Grammars without Transformation Rules: A defense of phrase structure," *Language* 39/4 (1963), p 597.

[249]*Idem*, p 597–598.

[250]Steven B. Smith, "Review of Wayne Tosh's Syntactic Translation (1965)," *Foundations of Language* 5 (1969), Wayne Tosh, *Syntactic Translation* (The Hague: Mouton, 1965).

[251]Harman, *Defense of Phrase Structure*, p 610.

linguists put forward the *weak ICG argument* – that while constituency grammar cannot rival transformational grammar as a broad approach to syntax, it has particular advantages which make it more valuable than other theories in certain restricted situations. The theoretical simplicity of ICG contrasted it from transformational and stratificational grammars, and made it especially useful for initial language analysis, whether in the field, the research lab, or the classroom. Constituency theory was used by language teachers in elementary and high schools, by fieldworkers confronted with unknown languages, by missionaries working with unwritten languages, and by students grasping to gain insight into new languages. While these various applications sustained the weak ICG argument through the 1960s, they had little impact on the theory-choice debates of that decade – a situation explored in detail in Chapter 6. As a result, by the end of the 1960s constituency theory controlled only a small share of the American academic linguistics market – a situation much changed from the previous decades.

<p style="text-align:center">* * *</p>

As the 1960s opened, three syntactic theories were competing for the attention of American linguists: constituency, stratificational, and transformational grammars. As we have seen, these theories all focused on the sentence structure of natural language – but they had significantly different socio-professional followings, they were motivated by different interests and priorities, and they used different syntactic tools and analytic procedures. By the end of the decade, transformational grammar had captured the attention of the majority of academic syntacticians, and stratificational and constituency grammars were left far behind. The following chapters aim to understand the syntactic theory-choice debates of the 1960s – and, specifically, to account for the rise of transformational grammar. We begin by exploring the concept of explanation in Chapter 3.

3   Conditions of Explanation

The aim of linguistics is to have an output of some interest, and everything hinges on how you define what is interesting. This is the area where it is hardest to reach any agreement whatsoever.

    Paul Garvin, at the 1962 Georgetown Round Table Meeting on Linguistics and Language Study[1]

[I]n the actual, workaday world, linguists tend to evaluate each other's hypotheses through a screen of their own preconceptions, which allow them to derive inferences about those hypotheses that would not otherwise be available.

    John Goldsmith and Geoffrey Huck, in *Historiographia Linguistica* (1998)[2]

The postwar decades brought change to American linguistics on many fronts, from the break with anthropology to the rise of syntax to the establishment of university linguistics departments. In this chapter, we explore an internally-focused aspect of this change – one which had far-reaching consequences for the theory-choice debates of the 1960s: conditions of explanation. The two main questions at stake are, firstly, what counts as explanation in linguistics? and, secondly, how is this decided?

As well as proposing theoretical frameworks and technical tools for language analysis, the three linguistic theories of interest to this study also enunciated, implicitly and explicitly, explanatory criteria for syntax. These are the conditions which, according to a given

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[1] Eric P. Hamp et al., "The Transformation Theory: Advantages and disadvantages (Panel I)," in Elisabeth D. Woodworth and Robert J. Di Pietro (eds.), *Report of the Thirteenth Annual Round Table Meeting on Linguistics and Language Study, 1962* (Washington DC: Georgetown University Press, 1962), p 20.

[2] Huck and Goldsmith, *On Comparing Linguistic Theories*, p 347.

school of thought, any syntactic theory must meet in order to be potentially capable of explaining sentence structure. These criteria – from accounting for the relationship between active-passive sentence pairs to explaining communicative devices to conforming to current neurophysiological views of the brain – were, importantly, set out by the working linguists of the era. That is, they are not the after-products of philosophers, historians, or commentators, but the time-of-inquiry results of discipline-internal priority setting. It is on these internal criteria that the theory-choice debates hinged: to convince a young linguist to adopt a particular syntactic theory, or to convince an established linguist to change allegiance from one theory to another, that linguist had to be persuaded first that the new theory's explanatory criteria are worth pursuing, and second that the theory is in principle capable of fulfilling these criteria. The decision to commit to a syntactic theory comes primarily from belief in that theory's potential ability to explain syntactic structure, language, and related elements of the world at large. As such, the theory able to dominate in the explanatory arena is best poised to gain and retain supporters.

In this chapter, I argue that transformational grammarians were successful in setting the dominant explanatory criteria for American linguistics in the 1960s. Beginning with the publication of *Syntactic Structures* in 1957, transformational theory raised compelling questions about language and set the stakes for linguistic explanation high. By bringing problematic data constructions, formalization, language creativity, and psychological validity to the forefront, Chomsky's program fundamentally changed the conception of explanation in syntax. Transformationalists placed their explanatory criteria center stage at conferences, in writing, and in the classroom, where they forcefully and repeatedly criticized rival theories for not meeting these criteria. Rivals were forced to respond. Soon, stratificational and constituency grammarians were devoting as much time and effort, if not more, to fitting their theories to the transformational explanatory criteria as they were to advancing their own explanatory priorities. By successfully naming the conditions for explanation in 1960s syntax, transformationalists provided their own supporters with highly significant questions to pursue and, at the same time, took energy and momentum away from rival

theories. This monopoly over explanatory criteria was central to the dominant position transformational grammar established in the American academic linguistics community.

The theory-choice debates of the 1960s centered on five key questions related to explanation, which will be explored in depth in this chapter: (1) What syntactic data should linguistic theories account for, and what restrictions must theories adhere to when accounting for this data? (2) To what extent should explanation and formalization be linked in linguistics? (3) What conditions must the power of linguistic theories meet, and how can this power be measured? (4) What import do external validity and applicability have for syntactic theories? (5) To what extent, if any, should linguistic theories aim to be psychologically valid?

The chapter opens with an historical overview of explanation in linguistics from the late 19th century to the mid-20th century (section 3.1). This is followed by remarks on the historian's task, which set the stage for my approach to explanatory concepts in the 1960s. Section 3.2 explores the treatment of syntactic data by constituency, transformational, and stratificational grammarians. I argue that transformational grammarians used a combination of self-promotion and unrelenting criticism of rivals to gain a monopoly on explanatory data criteria. They were able to attract young linguists with interesting problems, emphasize data areas in which transformational theory was strong, and direct the efforts of rival theories away from their own data priorities. Section 3.3 discusses the rise of formalization in 1960s linguistics, including the need for precise theoretical statements and the search for simplicity metrics. I show that while constituency and stratificational grammarians challenged TG in these areas, still transformationalists maintained control over explanatory criteria. Section 3.4 investigates the efforts of transformational grammarians to prove conclusively the inadequacy of immediate constituency grammar. While this work was turned on its head in the early 1970s, it enjoyed immense success through the 1960s, and was vital to building a base of support for transformational grammar. Section 3.5 shifts attention away from technical manifestations of explanation and towards external validity. By investigating computerizability, machine translation, and psychological validity,

I show that stratificational grammar placed high explanatory value on external validity –
a decision which would, in the end, be detrimental to that theory's ability to attract large
numbers of students. I conclude that transformational grammarians successfully monopo-
lized explanatory criteria through the 1960s. In doing so, they both created a momentum of
progress which was essential to attracting students, and detracted from the energy, efforts,
and priorities of rival theories.

3.1   Explanation in Linguistics: An historical perspective

Changing concepts of explanation have long had great impact on linguistics – and none
more so than the rise of synchronic linguistics at the turn of the 20th century. In the 19th
century, the study of language comprised three traditions: diachronic and historical work,
prescriptivism, and philology. The first conceived of linguistics as the study of families of
related language systems evolving through time. Through historical comparisons of lan-
guages, this school aimed to establish linguistic genealogy and identify proto-languages
(that is, the oldest common ancestor of a language family). At the helm of this movement
was German linguist Franz Bopp (1791–1867), who aimed to build genealogical language
study into a systematic science which could rigorously determine the historical origins of
Indo-European languages. Bopp's main work, *On Sanskrit* (1816), set this program in mo-
tion by exploring the relationships between the primary languages of interest in the era
– Latin, Greek, Germanic languages (including, among others, English and German), and
Sanskrit.[3] Within this framework, linguistic explanation meant accounting for language el-
ements and patterns in terms of the historic evolution of languages. Consider, for example,
the patterning of Sanskrit *ganas*, Latin *genus*, and Greek *genos*, as shown below:[4]

Sanskrit: *ganas, ganasas, ganasi, ganassu, ganasam,...*

Greek: *genos, geneos, genei, genea, geneon,...*

Latin: *genus, generis, genere, genera, generum,...*

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[3]Franz Bopp, *Analytical Comparison of the Sanskrit, Greek, Latin, and Teutonic Languages, shewing the original identity of their grammatical structure* (Amsterdam: Benjamins, 1974).

[4]Data from Ferdinand de Saussure, *Course in General Linguistics* (Chicago: Open Court, 2007), p 2.

Assuming that the Sanskrit represents the primitive form, the diachronic-historical explanation of this data asserts that the Greek language dropped *s* whenever it occurred between vowels near the end of a word, and the Latin language converted *s* to *r* in the same circumstances.

The second type of language study in the 19th century, prescriptive linguistics, is often associated with schoolbook grammars. It aimed to prescribe proper ways of speaking and writing by distinguishing correct from incorrect language and codifying these distinctions through grammatical rules. Classic prescriptivist teachings on English, for example, identify double negation (*I don't have no money*) and less-for-fewer substitutions (*there were less attendees than expected at the office party this year*) as grammatically incorrect. Finally, philology refers to the study of texts, and specifically to the critical interpretation of and commentary on texts. Philologists aim to reconstruct original texts based on extant copies and translations, and to provide historical accounts of manuscript evolution through the ages, including identification of original content, modifications, editions, and secondary authors. Most prominently, philologists have devoted massive effort to reconstructing the original textual content of works ranging from the Bible to Euclid's *Elements*.

With the turn of the 20th century came a new conception of linguistics, and with it a new conception of explanation. Teaching at the University of Geneva in the early 1900s, Swiss linguist Ferdinand de Saussure (1857–1913) rejected the tripartite 19th century conception of linguistics and recast the subject as the study of the structure of a given language at a single point in time, as informed by a community of speakers. Known as synchronic, or static, linguistics, Saussure's program focused on structural manifestations of contrasting language elements (for example, the contrast between singular and plural forms, or between nominative and accusative forms). The primary aim of linguistics, as stated in Saussure's *Course in General Linguistics*, was to "determine the forces operating permanently and universally in all languages, and to formulate general laws which account for all particular linguistic phenomena historically attested".[5] Reconstructed from student notes, Saus-

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[5]Saussure, *op. cit.*, p 6.

sure's *Course* was published posthumously in 1913. Under Saussure's influence, linguistics became comparative, not historical; it focused on speech, not writing or texts; and it was descriptive, not prescriptive. In this new paradigm, linguistic explanation meant accounting for language contrasts in terms of structural considerations, with no reference to historical change or language genealogy. Saussurian linguistics, for example, explains the American English pronunciation of a flap in the word *Italy* but not the word *Italian* by means of the realization of allophones in structurally determined environments.[6] University of Chicago linguist John Goldsmith calls this linguistics' "greatest contribution": the elaboration of a data analysis methodology "which was both rigorous and sensitive to details at the human level, and which analyzed language in an ahistorical fashion".[7] It was this new conception of explanation, and the subsequent rise of phonemic analysis in America, which set the stage for the linguistics of the 1960s of interest to this study.

Only a few decades after the rise of synchronic linguistics and the establishment of the Bloomfieldian program in America came another significant shift in explanatory priorities: the postwar years saw linguists engage in a dialogue about the role of psychological validity in their discipline. At opposite ends of the spectrum were two philosophical approaches to the relationship between language and the brain: at one end, linguists aimed to describe language data without positing any claims about psychological correlation, and, at the other, linguists aimed to develop explanatorily adequate theories which provided psychologically valid insight into the human language capacity. As noted in chapter 2, this debate reached its climax in the 1950s when Zellig Harris asserted that there is no single best grammar for any given dataset or language, but rather many equally correct and descriptively accurate grammars, and Charles Hockett argued that linguistic theories should ultimately aim to explain the human neural apparatus, and hence that for any given dataset or language a single best grammar could be chosen on the basis of adherence to known psychological facts. This debate echoed the contemporaneous dialogue in the American psychological community concerning the validity of behaviorism, and presented linguists

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[6]Example from John A. Goldsmith, *From Algorithms to Generative Grammar and Back Again*.
[7]Goldsmith, *From Algorithms to Generative Grammar*.

with stark choices in terms of philosophical and theoretical commitments. Speaking at the 1956 Georgetown Round Table Meeting, Fred Householder famously used the terms *God's truth* and *hocus-pocus* to describe these two positions: "The hocus-pocus view professes to believe that where there is a high degree of agreement in the use of terms and apparently successful communication, this is due entirely to chance, coincidence and conditioning, not to any external reality or naturalness of the class related by the term. The naïve realist or God's truth partisan, on the other hand, takes the argument from consensus or success of communication as convincing. He may even go further, however, and prescribe that the linguist, for instance, should never set up any arbitrary classes or make any decisions not based on objective, agreed-on similarities and differences".[8] Householder's terms caught on among American linguists, and soon gained stronger associations. Hocus-pocus came to refer to linguistic analyses which applied inductive processes to fit language data into preconceived hierarchies of elements and levels, while God's truth was attached to belief in the existence of unique, psychologically-correct analyses of language. Within a year of Householder's speech, Chomsky – through his publication of *Syntactic Structures* – would press the case for God's truth and establish psychological validity as a key explanatory criterion for transformational grammar. In the following decade, psychological validity became increasingly important to American linguists, but was interpreted and implemented differently by different schools of thought. As we will see in this chapter, while transformational and stratificational grammarians both held improved knowledge of the brain as an ultimate goal, stratificationalists would insist on a richer interpretation of this goal. Choice between theories, then, was not simply choice between technical linguistic tools, but also choice between different beliefs in the relationship between language, linguistics, and the human brain.

<div align="center">* * *</div>

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[8]Fred W. Householder, "Rough Justice in Linguistics," in Paul L. Garvin (ed.), *Report of the Seventh Annual Round Table Meeting on Linguistics and Language Study, 1956* (Washington DC: Georgetown University Press, 1957), p 153.

It is easy to think of any number of ideas, or concepts, which a syntactic theory might be expected to explain: the loss of syntax capability following an aphasia, the ability of humans to invent and popularize new idioms and metaphors, or commonalities between the sentence structure of related dialects and languages. The historian's task, however, as I see it, is to avoid imposing explanatory criteria on the theories under study. In order to understand theory-choice debates in linguistics or in any social or natural science, we need to look at explanation from the point of view of working scientists in the period of study. Otherwise, we risk distorting our historical analysis by imposing our own interests, values, and hindsight-rich priorities. Such imposition is especially evident in the University of Wisconsin-Milwaukee's Edith Moravcsik's 1980 commentary on explanation in linguistics. In her discussion of the 1979 *Current Approaches to Syntax* conference, held in Milwaukee, Moravcsik asserts that the most important criterion for comparing syntactic theories is "truth, or empirical adequacy", followed by "fruitfulness, or instrumentality, or external validity".[9] Truth, or empirical adequacy, however, is a broad label for what is in reality a complicated and diverse set of ideas, encompassing choices about data methodology, psychological validity, and human association with language. More importantly, it is a criterion which needs to be redefined for each syntactic theory at stake, since each theory has its own perspective on truth in the context of language and of linguistic study. By identifying this criterion as "tak[ing] precedence over all others", Moravcsik implicitly rejects any syntactic theory which does not place high explanatory value on truth.[10] By choosing explanatory criteria not from the point of view of working linguists, but from considerations of broad intellectual interests, Moravcsik fails to recognize the theory- and school-specific role of explanation.

The arguments presented in this chapter, and throughout this study, explicitly take into account the explanatory priorities set by linguists themselves: for example, the importance of discourse and idiom structure to stratificational grammarians, the centrality of specific

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[9] Edith A. Moravcsik, "Introduction: On syntactic approaches," in Edith A. Moravcsik and Jessica R. Wirth (eds.), *Current Approaches to Syntax* (New York: Academic Press, 1980), p 16.

[10] *Idem.*

datasets for transformational grammar, and the various manifestations of external validity across schools of syntactic thought. Explanatory criteria are not set after the fact; they are set by the scientists of a discipline at the time of their work. These criteria may be – and frequently are – affected and shaped by outside forces from funding bodies to ethical norms to philosophical considerations, but these too are an element of the times. With this in mind, I will not compare theories on empirical grounds – that is, I will not argue that syntactic theory A is better able to handle some construction or syntactic phenomenon than theory B. Rather, I am investigating the relative ability of theories to build bases of support, recruit students, and, ultimately, secure a portion of the American linguistics market. In doing so, it is critical to study the explanatory criteria valued by working linguists: this allows us to understand discipline-internal value choices and priority-setting. In this light, this chapter aims to provide a rich and historically-informed understanding of syntactic explanation in the 1960s by looking at the dynamics of explanatory criteria from the point of view of that decade.

3.2 Data

> [I]t must be emphasized that gross coverage of the facts (i.e., success in gen-
> erating actual sentences) is not a sufficient goal for linguistic description. [...]
> In addition to covering the facts, linguistic description must seek to reveal the
> high level of regularity, uniformity, and patterning that characterizes natural
> languages, and reveal it in a way that conforms with the intuition of speakers
> of the language.
>
> Robert Stockwell, in *The Transformational Model of Generative or Predictive Grammar* (1963)[11]

Thus a grammar of English should account for such facts as the following: 1. *John eats meat* is a sentence, *eats meat John* is not. 2. *They are flying planes* is ambiguous. 3. *John eats meat* and *meat is eaten by John* are closely related.

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[11]Stockwell, *The Transformational Model*, p 27.

> Paul Postal, at the 1962 Georgetown Round Table Meeting on Linguistics
> and Language Study[12]

> A stratificational grammar can provide a simple and natural treatment for [...]
> many of the grammatical features extending beyond the sentence.

> H.A. Gleason, at the 1964 Georgetown Round Table Meeting on Linguistics
> and Language Study[13]

"Given a large mass of data", Frederick Grinnell quotes in his recent *Everyday Practice of Science*, "we can by judicious selection construct perfectly plausible unassailable theories – all of which, some of which, or none of which may be right".[14] Data is commonly seen as the bread and butter of science: the observations, measurements, and experimental results on which theories are built. Considered by some to be objective and unquestionable, and by others to be manipulable and fallible, data plays a central role in scientific theory-construction – and in the historical analysis of those theories. In this section, I explore the treatment of syntactic data by constituency, stratificational, and transformational grammarians in the 1960s.

The basic data of syntax is a set of sentences (or, more precisely, a set of linear sequences of sounds) from one or more natural languages, and can be presented in audio, visual, or symbolic form in a variety of ways. As in other sciences, linguists do not aim to deal with all syntax data equally; rather, they identify various syntactic phenomena which it is essential for any proper theory to account for. That is, some syntactic phenomena carry more weight than others, and a theory incapable of accounting for highly-valued phenomena is considered highly deficient. Just as physicists agree that their theories do not always have to account for friction (for example, friction is often omitted from theories of planetary orbits since its effect will be negligible), linguists frequently agree that their theories can neglect low-priority syntactic phenomena. Importantly, the weighting of syntactic phenomena is neither universal nor timeless; it varies between theories, schools, and subschools, and is a

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[12]Hamp et al., *Transformational Theory (Panel I)*, p 3.

[13]Gleason Jr, *Organization of Language*, p 91.

[14]Frederick Grinnell, *Everyday Practice of Science: Where intuition and passion meet objectivity and logic* (Oxford: Oxford University Press, 2008), p 86.

central part of what the supporters of a theory see as explanation. This section investigates data priorities in 1960s American syntax, and the consequences of those priorities for the theory-choice debates of that decade.

The 1960s saw transformational and stratificational grammarians alike advance explanatory criteria for data – criteria which varied greatly between the schools. While transformationalists focused on a set of problematic sentence-level constructions I will call the TG *dataset* (this dataset includes, among other constructions, active-passive pairs and discontinuities), stratificationalists were interested in capturing supra-sentence phenomena, including metaphors and discourse style. In this section, I argue that transformational grammarians alone successfully universalized their explanatory criteria for data and, consequently, dictated the terms which any explanatory theory needed to meet. Forced to respond, rival syntactic theories had to prove themselves capable of accounting for the TG dataset as well as for their own data priorities. Through unrelenting criticism of the explanatory capacities of immediate constituency grammar, transformationalists forced proponents of ICG to defend themselves and modify their theory to explain the TG dataset. As a result, constituency grammarians spent the 1960s playing catch-up, and never managed to develop their own explanatory criteria for data. While stratificationalists did enunciate their own data priorities, these had little influence outside SG itself: supporters of rival theories looked down on supra-sentence phenomena as being of peripheral interest to syntax. By naming the criteria for data explanation, transformational grammarians both provided their own linguists with compelling problems to work on and severely constrained datawork in rival theories.

<div align="center">* * *</div>

The TG dataset comprised *problematic constructions* – a set of syntactic constructions which included active-passive pairs, coordination, discontinuities, nominalization, and constructional homonymity, chosen for the difficulties they had caused for constituency grammarians through the 1950s and for their amenability to transformational solutions. Importantly, these constructions were conceived and presented as language *patterns* which highlighted

correlations between sentence types – correlations, transformational grammarians argued, that linguistic theories were required to explain. In the late 1950s and early 1960s, the core transformationalist community at MIT presented transformational accounts of these patterns in monographs, doctoral dissertations, journal articles, and underground literature. Most prominently, Robert Lees devoted his doctoral studies to developing a sweeping transformational explanation of English nominalization. These accounts were immediately promoted as proving transformational grammar's ability to handle "a wealth of novel expressions" and became the showpiece of the transformational school.[15]

At the top of the transformational agenda was accounting for sets of sentences which were intuitively felt to be connected, or, for what Chomsky called "the obvious psychological fact that some pairs of sentences seem to be grammatically closely related".[16] Activepassive pairs led the list: a syntactic theory, Chomsky argued, must account for the fact that the sentence *John threw the ball* is "felt to be [...] a close relative" of the sentence *the ball was thrown by John* and, moreover, for the fact that the relationship between these two sentences is different from the relationship between *John threw the ball* and *who threw the ball?* or *did John throw the ball?*.[17] Transformational grammar explains these patterns by means of shared deep structures. A passive sentence, as explained in Chapter 2, is derived via transformations from the deep structure it shares with its active counterpart. Different transformations derive interrogatives from declaratives, and so on. This explanatory requirement quickly gained momentum in the linguistic community, and it was soon considered "obvious[]" that certain sentences bore intimate relationships to one another, and that these relationships required explanation.[18] By the early 1970s, one of the "major contributions" of *Syntactic Structures* was frequently identified as having "systematically described and explained [...] certain relations which were intuitively recognizable among certain sets of sentences".[19]

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[15]Lees, *Nominalizations*, p xvii.

[16]Chomsky and Miller, *Introduction to Formal Analysis*, p 296.

[17]*Idem.*

[18]Robert L. Allen, "Written English Is a 'Second Language'," *The English Journal* 55/6 (1966), p 744.

[19]Richard J. O'Brien (ed.), *Report of the Twenty-second Annual Round Table Meeting on Linguistics and Language Studies* (Washington DC: Georgetown University Press, 1971), p 76.

Transformational grammarians did not only provide solutions to problematic construc-
tions – they also continually criticized rival syntactic theories for not being able to handle
these patterns. Constituency grammar bore the brunt of this criticism, which was a main-
stay of the transformational polemic. A standard method of criticism was to identify sets
of rules capable of capturing problematic syntactic phenomena, and then argue that these
rules could not be incorporated into constituency grammars. The following rule, for ex-
ample, Chomsky argues, is necessary for explaining coordination because "it is easier to
state the distribution of 'and' by means of qualifications on this rule than to do so directly
without such a rule".[20] Here, necessity is equated with simplicity: this rule, Chomsky
continues, enables the grammar to be "enormously simplified".[21]

> COORDINATION RULE: If $S1$ and $S2$ are grammatical sentences, and $S1$ differs from
> $S2$ only in that X appears in $S1$ where Y appears in $S2$ (i.e., $S1 = ..X..$ and $S2 =$
> $..Y..$), and X and Y are constituents of the same type in $S1$ and $S2$, respectively,
> then $S3$ is a sentence, where $S3$ is the result of replacing X by $X + and + Y$ in
> $S1$ (i.e., $S3 = ..X + and + Y..$).[22]

While this rule is ideal for capturing coordination, Chomsky writes, "we cannot incorpo-
rate [it] or anything like it in a grammar of phrase structure, because of certain fundamental
limitations on such grammars".[23] The rule requires knowledge of the derivational history
of the sentences $S1$ and $S2$ (that is, knowledge of constituent type) – a knowledge which is
not available to phrase structure grammars. This rule can, however, be incorporated into
transformational grammars, which are able to appeal to derivational history. By present-
ing similar arguments for a wide variety of constructions from coordination to auxiliaries,
transformationalists built a body of evidence against constituency grammar while simul-
taneously highlighting the necessity of transformations.

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[20] Chomsky, *Syntactic Structures*, p 37.
[21] *Idem.*
[22] *Idem*, p 36.
[23] *Idem*, p 37. This approach to coordination has been widely criticised. The reader is directed to Simon C.
Dik, *Coordination: Its implications for the theory of general linguistics* (Amsterdam: North-Holland Publishing
Company, 1968).

With their highly-valued dataset in hand, transformational grammarians launched a twofold critique of the explanatory capacities of immediate constituency grammar. It is important to draw a distinction between these two parts, as they are often conflated in both 1960s-era literature and contemporary works. The first argument is that constituency grammarians were only able to *describe* language – that is, to segment and classify corpus data. The technical apparatus of constituency grammar, transformationalists argued, was not rich enough to explain complex phenomena such as intuitive connections between sentences. Secondly, transformational grammarians argued that the philosophical approach of constituency grammarians displayed no interest in explanatory accounts of data. Chomsky's portrayal of constituency grammarians as anti-mentalists with no regard for explanatory adequacy – a portrayal which, by the 1960s, was both misleading and misrepresentative – was central to his arguments against behaviorism which gained him fame both inside and outside of linguistics.

The essential technical flaw with constituency theory, transformationalists argued, is that it implicitly assumes that "every property essential to a language is characterizable on the basis of observable features of the *surface form* of its sentences" – an assumption which prevents the theory from accounting in principle for syntactic phenomena which necessitate a separation between deep and surface structures.[24] Syntactic theories which do not posit a separation of levels are, from the transformational perspective, incapable of explaining constructions such as active-passive pairs, which require two or more sentences to be linked on an abstract level. By "assum[ing] that each sentence is a unique event", wrote Curtis Hayes, constituency theory has no ability to "reveal the facts of sentence-relatedness".[25] Unlike the link provided in transformational grammar by deep structure, traditional constituency grammars generate each sentence independently of all others. Firmly believing that "the most important grammatical relations in sentences cannot be represented adequately in their surface structures", transformationalists were adamant that any syntactic

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[24]Jerrold J. Katz and Thomas G. Bever, "The Rise and Fall of Empiricism," in Thomas G. Bever, Jerrold J. Katz, and D. Terence Langendoen (eds.), *An Integrated Theory of Linguistic Ability* (New York: Thomas Y. Crowell Company, 1976), p 12, my emphasis.

[25]Hayes, *op. cit.*, p 96.

theory which generated surface forms directly would be fundamentally incapable of providing an explanatory account of language patterns.[26]

In order to achieve explanatory capability, Chomsky wrote in his contribution to the 1963 *Handbook of Mathematical Psychology*, a grammar must not only generate all and only the grammatical sentences of a given language, but also "associate with each of these sentences a structural description".[27] Sentence generation on its own is not sufficient: a syntactic theory must also make structurally-significant claims about those sentences. From the transformational perspective, the structural descriptions provided by ICG, or potentially provided by any constituency-type theory, had three flaws. First, since the structural descriptions failed to explain intuitive psychological associations between sentences, they did not "provide an account of the kinds of structural information available to the native".[28] Second, and related, by emphasizing "inventories of elements as over against rules", constituency grammars simply provide "sets of utterance classifications".[29] While these classifications can be made explicit, Paul Postal notes, "[t]he explicitness of [these] grammars is the explicitness of a library catalog and can provide no more explanatory insight".[30] This library metaphor runs throughout the transformational polemics of the 1960s, and is meant to portray ICG as an outdated, classificatory, and old-fashioned approach to language. Third, transformationalists argue that the complexity involved in explicit structural descriptions in the constituency paradigm is "truly extraordinary": in constituency grammars, Postal asserts, "many simply and easily discovered regularities [are] excluded, many essentially identical parts of the grammar [have] to be repeated several times, and so forth".[31]

Consider, for example, the sentence *this teacher's marks are very low.*[32] This sentence is

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[26]Thomas G. Bever, Jerrold J. Katz and D. Terence Langendoen (eds.), *An Integrated Theory of Linguistic Ability* (New York: Thomas Y. Crowell Company, 1976), p 2–3.

[27]Chomsky and Miller, *Introduction to Formal Analysis*, p 297.

[28]Paul M. Postal, "Limitations of Phrase Structure Grammars," in Jerry A. Fodor and Jerrold J. Katz (eds.), *The Structure of Language: Readings in the philosophy of language* (New Jersey: Prentice-Hall, Inc., 1964), p 144.

[29]Hamp et al., *Transformational Theory (Panel I)*, p 3.

[30]*Idem.*

[31]Postal, *Limitations of Phrase Structure Grammars*, p 145.

[32]Data from Lees, *Review of Chomsky*, p 386.

ambiguous with the two possible meanings *this teacher gives low marks to his students* and *this teacher gets low marks* (for example, in his Saturday-morning piano classes). This ambiguity, transformationalists argue, must be reflected in the structural description of the sentence. In traditional constituency theory, the only way of capturing such ambiguities is to assign each interpretation a different (that is, non-isomorphic) structural description (cf. Chapter 2, figure 2.18). In the case of the sentence above, however, Robert Lees argues, "[t]here is no reason [...] for assigning two different immediate constituent analyses" – and hence no way for constituency theory to explain the ambiguity.[33] The single structural description provided by constituency grammar for the sentence is shown in figure 3.1, in simplified form. Lees emphasizes that this is not an isolated instance; rather, "there are many cases of ambiguous sentences for which only a single [constituency] analysis seems justified".[34] Constituency theory is inherently weak at assigning structural descriptions, the transformationalists concluded, and hence could not provide explanatory accounts of syntactic data.
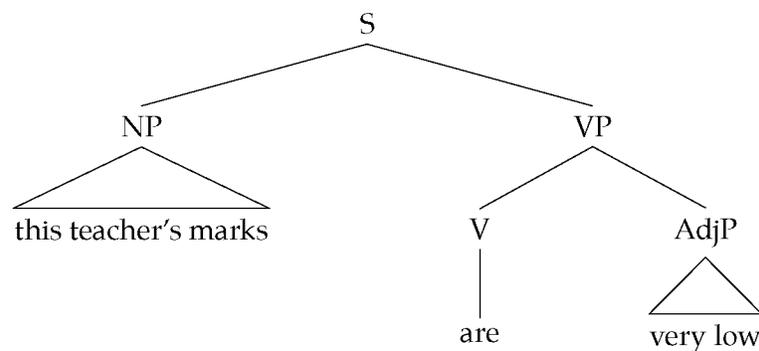
S Sentence
AdjP Adjective Phrase
NP Noun Phrase
V Verb
VP Verb Phrase

Figure 3.1: Immediate constituency analysis of *this teacher's marks are very low*

The failure of constituency theory, continued the transformationalist criticism, was not

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[33]Lees, *Review of Chomsky*, p 386.
[34]*Idem.*

limited to its technical apparatus, but extended to its philosophical framework. "The failure of a taxonomic theory to handle the full range of facts about linguistic structure", Jerrold Katz wrote in his 1964 article *Mentalism in Linguistics*, "is due to the failure of such theories to concern themselves with mental capacities, events, and processes".[35] By subscribing to mentalism, Katz continues, transformational grammarians "provide ourselves with the conceptual machinery that makes it possible to account for the full range of linguistic facts".[36] Here, transformationalists draw a direct link between philosophical stance and explanatory data capabilities: the rejection of mental constructs, they contend, entails the rejection of abstract, unobservable levels of linguistic structure – and hence the rejection of any multi-level view of language. Transformational grammarians concluded that the single-level conception of language inherent to constituency grammar prevents that theory from in principle accounting for problematic constructions and, further, that the constituency community's rejection of mentalism shows that the school has no interest in developing explanatory accounts for such constructions. As discussed in Chapter 2, however, this portrayal of the ICG philosophical stance was, by the 1960s, outdated and inaccurate.

The response of the constituency grammar community to these criticisms consisted of two parts, one more effective than the other. First, constituency grammarians tried to improve their data-handling capabilities by making technical modifications to constituency theory. Large numbers of linguists contributed to this effort in the early 1960s – linguists who were not convinced of the inadequacy of ICG and who were uncomfortable with the proposal for a transformational approach to syntax. "Many linguists appear to feel [...] that whatever flaws can be shown [in constituency theory] can be eliminated by further elaboration and refinement of the underlying immediate constituent notions", wrote an exasperated Postal in 1964.[37] However, this effort was spread between academic linguists, machine translators, fieldworkers, and missionaries, and lacked coordination and leadership.

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[35] Katz, *Mentalism in Linguistics*, p 127.
[36] *Idem.*
[37] Postal, *Limitations of Phrase Structure Grammars*, p 145.

Theoretical refinements were proposed in private papers and public journals, but there was never any organized effort to pull these refinements together into a cohesive program – and hence there was no common consensus or goals about how theory modifications should be made to work with one another. The response was nowhere near as effective and planned as the criticisms coming from the transformational school. Perhaps inevitably, transformationalists pounced on this decentralized effort, comparing the constituency modifications to astronomical epicycles – technical band-aids applied with no consideration for the overall effect. For each individual technical deficiency with constituency grammars, Chomsky asserted, "it is possible to devise some *ad hoc* adjustment that might circumvent it".[38] "Much to be preferred", he continued, "would be a conceptual revision that would succeed in avoiding the mass of these difficulties in a uniform way, while allowing the simple constituent-structure grammar to operate without essential alteration for the class of cases for which it is adequate and which initially motivated its development. As far as we know, the theory of transformational grammar is unique in holding out any hope that this end can be achieved".[39]

More comprehensive was the response led by Gilbert Harman, who worked at the MIT machine translation group in the early 1960s. Harman aimed to develop a modified constituency theory of language which avoided the technical problems identified by Chomsky. This was a systematic, planned effort designed to undermine the transformational criticisms of constituency grammar by introducing modifications to that theory in order to enable it to handle problematic constructions, without worrying in the first instance about the increasing complexity of the resultant grammar. For example, Harman argued that while Chomsky's phrase structure formalization did not allow for discontinuous constituents, constituency grammars can easily be equipped with a "neat formal apparatus for handling discontinuous elements" – an apparatus already devised by fellow machine translator Victor Yngve.[40] In this light, Harman continues, it is "pointless" to reject constituency

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[38] Chomsky and Miller, *Introduction to Formal Analysis*, p 299.
[39] *Idem.*
[40] Harman, *Defense of Phrase Structure*, p 604.

theory on the basis of a formalization which is not equipped to handle discontinuities.[41] Similarly, Harman argues that phrase structure grammars can be modified to allow deletions, which are essential for constructions such as *John bought a bag of oranges, and Alice a basket of apples*. With such modifications, he concludes, constituency principles can provide a syntactic theory fully capable of accounting for the TG dataset.

While Harman's modified constituency grammar received "favorable reactions from older more traditional linguists who were delighted with objections to Chomsky", it had little impact on young linguists, or on the transformationalists' view of constituency theory.[42] Trained in transformational theory at American universities (cf. Chapter 4), young linguists were unconvinced that constituency grammar could be built into a generalized, coherent, and explanatory account of syntactic structure. At the 1964 Linguistic Society of America Linguistic Institute, Chomsky rejected Harman's work outright, asserting that it failed to "assign to each sentence the correct deep and surface structure" necessary for an explanatory account of language.[43] Whether or not Harman's theory could generate all and only the grammatical sentences for a given language was irrelevant, Chomsky continued, since it could not motivate this generation by means of explanatory structural descriptions. James McCawley further criticized Harman for adding modifications with no regard for increasing complexity, and argued that Harman's grammar would, in the end, require "infinitely many rules".[44] As criticism continued to flow from the transformational school, linguists began to turn away from constituency theory. By the mid to late 1960s, the constituency grammar community in America was much reduced. Young linguists were choosing transformational grammar, and those who continued to support constituency theory had less and less of a voice in the mainstream academic linguistic community.

The debate between transformational and constituency grammarians over data was long and heated, and with far-reaching consequences. For transformationalists, it was a focused

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[41]*Idem*, p 605.

[42]Harman, *Personal Communication*.

[43]Noam Chomsky, *Topics in the Theory of Generative Grammar* (The Hague: Mouton & Co., 1966), p 48.

[44]James D. McCawley (ed.), "Review of Thomas A. Sebeok's (ed.) Current Trends in Linguistics, Vol. 3: Theoretical foundations," in *Grammar and Meaning: Papers on syntactic and semantic topics* (New York: Academic Press, 1976), p 173.

critique, designed to reduce support for constituency theory within the American linguistics community and, consequently, increase support for the transformational paradigm. For constituency grammarians, it quickly became a question of survival: forced into a corner with a great many problems to answer for, proponents of constituency grammar expended much time and effort on modifying their theory to match the explanatory expectations set out by TG. This debate gave transformationalists some of their strongest ammunition in their efforts to win over American linguists: painting constituency grammar as fundamentally inadequate and declaring that transformational grammar would solve all the problems with the older theory were two sides of the same coin. Indeed, Postal explicitly highlights the implied historical progression: "PSGS [phrase structure grammars] are simply incapable of correctly characterizing the sentences of human languages", he wrote, and "[t]his conclusion was the basis for the development of TG".[45] That transformational grammars were designed and developed specifically as a replacement for constituency grammars was repeatedly emphasized by Lees and Chomsky in print and in speech: "Chomsky [...] has been led to set up a whole level of grammatical transformations to deal with all the difficulties encountered in trying to state explicitly a complete and simple immediate-constituent grammar", wrote Lees in 1957 and, a year later, Chomsky told the audience at the Third Texas Conference that the "inordinate complexity, inability to state many real generalizations and regularities or to account for many facts about English structure which are intuitively obvious to any native speaker [...] can be eliminated if we extend our concept of linguistic structure to include a new level of transformational analysis".[46] Both parts of this argument were convincing to a great many American linguists – especially to younger linguists who had never been trained in constituency theory. By the mid 1960s the need to "go beyond the limitations of IC [immediate constituency] grammar" was widely agreed upon at campuses from Massachusetts to Ohio to Texas.[47] The view that transformational theory offered the proper remedy likewise grew in fashion, although it received competi-

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[45]Postal, *Constituent Structure*, p 72.

[46]Lees, *Review of Chomsky*, p 388, Chomsky, *A Transformational Approach*, p 135.

[47]Stockwell, *The Transformational Model*, p 27.

tion from stratificational grammar.

<div align="center">* * *</div>

While constituency theory struggled under the transformational data demands, stratifica-tional grammarians were much better able to account for the TG dataset and establish their own data priorities. By placing high explanatory value on supra-sentence phenomena, stratificationalists established a distinct presence and brand on the American linguistics scene: they ensured their theory would attract linguists with broad-based interests, and they clearly differentiated themselves from the transformational paradigm. Stratificational grammarians could, however, not focus solely on their own data priorities: the forceful per-sonalities on the transformational scene, combined with TG's continual public challenges to rival theories, meant that stratificationalists had no choice but to tackle the TG dataset in or-der to prove their explanatory abilities. In this respect, Wallace Chafe accurately captured the sentiment in the stratificational community when he noted that "[t]he Chomskyan lit-erature is loaded with provocative data" – and that "[i]t is not necessary to agree with Chomskyan explanations of these data to admit that explanations of some kind must be sought".[48]

Stratificational grammarians tackled the TG dataset by exploiting the multiple strata at the heart of their theory. These strata allow for the postulation of more than one form for each individual sentence. Cognizant that immediate constituency grammar was widely considered to have failed because of its restriction to surface structure, stratificationalists emphasized the ability of their theory to posit abstract representations spread over several levels. Under the stratificational account, for example, active-passive pairs are connected by "a single *sememic* structure which [has] alternate lexemic realizations, one active, the other passive".[49] As shown in figure 3.2, active and passive counterparts are realized iden-tically on the sememic stratum, drawing the explanatory link required by transformational grammarians. This sememic realization corresponds to an abstract, or semantic, under-

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[48]Wallace L. Chafe, "Review of Sydney M. Lamb's Outline of Stratificational Grammar," *Language* 44/3 (1968), p 602.

[49]Makkai and Lockwood (eds.), *Readings in Stratificational Linguistics*, p 119, emphasis in original.

standing of the real-world situation described by the active-passive pair, which is identical regardless of whether it is expressed in the active or passive form. "The common sememic structure", Makkai explains, "does not resemble either of its realizations (or any other one it might have) any more closely than the other. Thus the relationship of such pairs of sentences is in no sense a matter of the 'derivation' of one from the other (as it has been treated in some of the earlier versions of transformational theory). Rather it is an instance of the phenomenon known in stratificational theory as *diversification* – the existence of alternate realizations of a single higher element or structure on a lower stratum".[50] This identical and abstract realization of both counterparts on the sememic stratum would, as we will see, be central to the stratificational argument that SG can handle both the production and comprehension, or encoding and decoding, of language.

Sememic   SHARED ABSTRACT
Stratum   SEMEMIC FORM

Lexemic   ACTIVE   PASSIVE
Stratum   COUNTERPART   COUNTERPART
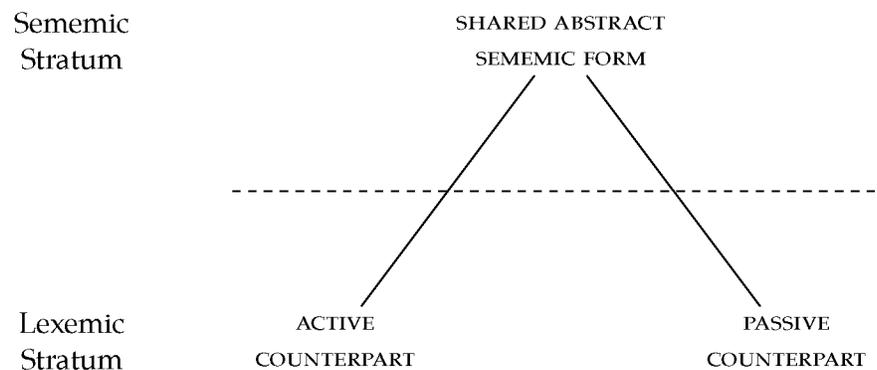
Figure 3.2: Stratificational treatment of active-passive pairs (diversification at the lexemic-sememic strata boundary)

Stratificationalists handle ambiguous sentences by means of the reverse procedure, known as neutralization. In cases of constructional homonymity such as *visiting linguists can often be a problem*, the construction has two distinct representations on the sememic stratum (one for each possible semantic interpretation), but a single neutralized realization on the lexemic stratum.[51] By representing ambiguous constructions with distinct structures on at least one level, stratificationalists answered the transformationalist demand for struc-

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[50]*Idem.*
[51]Data from *Idem*, p 119.

tural descriptions capable of capturing constructional homonymity and other ambiguities. The same method can be applied to handle ambiguous constructions such as the triplet of utterances *his picture* (he possesses it), *his picture* (he made it), and *his picture* (it is a picture of him), which cause particular difficulties for constituency theory.[52] "A properly stratified system", argued John Algeo, "will have no trouble in handling any kind of homonymy nor its opposite synonymy, the overlapping on some stratum of texts that are distinct on lower strata".[53]

These examples are representative of the techniques used by stratificational grammarians to handle the TG dataset. Given a particular construction of high explanatory value to the transformational school, stratificationalists would typically split the construction over adjacent strata by designing a pair of systems linked by inter-stratal realization. In effect, stratificational grammarians were using instances of realization to substitute for transformational rules. This allowed a great degree of flexibility for handling constructions which required some level of common structure. All constructions in the TG dataset, Makkai emphasized, can be "handled by systematic interrelations of different strata".[54]

While tackling the TG dataset was unavoidable, stratificationalists were more interested in their own data agenda, which quickly came to be representative of the theory: stratificational grammar "aims at accounting not merely for sentences", wrote Algeo in the *Journal of English Linguistics*, "but also for texts of larger extent: paragraphs, narratives, sonnets, five-act tragedies, epics, and the *Encyclopedia Britannica*".[55] This view set SG apart from the transformational school, which was largely uninterested in supra-sentence phenomena. It was here that stratificationalists could promote their theory as offering something genuinely novel. Explanation of language as a communicative device, they argued, would come not from studying isolated grammatical constructions, but from tackling a fuller range of language phenomena.

The stratificational interest in supra-sentence phenomena can be distinguished from the

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[52] Data from Algeo, *Stratificational Grammar*, p 5.
[53] Algeo, *Stratificational Grammar*, p 5.
[54] Makkai and Lockwood (eds.), *Readings in Stratificational Linguistics*, p 119.
[55] Algeo, *Stratificational Grammar*, p 7.

transformational interest in sentence-level phenomena in one important respect. Whereas transformational grammarians presented a set of syntactic *patterns* (that is, empirical generalizations capturing large amounts of data), stratificationalists never presented robust generalizations of narratives, discourses, poetry, or idioms and metaphors. For example, while the transformational interest in active-passive pairs was underpinned by a rich dataset including dozens of verbs which fit the same pattern, the stratificational interest in supra-sentence phenomena was restricted to individual occurrences of data. As such, stratificational grammarians were not able to provide generalizable pattern-based analyses of their data, but rather offered detailed analyses of individual phenomena. This distinction would, as we will see, be important to sustaining research interests.

The supra-sentence data which most interested stratificational grammarians were idioms and metaphors. In his extensive work on idioms, Adam Makkai argues that stratificational grammar can explain idiomatic and literal meanings "in a much more elegant and efficient way than other theories".[56] The title of Makkai's work – *Stratificational Solutions to Unbridgeable Gaps in the Transformational-Generative Paradigm* – captures the sentiment in the stratificational community that SG excelled in areas where TG was weak. Transformational grammars are "notoriously weak in handling sentences which are in some way idiomatic or metaphorical", said Georgetown University's Robert Di Pietro in a 1973 lecture: constructions such as *shadow of a doubt* and *concrete suggestions* cannot be handled by the transformational paradigm, he argued, because "*doubts* being [+abstract] do not cast shadows [and] *suggestions*, no matter how good they are, do not merit being marked [+concrete]".[57] Stratificationalists attributed their success with idioms and metaphors, and indeed all supra-sentence phenomena, to the level of abstraction provided by a conception of language which assumes multiple strata, each of which is significantly conceptually different from the others. "One of the big contributions of MIT linguistics has been to point

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[56] Adam Makkai, "Stratificational Solutions to Unbridgeable Gaps in the Transformational-Generative Paradigm: Translation, idiomaticity, and multiple coding," in Konrad Koerner (ed.), *The Transformational-Generative Paradigm and Modern Linguistic Theory* (Amsterdam: John Benjamins B.V., 1975), p 77.

[57] Robert J. Di Pietro, "New Vistas in a Post-transformational Era," in Roger W. Shuy (ed.), *Some New Directions in Linguistics* (Washington DC: Georgetown University Press, 1973), p 38.

out that surface structures do not convey meanings directly, that a lot of conceptual material is not included in the surface structure", Wallace Chafe noted in a 1972 interview, and hence "it has always surprised me that transformationalists still do not seem to understand how idioms operate".[58] Idioms and metaphors are, he continues, a clear example of "the discrepancy between conceptual structures and surface structures" – and thus a clear argument for the stratificational separation of the lexemic and sememic strata.[59]

If the key flaw in constituency theory was recognizing only a single level of grammatical structure, stratificationalists said, then the key flaw in transformationalism was not going far enough to rectify this mistake: "there is no real break between the levels of deep structure and surface structure corresponding to the real break that we find in a stratificational framework between, say, the sememic stratum and the lexemic stratum", explained Lamb.[60] The approach taken in *Syntactic Structures*, he continues, is that of process, or mutation, where "the linguist accounts for the relationship between alternative linguistic entities by deriving one of them from the other by means of one or more rules [that is, transformations]".[61] In contrast, stratificational grammar "considers the two related entities to be realizations of a more abstract entity at a higher level of the linguistic structure. Thus the two related entities are 'derived' from the more abstract one rather than one from the other".[62] While deep structure can be likened to the sememic stratum, stratificationalists insisted that it is not abstract enough to properly capture the semantic content of utterances – and, in particular, that it lacks the abstraction necessary to account for various types of meaning, such as idiomatic and literal meaning. This lack of abstraction is due to the transformational assumption that multiple surface structures should be derived from a single deep structure, Lamb argues – and, as such, deep structures are required to be rich enough to contain phenomena appropriate to both the lexemic and morphemic strata. Transforma-

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[58] Herman Parret, "Interview with Wallace L. Chafe (conducted in Berkeley, California, on 11 October 1972)," in *Discussing Language: Dialogues with Wallace L. Chafe, Noam Chomsky, Algirdas J. Greimas, M.A.K. Halliday, Peter Hartmann, George Lakoff, Sydney M. Lamb, Andre Martinet, James McCawley, Sebastian K. Saumjan, and Jacques Bouveresse* (The Hague: Mouton, 1974), p 19.

[59] *Idem.*

[60] Parret, *Interview with Lamb*, p 182.

[61] Lamb, *Review of Chomsky*, p 411.

[62] *Idem.*

tional grammar cannot in principle handle supra-sentence data, Lamb concludes, because it "mix[es] properties actually belonging to different structural layers".[63]

Ultimately, stratificational grammarians promoted their theory as superior to both TG and ICG at capturing linguistic data. The stratificational approach, they argued, provides "a simple and natural treatment" for both sentence-level data and supra-sentence phenomena.[64] In this latter area, where transformational theory is "labored and counter-intuitive at best", Gleason emphasized at the 1964 Georgetown Round Table Meeting, stratificational grammar had the most to offer.[65] The explanatory power provided by multiple strata, he concluded, meant that "[f]or a reasonable and useful degree of comprehensiveness, a stratificational grammar will prove simpler and clearer than a transformational-generative grammar".[66] Gleason's enthusiasm for stratificational theory was, however, only shared by a minority of American linguists.

When considering the advantages of stratificational grammar in the supra-sentence arena, many linguists were put off by the lack of agreement within SG about the number of strata required by the theory. While we have been discussing the four-strata version of SG (with phonemic, morphemic, lexemic, and sememic strata), the number of strata varied through the 1950s and 1960s. In the mid-1950s, Lamb proposed a three-strata version; in 1961 there were four strata; in 1962, five strata; and the version presented in Lamb's 1966 *Outline of Stratificational Grammar* had six strata. This ever-changing – and ever-increasing – number of strata was cause for confusion outside of the stratificational school, and eventually resulted in dissatisfaction within the community itself. The disagreement over the proper number of strata, the University of Reading's F.R. Palmer wrote, "provides us with very little faith about their 'reality'".[67] Hockett, who had earlier espoused stratificational grammar, complained in his 1968 review of Lamb's *Outline* that "more and more strata have been recognized, with no obvious limit in sight".[68] "[T]he seed was simple", he con-

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[63]*Idem*, p 412.
[64]Gleason Jr, *Organization of Language*, p 91.
[65]*Idem*.
[66]*Idem*.
[67]Palmer, *Review of Lamb*, p 291.
[68]Charles F. Hockett, "Review of Sydney M. Lamb's Outline of Stratificational Grammar," *International*

tinued, "but the plant grown from it has now become very complicated. Lamb's system has (in the booklet under review) not two strata, but six. Or perhaps it is eleven. Whether it is six or eleven depends on just how the diagram on page 20 is to be interpreted".[69] More strongly, Don Vesper argued that stratificational theory was doomed to fail because it was ballooning out of control, and would eventually require an infinite number of strata.[70]

This was no idle problem: in the late 1960s, stratificationalists were met with questions about the number of strata in their theory every time they proposed a solution for a data problem. Advances in the theory's capacity to handle data were, in effect, superseded by a specific technical problem. In his memoirs, Gleason argues that stratificational grammarians were unfortunate to have "put the constant adjustment and readjustment upfront, in a very visible place".[71] Other theories, he continues, were "able to do their adjusting in less conspicuous places" and hence presented a consistent public face.[72] While their use of multiple strata enabled stratificationalists to explain supra-sentence phenomena, it also brought them unwanted complications.

Through the 1960s, stratificational grammarians made clear their data priorities: they valued idioms and metaphors, discourse style and poetry. Explanatory linguistic theories, they argued, had to be able to account for such supra-sentence linguistic phenomena. The time and effort that stratificationalists were forced to spend on the TG dataset, however, was time and effort which could not be spent advancing their own priorities. The stratificational data agenda was also overshadowed by that of transformational grammar: while transformationalists studied broad linguistic patterns, or empirical generalizations, which captured large numbers of disparate data points under one explanatory framework, the stratificational emphasis on supra-sentence phenomena prevented them from postulating data generalizations. From the point of view of explanation, the former offered linguists a more satisfying research program with an emphasis on general language principles. In

Journal of American Linguistics 34/2 (1968), p 147.
[69]*Idem.*
[70]Don R. Vesper, "Hockett, Lamb, and Lamination," *International Journal of American Linguistics* 35/1 (1969).
[71]Gleason Jr, *Theories in Conflict*, p 145.
[72]*Idem.*

contrast, the stratificational explanatory criteria never became highly valued outside the theory itself. Linguists interested in pursuing stratificational grammar were also constantly challenged by technical problems including an ever-changing number of strata and a confusing notation system (cf. Chapter 4) – problems which contributed greatly to the low market value of their work on the 1960s American linguistics scene.

* * *

The TG dataset identified in the early 1960s retained its high explanatory value not only through that decade, but for decades afterwards. Even after flaws with the transformational approach were well-known, and after the school split into competing factions during the Semantics Wars, this original dataset remained important both for research and in the classroom. In the research context, this dataset fulfilled the role of a crucial experiment: newly-proposed syntactic theories (and modifications to existing theories) cut their teeth by demonstrating their ability to handle this data; any theory or modification lacking this ability was rejected. In the classroom, students were taught to appreciate and understand syntactic methodology through canonical examples pulled from this dataset (cf. Chapter 4). At the 1979 *Current Approaches to Syntax* conference, for example, representatives from 14 different syntactic schools were "given a set of 17 English sentences and [...] asked to produce a sample grammar for them stated in terms of their theories".[73] The dataset, presented below, conforms to the original transformational priorities: it includes active-passive pairs (3,4), declarative-interrogative pairs (3,5), embedding (8), discontinuous constituents (13), deletions (15), coordination (16), and so on.

> *Data for the sample grammar:*[74]

1. The woman walked.
2. Every woman walked.
3. The farmer killed the duckling.
4. The duckling was killed by the farmer.

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[73]Edith A. Moravcsik and Jessica R. Wirth (eds.), *Current Approaches to Syntax* (New York: Academic Press, 1980), p xiii.
[74]*Idem*, p 392.

5. Who killed the duckling?

6. A farmer killed every duckling.

7. John killed a duckling with an axe.

8. The woman believed that John killed the farmer.

9. The woman believed John to have killed the farmer.

10. The woman believed the farmer to have been killed by John.

11. The farmer was believed by the woman to have been killed by John.

12. The farmer gave the axe to John.

13. The farmer gave John the axe.

14. The axe killed the duckling that John loved.

15. John killed the woman and Bill, the farmer.

16. John loved the woman and he killed the farmer.

17. John loved the woman and killed the farmer.

Despite its ubiquity in American syntax, the TG dataset was not accepted by all. Amerindian language expert Lyle Campbell, for example, was trained in transformational theory at the University of Washington and UCLA as a graduate student in the late 1960s and early 1970s, but by 1975 had grown "disenchant[ed] with certain basic assumptions of the Transformational-Generative paradigm".[75] In particular, Campbell criticizes the transformational school for teaching students to "strain at gnats and swallow camels" – that is, to expend much time and effort on very narrow grammatical constructions deemed to be of high explanatory value, while letting larger-scope datasets pass by unexamined.[76] Similarly, Dwight Bolinger complains that transformational grammarians had "a certain proneness to skimp the specimen-gathering phase of our science and to base generalizations on insufficient data" – a tendency perpetuated by the existence of a high-value dataset used as a gatekeeper for new theories and theoretical modifications.[77] This opposition was,

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[75]Lyle Campbell, "Epistemological Dilemmas and the TG Paradigm," in Konrad Koerner (ed.), *The Transformational-Generative Paradigm and Modern Linguistic Theory* (Amsterdam: John Benjamins B.V., 1975), p 351.

[76]*Idem.*

[77]Dwight L. Bolinger, "Syntactic Blends and Other Matters," *Language* 37 (1961), p 366.

however, limited, and the transformational data agenda spread to influence all areas of American syntax.

The strong influence transformational grammar exerted over explanatory data criteria extended beyond the identification of high-value data sentences and affected the approach of American linguists to their phenomenon of study as a whole. By placing emphasis on the ability of formal systems to generate all and only the grammatical sentences of a given language, transformationalists put grammaticality judgments front and center – and, consequently, highlighted the need to explain not only grammatical, but also ungrammatical, sentences. Further, the transformational interest in accounting for the human capacity to understand an infinite number of novel grammatical utterances implicitly meant an interest in understanding language-specific and language-universal constraints on grammaticality. It was no longer enough for a syntactic theory to provide structural descriptions for sentences; explanation now also required accounting for the patterning of acceptability judgements.

With rising interest in what could *not* be said came a notational innovation: the use of the asterisk (*) to label unacceptable constructions. While small, the asterisk had "a great impact" on American linguistics, James McCawley recalls: people were "no longer concerned with just the tabulation of grammatical examples but also with stating explicitly what is not grammatical".[78] The asterisk allowed linguists to list acceptable and unacceptable sentences in an economical and simple fashion, such as in the example below.

> Example dataset:[79]
>
> > (1) The book is interesting.
> >
> > (2) The child is sleeping.
> >
> > (3) The book seems interesting.
> >
> > (4) * The child seems sleeping.

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[78] Herman Parret, "Interview with James McCawley (conducted in Chicago, Illinois, on 27 October 1972)," in *Discussing Language: Dialogues with Wallace L. Chafe, Noam Chomsky, Algirdas J. Greimas, M.A.K. Halliday, Peter Hartmann, George Lakoff, Sydney M. Lamb, Andre Martinet, James McCawley, Sebastian K. Saumjan, and Jacques Bouveresse* (The Hague: Mouton, 1974), p 249, Giorgio Graffi, "The Asterisk from Historical to Descriptive and Theoretical Linguistics: An historical note," *Historiographia Linguistica* 29/3 (2002).

[79] Adapted from Chomsky, *Syntactic Structures*, p 15.

Datasets consisting of a small number of closely-related sentences displaying a pattern which is acceptable in all but a few cases (such as *the child seems sleeping* above) became commonplace in transformational literature by the early 1960s. The data chosen for these sets was usually obtained by modifying a simple acceptable pattern in stages until it crossed the line to unacceptability. Typically comprising between two and ten sentences, these datasets significantly affected the approach of transformationalists to linguistic phenomena. The paradigmatic puzzle of transformational grammar came to be the development of one or more transformations which would generate all the acceptable sentences from a dataset, and none of the unacceptable ones. Given the idiosyncratic nature of these datasets, the resulting transformations were highly specific and narrow in scope. By the mid-1960s, transformationalists agreed that solving the language puzzles presented by these datasets was the way forward.

From this new explanatory criterion emerged a novel way of testing theories and theoretical modifications: the counterexample. As well as rejecting a proposed theory or modification because it could not generate some set of grammatical constructions, it became commonplace to reject theories and modifications because they allowed some set of ungrammatical sentences. Often, the allowance of a single counterexample was enough to doom a theoretical proposal. Manufacturing counterexamples became a highly-valued skill in transformational grammar, and those who could do so on the spot from the conference floor – chief among them Paul Postal and Robert Lees – gained a fearsome reputation as theory-breakers. "It is obviously our duty to go to the collections of data", said Chomsky at the 1958 Texas Conference, but "[a]nother duty is to sit for hour after hour trying to dream up exceptions. It perhaps depends on one's temperament which he does most of, but both are necessary".[80] As transformationalists learned to set up partial grammars which were resistant to counterexample attacks, a new style of transformation emerged – one which dealt with syntactic nuances, and which was designed to generate only certain manifestations of a syntactic pattern.

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[80] Hill (ed.), *Third Texas Conference*, p 32.

As grammaticality judgments and the use of counterexamples gained momentum in the transformational school, both concepts began to attract attention – and criticism – from outside. Opponents of TG accused transformationalists of bending grammaticality judgments in order to support their theories; that is, of declaring unacceptable sentences acceptable if they were generated by the theory, and acceptable sentences unacceptable if they were not. Consider the sentences below:

1. Your making a reference to the book displeased the author.
2. * They never insulted the men, who were Democrats.

Sentence (1) was marked as acceptable by Bruce Fraser in his 1970 paper *Some Remarks on the Action Nominalization in English*, and sentence (2) was marked as unacceptable by Paul Postal in his article *On the So-called Pronouns in English* of the same year.[81] However, these judgments have been challenged by Stanford University's Thomas Wasow, who calls them "questionable" in his article *The Wizards of Ling*, where he also accuses Fraser and Postal of treating data "cavalierly".[82] "[M]any linguists", he adds, "are less discriminating in the employment of stars than are television executives".[83]

Sentences for which acceptability is questionable to a native speaker, or contested between several native speakers, are known as *pathological data*. As transformational grammarians began to make more and more use of pathological data in the 1960s, opponents openly questioned their motivations, suggesting that such data was being used not to increase understanding, but to save theories. During a heated discussion at the 1962 Georgetown Round Table Meeting, Paul Garvin exploded at Postal, saying that with such judgments, "then, of course, you are in the marvelous position where whenever you can't analyze something you simply say, 'this is not English' ".[84]

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[81] Bruce Fraser, "Some Remarks on the Action Nominalization in English," in Roderick A. Jacobs and Peter S. Rosenbaum (eds.), *Readings in English Transformational Grammar* (Massachusetts: Ginn and Company, 1970), Paul M. Postal, "On the So-called Pronouns in English," in Roderick A. Jacobs and Peter S. Rosenbaum (eds.), *Readings in English Transformational Grammar* (Massachusetts: Ginn and Company, 1970).

[82] Thomas Wasow, "The Wizards of Ling," *Natural Language and Linguistic Theory* 3/3 (1985), p 487. For similar data challenges see Paul M. Postal, *Skeptical Linguistic Essays* (Oxford: Oxford University Press, 2003).

[83] Wasow, *The Wizards of Ling*, p 487.

[84] Hamp et al., *Transformational Theory (Panel I)*, p 37.

One of the earliest challenges to the transformational conception of grammaticality was made by Archibald Hill in 1961, in an article published in *Word*. Hill argues that the practice of asking about the grammaticality of isolated sentences results in misleading conclusions about linguistic data. Rather, he continues, transformationalists "should take sets of superficially similar sentences, and apply identical transforms to all of them, exhibiting the results. The sets, transforms, and results can be submitted to naïve expert speakers, and acceptance, rejection, and responses can be expected to be reliable, as they are not when what is submitted to the naïve expert speaker are isolated sentences".[85] Hill's call was largely ignored in the 1960s and questions about proper data practices continued to mark a sharp divide between linguists through the 1970s and 1980s. Transformationalists defended their use of pathological data by arguing that "the theoretical questions we want to answer are not, so far as we know, settled by the clear and unobjectionable data", and hence that studying borderline sentences provided the only means for real progress in the discipline.[86] Opponents replied that pathological data failed to be replicable, and hence was not suitable for a scientific theory.[87] Guy Carden and Thomas Dieterich captured the core of this problem in their presentation to the 1980 Biennial Meeting of the Philosophy of Science Association, where they noted that "no one is willing to throw away the sample sentence that provides the clinching argument for or against some particular analysis" – and implied that transformational grammar was being built on a highly questionable empirical foundation.[88] The repercussions of this debate, while fascinating and still ongoing, are beyond the scope of this study.[89]

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[85] Archibald A. Hill, "Grammaticality," *Word* 17 (1961), p 10.

[86] Morris Halle and James Higginbotham, "Wasow on Scientific Linguistics," *Natural Language and Linguistic Theory* 4 (1986), p 292.

[87] Guy Carden and Thomas Dieterich, "Introspection, Observation, and Experiment: An example where experiment pays off," *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 2 (1980), Richard Grandy, "Some Thoughts on Data and Theory in Linguistics," *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 2 (1980), James W. Ney, "The Decade of Private Knowledge: Linguistics from the early 60's to the early 70's," *Historiographia Linguistica* 2/2 (1975), Thomas Wasow and Jennifer Arnold, "Intuitions in Linguistic Argumentation," *Lingua* 115 (2005).

[88] Carden and Dieterich, *op. cit.*, p 54.

[89] The interested reader is directed to *Idem*, Grandy, *op. cit.*, Ney, *Private Knowledge*, Wasow, *The Wizards of Ling* and Wasow and Arnold, *Intuitions in Linguistic Argumentation* for more detail.

Stratificationalists were particularly vehement about what they saw as abuses within the transformational school of the competence-performance distinction. Led by Chomsky, transformational grammarians were interested in accounting only for competence, and saw performance as irrelevant to syntactic explanation. By labeling such a large dataset as outside the realm of linguistic explanation, Wallace Chafe argued, the competence-performance distinction "has been used as a rationalization for avoiding all sorts of important, relevant, and interesting questions about language".[90] Only the stratificational emphasis on language as a communicative device, he continues, offers the possibility of explaining everyday language use. By refusing to grant competence and performance equal footing, Chafe concludes, transformational grammarians lost sight of "what was really going on when people talked".[91] This argument highlights a key distinction between the stratificational and transformational schools of thought which will be further considered in section 3.5: whereas the exclusion of performance data was for the transformationalists a necessary measure for an unimpeded study of the human language capacity, the inclusion of the same data was for the stratificationalists essential to understanding communication. Oftentimes, however, the core of this disagreement was overshadowed by rhetoric as transformational grammarians built the competence-performance distinction into a key part of their polemics. "In the heat of battle", Gleason recalled in his memoirs, transformationalists found it "easy to simply label any material marshalled against TGG [transformational generative grammar] arguments as 'mere performance'. Such labeling came to be almost routine, whatever the nature of the data".[92]

While the stratificationalists gained little ground by challenging the competence-performance distinction directly, their challenges to the theoretical validity and psychological reality of strictly binary grammaticality judgments were more fruitful. "There is no boundary between grammatical and ungrammatical", said Lamb; instead, "we are really

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[90] Parret, *Interview with Chafe*, p 4.
[91] *Idem.*
[92] Gleason Jr, *Theories in Conflict*, p 73.

dealing with a *continuum*" of possible grammaticality judgments.[93] Such a continuum prevents linguists from having to make strict choices for pathological data, including in cases where native speakers cannot decide on a judgment, where judgments on a single construction vary between speakers of different dialects or styles, where judgments change over time (for example, between generations), and where judgments depend on accepting particular (and often peculiar) semantic preconditions.[94] By proposing a grammaticality continuum, stratificationalists both avoided having their own theoretical decisions hinge on pathological data and presented a strong challenge to the practices of transformational grammarians.

Stratificational grammarians used the same line of reasoning to challenge the transformational goal of specifying all and only the grammatical sentences of a given language. Under the continuum assumption, they argued, this goal was not only unfeasible but, in fact, nonsensical since the continuum assumption allows no identifiable set of grammatical sentences. "[T]he main purpose of a stratificational grammar", explained Geoffrey Sampson, "is not to generate all and only the utterances of the language in question, but rather to provide the correct realization for any content or expressive structure which is appropriate for the language".[95] Under this view, the appropriate structures of a language are not static, but rather change continually as the language evolves. The continuum assumption allows stratificationalists to build language change into their network structure. "[T]he linguistic structure itself, i.e. the network, undergoes changes while it is being used", Lamb

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[93]Parret, *Interview with Lamb*, p 210, emphasis in original. While transformational grammarians occasionally discussed gradations of grammaticality, in practice they generally operated with the binary scheme. See Noam Chomsky, *The Logical Structure of Linguistic Theory* (New York: Plenum Press, 1975), Chapter 5.

[94]Another linguist who challenged the idea of strict binary grammaticality judgments in the 1960s was Charles Hockett. In his 1966 *Language, Mathematics, and Linguistics*, Hockett structured the debate around the concepts of well- and ill-defined systems. While well-defined systems "can be completely and exactly characterized by deterministic functions", he explained, ill-defined systems are characterized by constant change. For Hockett, language – and, specifically, grammaticality judgments – are constantly changing, and this change needs to be accounted for in linguistic theories. "A language is a kind of system in which *every actual utterance*, whether spoken aloud or merely thought to oneself, at once and the same time by and large *conforms* to (or *manifests*) the system, and *changes* the system, however slightly", he wrote – a view incompatible with binary grammaticality judgments. Charles F. Hockett, "Language, Mathematics, and Linguistics," in Thomas A. Sebeok (ed.), *Current Trends in Linguistics, Volume III: Theoretical foundations* (The Hague: Mouton & Co., 1966), p 45, 83, emphasis in original.

[95]Sampson, *Stratificational Grammar*, p 10, note 3.

explains: "[i]n other words, new connections get formed all the time".[96] These new connections account for language evolution and highlight the stratificational aim of developing a linguistic theory which can explain actual language use. "The usual result of understanding some sentence containing *new* information", Lamb continues, is that "one or more new connections are formed at the sememic stratum".[97] Consider, for example, the case where a new idiom or metaphor is emerging and gaining increased use among a population of speakers. As it grows in acceptance, the required connection in the linguistic network is gradually constructed from repeated use. With Lamb's network diagram notation, this can be represented as a faint line which becomes heavier as the construction enters common usage. "[I]n terms of neurophysiology", Lamb explains, this corresponds "to a synapse which is gradually becoming established by being crossed repeatedly".[98] From the stratificational perspective, psychological reality is not compatible with a strictly generative grammar, since such a grammar requires the identification of a well-defined set of grammatical sentences. The assumption of a grammaticality continuum thus plays a four-fold role in stratificational grammar: it avoids reliance on pathological data, it enables the explanation of newly-emergent grammatical phenomena, it challenges the transformational paradigm, and it allows for a productive psychological interpretation.

The question of grammaticality (or acceptability) judgments is tied up in a much broader debate in linguistics and in the social sciences more generally about introspection and methodology. With Chomsky's promotion of introspection as a key linguistic method in his 1965 *Aspects of the Theory of Syntax* – and specifically his arguments against the use of experimental and statistical methodology in linguistics – arose a culture whereby transformational grammarians obtained acceptability judgments through personal introspection. The technique, as described by an irate Lamb, was "just making up sentences and asking people, or often just asking oneself, whether they are grammatical or not".[99] Transformationalists were simultaneously making judgments on data and developing theories to

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[96]Parret, *Interview with Lamb*, p 211.
[97]*Idem*, p 211, emphasis in original.
[98]*Idem*, p 211.
[99]Lamb, *Translation and the Structure of Language*, p 178.

account for these judgments. This methodology drew criticism from linguists of all persuasions including, unusually, from some within the transformational school itself.

James Ney, an SIL-associated linguist who trained at the University of Michigan under Kenneth Pike, objected "quite strenuously to the transformationalist's penchant for basing theoretical generalizations on 'linguistic intuition'".[100] The theory which resulted from introspective judgments, he argued, is "essentially untestable".[101] He proposed replacing single speaker judgments with statistical samples from native speaker communities – a view shared by the growing psycholinguistics community. Speaking in 1974, James McCawley challenged the utility of introspective judgments on the basis of semantic context: "I question that grammaticality judgments, independent of meaning and context, are accessible to observation", he said: "[w]hen an informant tells you that such-and-such a sentence is grammatical or not, what he has generally done it is to attempt to envision a situation in which someone might say it and report that it is grammatical if he has succeeded in thinking of such a situation; he may report as ungrammatical a sentence which he could perfectly well use if given appropriate circumstances, as a result of his failure to think of such circumstances".[102] Finally, Di Pietro argued that the ease of performing self-introspections put blinders on transformationalists: "it made it most fashionable to analyze one's own language, rather than attempt to work on others whose linguistic potentialities might be more inaccessible", he said at the First LACUS Forum, and "[s]ince the language of most generativists was English, the study of other languages diminished".[103] More recently, single-speaker introspection has been challenged by Stanford philosopher Thomas Wasow, University of North Carolina psycholinguist Jennifer Arnold, and Rice University philosopher Richard Grandy.[104] Together, they argue that introspective judgments fail to conform to scientific methodology since single-speaker data is not verifiable; since personal introspection by the working linguist is not objective; and since the pathological data which

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[100] Ney, *From the Bottom Up*, p 135.

[101] Ney, *Private Knowledge*, p 152.

[102] Parret, *Interview with McCawley*, p 251–252.

[103] Robert J. Di Pietro, "The Need to be Practical," in Adam Makkai and Valerie Becker (eds.), *First LACUS Forum* (South Carolina: Hornbeam Press Inc., 1974), p 48.

[104] Grandy, *op. cit.*, Wasow, *The Wizards of Ling*, Wasow and Arnold, *Intuitions in Linguistic Argumentation*.

frequently arises from introspection is not replicable. Akin to Ney, they argue that linguistic data-collection should follow statistical methodology, and that introspections "should be treated as a form of experimental data and evaluated at such".[105] While it is beyond the scope of this study, syntactic methodology is still the subject of a controversial and hard-fought debate in the American linguistics community.[106]

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Syntactic data was front-and-center in the theory-choice debates of the 1960s. Through a combination of forceful self-promotion and unrelenting criticism of rivals, transformational grammarians gained a monopoly on explanatory data criteria. Rival theories had no choice but to tackle the TG dataset: for constituency grammarians, this effort consumed their work through the 1960s and prevented them from elaborating their own data priorities; for stratificational grammarians, it took resources away from pursuing the data they identified as being of high explanatory value. By naming explanatory data criteria, transformationalists were able to attract young linguists to work on interesting problems, emphasize data areas in which transformational theory was strong, and direct the efforts and work of the profession at large. The dominance of transformational grammar in this arena was already clear by 1964, and when Chomsky asserted in June of that year that there is "no reason to suppose that [...] explanatory adequacy can be achieved outside of the framework of transformational generative grammar", he was speaking for a large portion of the American academic linguistics community.

3.3 Formalization

> Perhaps the most lasting result of the linguistic research of the last decade will be the acquisition of the language and techniques of modern logic and mathematics for the discussion of fundamental linguistic problems.
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> Emmon Bach, in *An Introduction to Transformational Grammars* (1964)[107]

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[105] Wasow and Arnold, *Intuitions in Linguistic Argumentation*, p 1485.
[106] See note 89.
[107] Bach, *An Introduction to Transformational Grammars*, p 143.

> Linguistics, like the other social sciences, assumes that it can and should operate via the 'scientific method' developed for the physical sciences since the fifteenth century; assumes that it is both possible and desirable to treat the artifacts of language [...] as though they were molecules or stars.
>
> Robin Lakoff, in *The Way We Were; or; the actual truth about generative semantics: a memoir* (1989)[108]

In linguistics as in many American social sciences of the era, explanation and formalization were tightly linked in the 1960s. As they strove to gain the prestige, funding, and status associated with traditional natural sciences, linguists saw formalization as key to their mission. Physics – king of the natural sciences – stood out for its formality, and especially its use of mathematics to accurately and meaningfully study physical phenomena. Mathematical representations and manipulations of data, linguists concluded, were central to the success of the natural sciences. "Everyday language is often ambiguous", wrote the University of Florida's John Algeo, and "[q]uasi-mathematical statements can force the grammarian to be explicit, to formulate his description precisely".[109] William Cooper compared linguistics directly to physics, asserting that "certain theoretical issues in linguistics [...] cannot be approached without a mathematical development of the concepts involved. The situation in linguistics is not much different from physics or some other science: if only a simple intuitive approach is to be used, mathematics is dispensable; but where rigor is needed, so is mathematics".[110]

The linguistic community's interest in mathematics was further stimulated by tight connections between mathematics and language in a set of newly emerging disciplines, from computer science to communications engineering. In the early 1960s, Emmon Bach recalls, telephone engineers were applying language structure theory to study transmission systems and computer scientists were working on machine translation and other language-intensive tasks – and even linguists with no mathematical background couldn't help but

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[108] Lakoff, *The Way We Were*, p 966.
[109] Algeo, *Where Do We Go?*, p 109–110.
[110] Cooper, *Set Theory*, p 11.

find these new interdisciplinary connections fascinating.[111] In this light, the concept of linguistic explanation became intimately associated with formalization. Given two conceptually equivalent explanations for a linguistic phenomenon, one expressed in prose and one with mathematical symbols, the latter was considered to be more explanatory. In the late 1950s and 1960s, mathematics and formalization "captured the imagination of linguists" and substantially altered their conception of explanation.[112]

The key link between explanation and formalization – the link which would motivate a decade of linguistic thought – was made by Chomsky in the mid-to-late 1950s, recorded in *The Logical Structure of Linguistic Theory*, and released in *Three Models for the Description of Language* (*ire Transactions on Information Theory*) and *Syntactic Structures*. With his presentation of a formal system for representing immediate constituency grammars (that is, his phrase structure grammar, or psg, system), Chomsky brought to light the precise nature of phrase structure grammars, and thereby demonstrated the utility of formalization. It was with the psg formalization that Chomsky rigorously identified the limitations of rewrite rules and built his technical arguments against constituency theory. (Whether psgs were in fact accurate representations of icgs is discussed below.) "Chomsky was [...] the first to really attempt to formulate and make precise the conceptions underlying immediate constituent analysis as a theory of syntax", wrote Paul Postal in 1964, and with this work linguists were "for the first time in the position of being able to seriously inquire into the descriptive adequacy of the underlying theory".[113] High-profile linguists and philosophers from Postal to Lees to Katz recognized the dearth of formalization in American linguistics and singled it out as a crucial flaw requiring immediate attention. "There was before Chomsky's work [...] little interest in the goal of specifying exactly the character of the notions *linguistic rule, grammar*, and so forth", continued Postal, but as the new formalization spread such notions became common topics of discussion in print and at conferences.[114]

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[111]Bach, *An Introduction to Transformational Grammars*, p 143.

[112]Paul L. Garvin (ed.), "Review of Roman Jakobson's (ed.) Structure of Language and Its Mathematical Aspects," *International Journal of American Linguistics* 29/2 (1963), p 174.

[113]Postal, *Limitations of Phrase Structure Grammars*, p 141.

[114]*Idem*, p 141, emphasis in original.

Lees went a step further, drawing a tight link between Chomsky's formalization of linguistic theory and scienticity in his 1957 review of *Syntactic Structures*. "Chomsky's book", Lees wrote, "is one of the first serious attempts on the part of the linguist to construct within the tradition of scientific theory-construction a comprehensive theory of language which may be understood in the same sense that a chemical, biological theory is ordinarily understood by experts in those fields".[115] Formalization and scienticity were wedded together in the transformational paradigm by the early 1960s, and soon after became intimately associated with explanation.

Nearly substitutable for the term *formal* in 1960s linguistics was the term *precise*. Imprecise syntactic theories, transformationalists argued, could not be tested, evaluated, or compared. "Only precise ideas are subject to real disconfirmation as well as to real confirmation", wrote Postal in 1967, and "[o]nly a precise conception of the form of grammatical rules makes it possible for a grammar to make a clear and thus testable claim about the sentences of individual languages".[116] Formalization was necessary, Postal concluded, because "[i]t is difficult [...] to determine just what inferences may be drawn from imprecise theories [and because] unformalized notions may conceal several alternative conceptions, hidden contradictions, unspecified questions of real theoretical interests, etc".[117] The first target of this line of reasoning was constituency grammar. It was only with the PSG formalization, transformationalists said, that constituency theory was shown to be "radically inadequate" at handling syntactic data and providing structural descriptions.[118] Formalization, they concluded, demonstrated the failure of constituency theory to explain syntactic phenomena and provided an understanding of the cause of this failure. The link between formalization and explanation was complete: in order for a theory's explanatory capacity to be evaluated, linguists inside and outside the transformational school agreed, that theory must necessarily be presented in a precise, formal manner.

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[115] Lees, *Review of Chomsky*, p 377.
[116] Postal, *Constituent Structure*, p 80.
[117] *Idem*, p 18.
[118] Postal, *Limitations of Phrase Structure Grammars*, p 144.

Having established the necessity of formalization, transformationalists then turned to the elaboration of an associated linguistic methodology to which any theory hoping to offer an explanatory account of syntax was required to adhere. Descriptive analyses of language – that is, procedures for the segmentation of syntactic strings – were, transformational grammarians argued, not an adequate basis for an explanatory linguistic theory. Such an approach to linguistics, Lees argued, "is tantamount to viewing physiology as a branch of surgery, or organic chemistry as a branch of petroleum engineering".[119] These "crude goals", he continued, could never give rise to explanatory capacity.[120] Instead of simple description, linguistic theories were required to assign grammatical structures to sentences, "no matter how abstractly these structures may have to be formulated and no matter how indirectly they may happen to relate to the physical record of the sentences".[121] It was on this methodological basis that transformationalists judged all rival syntactic theories. "A grammar *must*", Chomsky wrote in 1963, "generate a language regarded as an infinite set of sentences. It *must* also associate with each of these sentences a structural description; it *must*, in other words, generate an infinite set of structural descriptions, each of which uniquely determines a particular sentence".[122] With this stipulation, grammars which did not hold a strong view of generative capacity – such as stratificational grammar – were excluded from the transformational conception of explanation. Theories which transformationalists identified as merely descriptive – such as constituency grammars – were similarly excluded.

While the elevation of formalization to an explanatory criterion was largely a successful strategy for transformational grammarians, it caused two problems which plagued TG through the 1960s. The first concerns the claim that Chomsky's phrase structure grammars are an accurate formalization of, or are formally equivalent to, immediate constituency grammars. From the outset, constituency grammarians protested adamantly that the phrase structure formalization did not accurately represent their syntactic theory; rather,

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[119]Lees, *Nominalizations*, p xxv.
[120]*Idem.*
[121]*Idem.*
[122]Chomsky and Miller, *Introduction to Formal Analysis*, p 297, my emphasis.

they argued, PSGS were limited and bastardized versions of ICGS. This argument presented the greatest hope to the constituency grammar community: if they could prove conclusively that ICGS were indeed more powerful than the system represented by PSGS, then they could dismiss the transformational arguments against PSGS as irrelevant to their work, and hence refute the claim that constituency grammars could not be explanatory.

Importantly, constituency grammarians did not contest the assertion that PSGS could not adequately capture problematic constructions; rather, they countered that constituency grammars – which, they underlined, were not equivalent to PSGS – *could* capture these structures. This argument was put forward most forcefully by Gilbert Harman. "Chomsky and others", Harman wrote in 1963, "have argued that a theory of language which supposes that the grammar of a natural language may be completely described by means of a phrase-structure grammar must be inadequate. They have also argued that no phrase-structure grammar will be adequate for giving a full grammatical description of sentences in English. Their arguments, however, have been based on a very particular definition of phrase-structure grammar which greatly restricts the amount of information supplied by such a grammar".[123] "[T]here is no reason to place these restrictions on the notion of phrase-structure grammar", he continues, and "if Chomsky's conception is modified slightly to permit grammars of the type described in this paper, objections against the theory of such a grammar can be met".[124] As explained earlier, Harman goes on to provide a modified version of immediate constituency grammar which he claims is of equivalent explanatory capacity to transformational grammars. Harman concludes that transformations have "no advantage" over constituency theory, and thus that "there is no need to introduce transformational rules into generative grammars of natural languages".[125] Gilbert Harman's arguments to this effect were supported by, among others, the Illinois Institute of Technology's William Austin, who called the PSG restrictions "arbitrary", and John Street,

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[123]Harman, *Defense of Phrase Structure*, p 597.
[124]*Idem.*
[125]*Idem*, p 597–598.

who argued that Chomsky's phrase structure grammars were excessively constrained.[126] Charles Hockett wrote in *Language* that PSGS were not "properly formulated", and even Sydney Lamb weighed in in defense of constituency grammarians, arguing that PSGS were "not a model that has actually been used by any real linguists".[127]

The potential of Harman's argument to undermine a key tenet of transformational grammar was well-appreciated by supporters of that theory, and the transformational response – successful through the 1960s – was to dismiss Harman's work as irrelevant and continue to assert that *no* constituency grammar, however formulated, was capable of explanation. Chomsky's direct response to Harman's 1963 publications is a classical example of transformational polemics: Harman's paper, Chomsky said in a lecture at Indiana University, is "entirely irrelevant to the whole issue"; it is, he continued, "nothing more than terminological equivocation" and "hardly worth pursuing".[128]

The second problem caused for transformational grammar by its emphasis on formalization presented greater difficulties: it concerns the elaboration of a simplicity metric for grammars. In transformational thought, formalization and simplicity were intimately linked. A key reason for formalization, Chomsky emphasized in *Syntactic Structures*, was to enable the comparison and evaluation of syntactic theories – tasks which, in turn, were to be accomplished by means of a simplicity metric. "Our ultimate aim", wrote Chomsky, "is to provide an objective, non-intuitive way to evaluate a grammar once presented, and to compare it with other proposed grammars".[129] By measuring or quantifying the relative simplicity of competing grammars, Chomsky aimed to automate the selection of simple grammars. Simpler grammars are preferred, he continued, because "the simpler grammars meet certain external conditions of adequacy while the more complex grammars that embody different decisions about assignment of sentences to the kernel, etc., fail these con-

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[126]William M. Austin, "Logicalism and Formalism in Linguistics," in William M. Austin (ed.), *Papers in Linguistics in Honor of Leon Dostert* (The Hague: Mouton, 1967), p 19, Street, *op. cit.*.

[127]Charles F. Hockett, "Linguistic Elements and Their Relations," *Language* 37/1 (1961), p 52, Lamb, *Review of Chomsky*, p 412. For more recent discussion, the reader is directed to Robert Borsley, *Modern Phrase Structure Grammar* (Oxford: Blackwell, 1996) and Alexis Manaster-Ramer and Michael B. Kac, "The Concept of Phrase Structure," *Linguistics and Philosophy* 13 (1990).

[128]Chomsky, *Topics*, p 41, 46.

[129]Chomsky, *Syntactic Structures*, p 56.

ditions".[130] For Chomsky, simplicity was a systemic measure; that is, any proposed simplicity metric would have to provide a method of measuring the simplicity of the whole grammatical system at stake. The idea was, in principle, elegant and intuitive, and it was immediately promoted by Lees, who in 1957 lauded Chomsky's ideas for "provid[ing] an opportunity for the application of explicit measures of simplicity to decide preference of one form over another form of grammar".[131]

Chomsky did not provide a detailed explication of simplicity metrics in the widely-distributed *Syntactic Structures*, where he argued it would "go far beyond the scope of the present monograph", but rather in his lesser-known *The Logical Structure of Linguistic Theory*.[132] "In constructing a grammar", he wrote in the 1975 release of that manuscript, "we try to set up elements having regular, similarly patterned, and easily statable distributions, and which are subject to similar variations under similar conditions; in other words, elements about which a good deal of generalization is possible and few special restrictions need to be stated".[133] Importantly, he continued, "any simplification along these lines is immediately reflected in the *length of the grammar*".[134] Grammar length or, equivalently, notation length, quickly became the basis of the transformational simplicity metric. The key to implementing such a metric was to devise a weighted function acting on the notational symbols of the grammar, designed so as to favor reductions in certain parts of the grammar in ways appropriate given system-wide considerations. By permitting "just those reductions in length which reflect real simplicity, that is, which will turn simpler grammars [...] into shorter grammars", length of notation could be used as a measure of simplicity, and hence as a method of evaluating alternative versions of transformational grammars.[135]

Despite enunciating and promoting the concept, transformationalists accomplished little with respect to simplicity metrics in the 1960s. "[N]o explicit proposal has ever been made for an evaluation [simplicity] measure of the type which Chomsky envisions", noted James

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[130]*Idem*.
[131]Lees, *Review of Chomsky*, p 378.
[132]Chomsky, *Syntactic Structures*, p 55.
[133]Chomsky, *Logical Structure*, p 117.
[134]*Idem*, p 117, my emphasis.
[135]Goldsmith, *From Algorithms to Generative Grammar*, p 117.

McCawley at the 1964 Linguistics Society of America Linguistic Institute.[136] The transformational school, he continued, had been unable to design a simplicity measure capable of assigning an overall system-wide cost to a grammar. The chief difficulty lay in developing a metric which could simultaneously act on all three parts of transformational grammars (that is, rewrite rules, transformational rules, and morphophonemic rules), and which could meaningfully interpret the effects of shifting phenomena between the phrase structure and transformational components. "The actual practice of transformational grammarians", McCawley concluded, "has been to rely very little on evaluation measures but rather to choose between grammars on the basis of either 'crucial experiments' (i.e. eliciting data for which the two grammars have different implications), or of data from other languages, which permit tighter universal constraints to be imposed on the class of possible grammars".[137] This lack of progress continued through the decade and, by the early 1970s, the transformationalist effort was fully turned toward other ideas including, most prominently, the search for linguistic universals.

Where transformational grammarians floundered, however, stratificational grammarians flourished, and proved far more adept at implementing simplicity measures. While simplicity metrics had not been essential to Lamb's original conception of SG, stratificationalists soon recognized that such metrics would be relatively easy to implement within their theory, and hence that they could better TG in an area which transformational grammarians themselves proclaimed to be important. The amenability of stratificational theory to simplicity metrics, its proponents argued, was due to the uniform nature of the multiple strata comprising linguistic representation: whereas transformational grammar contains three rule types which are "not obviously logically comparable", stratificational grammar is based on relationships which are "all logically compatible" and uses the same nodes and realization types on each stratum.[138] This uniformity in the grammar meant that stratificational simplicity metrics could easily transition between linguistic levels, and hence easily

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[136] McCawley, *Review of Sebeok*, p 172.
[137] *Idem.*
[138] Sullivan, *Stratificational Theory*, p 325, Lockwood, *Stratificational Linguistics*, p 12.

provide a system-wide measure.

In its basic form, the stratificational simplicity metric operates by counting the number of nodes required by a grammar. Because of the small number of node types in SG (cf. Chapter 2, figure 2.6), their centrality to network structure, and their uniformity across all strata, node-counting provides "a simple, uniform *evaluation measure*" for stratificational theory.[139] "This measure", linguist and computer scientist Alexander Borgida asserted, "enables one to decide which of two networks representing the same data is simpler, and hence constitutes a better description".[140] The development of node-counting into an explicit and workable metric was accomplished by Peter Reich and released in his 1968 *Symbols, Relations, and Structural Complexity*, originally written as a report to the Yale Linguistic Automation Project, and later reprinted in the Makkai and Lockwood anthology. Reich's method is based on the concept of effective information. Two network diagrams are said to provide the same *effective information* if and only if they provide the same set of outputs (that is, if they allow for the same set of linguistic constructions). It is, of course, possible that two network diagrams have the same effective information but are *not* isomorphic to one another – that is, they have different configurations of lines and nodes. Given two such diagrams, Reich's simplicity metric chooses between them by deciding which has the simpler network configuration. The metric uses a weighted count of nodes designed to measure the number of relationships in a network system, as well as the complexity of those relationships. Reich's evaluation procedure, Makkai and Lockwood conclude, "provides a stratificational answer to Chomsky's assertion of 1957 that one of the important goals of theoretical linguistics should be the construction of an evaluation procedure to allow the comparison of competing treatments of the same data".[141]

Their own simplicity metric thus developed, stratificationalists were striking in their criticism of transformational grammar in this arena. "For years", the University of Florida's William Sullivan wrote, transformationalists have been "disagreeing on the development of

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[139] Borgida, *op. cit.*, p 397, emphasis in original.

[140] *Idem*, p 397.

[141] Makkai and Lockwood (eds.), *Readings in Stratificational Linguistics*, p 3.

a general simplicity metric" and, as a result, within transformational theory "it is impossible to compare and assess different answers to particular issues".[142] Lockwood emphasizes that TG "failed" in this area – an area which transformationalists themselves identified as critical.[143] The chief fault with transformational grammar, stratificationalists agreed, lay in its formalization, "which provides basically different kinds of systems for phonology, syntax, and semantics [...] so that solutions resulting in complications in one component at the expense of simplification in another cannot be compared".[144] While a reduction in transformational complexity could result in an increase in phrase structure complexity and vice versa, formal differences between these levels could not easily be compared by simplicity metrics.

Lockwood emphasizes that the relative success of the two theories in establishing a workable simplicity metric is rooted in their notation systems. The notation-length simplicity measure preferred by the transformationalists, he argued, was not appropriate for a theory "where the symbolization does not correspond to clear notions of simplicity and generality".[145] In stratificational theory, simplicity metrics could take advantage of a graphic notation system which represented tactic phenomena with a small set of basic relationships. In contrast, Lockwood continues, the algebraic notation used by transformational theory "represents linguistic relationships in far too indirect and nonuniform a manner to be useful for the development of an effective evaluation measure".[146] The success stratificationalists enjoyed with simplicity metrics – and the failure of transformationalists in the same arena – was a source of great pride within the stratificational community through the late 1960s and 1970s. This was the area in which stratificationalists could argue most directly for the superiority of their theory over TG. Stratificational grammar rose to "Chomsky's challenge", Lockwood concluded, while "[t]he followers of transformational theory,

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[142]William J. Sullivan, "Toward a Logical Definition of Linguistic Theory," in Edward L. Jr. Blunsitt and Robert J. Di Pietro (eds.), *Third LACUS Forum* (South Carolina: Hornbeam Press Inc., 1976), p 393.

[143]Lockwood, *Stratificational Linguistics*, p 11.

[144]*Idem*, p 12.

[145]*Idem*, p 265.

[146]*Idem*.

on the other hand, have retreated from this goal when faced with adversity".[147] However, as we will see in Chapter 4, while Lamb's notation system was celebrated by stratificationalists as an advantage in developing a simplicity metric, outsiders found the system confusing and unwieldy.

<p style="text-align:center">* * *</p>

The emphasis on formalization in 1960s linguistics, and the equivalence of formalization and explanation, both emerged from within the transformational school of thought, but their reach extended well beyond TG and forced rival syntactic theories to act and answer. The operating principle at the time, Robert Hall noted, was that "sciences are to be evaluated according to the extent to which they can be organized on the basis of mathematical procedures" – and linguists, with their sights set on the natural sciences, were intent on a mathematical reorganization of their subject.[148] Importantly, the manifestations of this fashion were dictated by the transformational school. While transformationalists were challenged on their identification of phrase structure and immediate constituency grammars and on their implementation of simplicity metrics, they still succeeded in setting the conditions of explanation: while simplicity metrics were foreign to the original conception of stratificational grammar, stratificationalists felt compelled to devote much time and effort through the 1960s to demonstrating their ability to handle this concept. Similarly, while constituency grammarians did not agree with the PSG formalization, they recognized that it proved the necessity of formalization in linguistics, and their rejection of PSGS went hand-in-hand with efforts to develop an accurate formalization of constituency grammars. The extent to which rival theories felt pressured to meet the transformational explanatory priorities is shown by Borgida's comment at the Fourth LACUS Forum, held in 1977 at Canada's McGill University, that "[l]inguistic theories should be judged on the basis of their ability to usefully present actual linguistic data [but] since the advent of the Transformational Generative approach, it has become necessary to consider the formal aspects of competing

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[147] *Idem.*

[148] Robert A. Hall Jr (ed.), "Review of Charles F. Hockett's The State of the Art," in *Linguistics and Pseudo-linguistics: Selected essays, 1965–1985* (Amsterdam: John Benjamins Publishing Company, 1987), p 66.

theories as well".[149] In considerations of formalization as in considerations of data, transformational grammarians monopolized explanatory criteria. Rival syntactic theories were left trying to meet these expectations, sometimes successfully and sometimes not. However, even when they were successful, such as with the stratificational implementation of simplicity metrics, it was at the expense of setting their own agenda and priorities.

3.4 Power

> The 60s were a time when in the background of all the debate was the notion of 'power' of a model, and the alleged proofs that only transformations could bestow on a grammar sufficient power.
>
> H.A. Gleason, in *Theories in Conflict: North American linguistics in the fifties and sixties (a memoir)* (1988)[150]

In the early 1960s, transformational grammarians desired one thing above all others: a rigorous proof of the need for transformations as a linguistic tool. While Chomsky had convinced a significant portion of the American linguistics community that transformational grammars were better equipped to handle natural language syntax than constituency grammars, he had not conclusively demonstrated that there was no conceivable other linguistic tool which could match the explanatory adequacy of transformations. The desire for such a proof came to center stage during the power debates of the 1960s.

Syntactians used the term *power* in a variety of related ways through the 1960s, but rarely provided a formal definition.[151] At its most basic, power refers to an overall measure of a linguistic theory's ability to capture data. Low-power theories, for example, can capture simple declarative clauses but not embedded clauses, while higher-power theories can capture both sets of clauses. Higher power is, however, not always advantageous, because a theory with unrestricted power would produce all possible constructions from a language (that is, the set of all grammatical and all ungrammatical constructions). As enunciated

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[149]Borgida, *op. cit.*, p 391.
[150]Gleason Jr, *Theories in Conflict*, p 163.
[151]For an early discussion of power, see Chomsky, *Syntactic Structures*, p 18ff.

by Chomsky in the late 1950s, a central aim of linguistics was to develop syntactic theories whose power was exactly suited to natural language. Exploring the power of grammars was, Gleason recalls, "novel in both form and intent", and it was one of the most misunderstood concepts operating in 1960s linguistics, in large part because it was tightly connected with mathematical ideas beyond the knowledge of many linguists.[152]

Power considerations were at the root of the transformational identification of constituency theory as inherently taxonomic. Under the constituency paradigm, wrote Robert Lees, linguistics was analogous to "a herbarium, anatomical map, or library catalog", while with the rise of transformational grammar linguistics became "a scientific theory embodying proposed laws of nature".[153] Rephrased in terms of power, this distinction asserts that transformational theory is "essentially *more powerful* than description in terms of phrase structure" – and it is this additional power, Chomsky argues, which enables TG to capture data in an explanatorily adequate manner.[154] Transformational grammars earned their additional power from the ability of transformational rules to appeal to the derivational history of a string. Constituency grammars, on the other hand, were limited to rewrite rules, which can appeal only to the shape of a string at the moment of rule application. In the case of coordination, for example, knowledge of the past history of strings was considered essential for the application of the conjunction rule described in section 3.2. This appeal to derivational history was, however, not a rigorous proof of the incapacity of constituency grammars to capture natural language. Such a proof would require demonstrating the existence of a syntactic construction in some natural language which it is impossible to generate with phrase structure rules.[155]

The idea of such a proof was immensely appealing to transformationalists, because with it in hand they could conclusively reject PSGs and any equivalent linguistic theories. It was

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[152]Gleason Jr, *Theories in Conflict*, p 79. Mathematically, the *power* of a syntactic program is equivalent to the formal closure of that program under its permissible operations.

[153]Lees, *Review of Chomsky*, p 380.

[154]Chomsky, *Syntactic Structures*, p 47, my emphasis.

[155]This form of proof is simple: it is a proof by contradiction. The proposition to be disproved is that for all S (where S is a sentence from any natural language), a phrase structure grammar can be written to generate S. The proof operates by demonstrating the existence of a single sentence S' which can *not* be generated by a phrase structure grammar.

Paul Postal who made the most important attempt to produce a conclusive proof of the power deficiency of phrase structure grammars. Published in a 1962 Quarterly Progress Report from MIT's Research Laboratory of Electronics, Postal's work aimed to show that Mohawk (an Amerindian language spoken in eastern Ontario, southern Québec, and up-state New York) is not a context free language – that is, to show that "it is impossible to construct a finite set of context-free [...] rules which will enumerate all and only Mohawk sentences".[156] Since phrase structure rules are incapable of generating non-context-free languages, this would prove that phrase structure grammars cannot generate the Mohawk language. To this end, Postal demonstrated a set of Mohawk constructions which he argued were formally equivalent to a symbolic language known to be non-context-free. This proof, he concluded, provided conclusive evidence that PSGS "cannot yield correct grammars for natural languages".[157]

Having proved to his satisfaction that phrase structure grammars are fundamentally not powerful enough to capture natural language, Postal turned to the second part of his program: showing that other rival syntactic theories were equivalent to PSGS, and by extension also inadequate for natural language. The result – a 1964 manuscript titled *Constituency Structure: A study of contemporary models of syntactic description* – would grow into one of the most influential books for a generation of linguistics students. "The purpose of the present study", Postal wrote in the book, "is to argue that despite a multitude of minor differences, both of substance and terminology, there is an essential equivalence among those syntactic conceptions prevalent in the United States which do not utilize transformational devices".[158] Postal investigates eight non-transformational linguistic theories from immediate constituency grammar to stratificational theory to the lesser-known morpheme class substitution system grammar. Each of these grammars, he argues, is formally equivalent to

[156]Postal, *Limitations of Phrase Structure Grammars*, 146. A context free language is a language generated by a grammar in which every rule has the form $A \rightarrow B$, where A is a single non-terminal symbol, and B is a string of one or more terminal and/or non-terminal symbols. Importantly, the left-hand symbol (A) has no contextual restrictions.

[157]Paul M. Postal, "On the Limitations of Context-Free Phrase-Structure Description," *Research Laboratory of Electronics (Quarterly Progress Report, Massachusetts Institute of Technology)* 64 (1962), p 236.

[158]Postal, *Constituent Structure*, p 1.

phrase structure grammar – and hence not powerful enough to explain natural language.

Postal received strong support from Chomsky, who referred his readers to Postal's manuscript when asserting that non-transformational linguistic theories all "seem to fall largely within the scope of the theory of constituency-structure grammar".[159] The equivalence of stratificational grammar with phrase structure grammars was, however, vigorously denied by the stratificational school. Postal's assessment of SG is "clearly wrong", said Gleason at the 1964 Georgetown Round Table Meeting: "Postal's understanding of stratificational grammar completely misses the point. Lamb's 'representational rules' [...] *do* 'affect generative power' precisely because they do 'map one type of representation into another' ".[160] Representation (more commonly called realization), Gleason argued, introduced a level of abstraction sufficient to endow stratificational theory with precisely the power necessary for syntactic explanation. Stratificational grammar, he concludes, "can be shown to have all the 'power' that a transformational-generative grammar has".[161] From across the Atlantic, Geoffrey Sampson added that stratificational grammars "are not equivalent to PSGS, because of their provision for anataxis and discontinuous [constituents]" – that is, because of their ability to handle problematic constructions.[162]

Despite these protestations, Postal's book was very influential among young American linguists. Postal had developed a fearsome reputation as a bold speaker, a skilled debater, and an ardent transformationalist – qualities which gained him a following and an audience. *Constituent Structure* was built on the principle that no theory other than transformational grammar could possibly be explanatory – a principle which manifested itself in Postal's narrative and tone, both of which exuded negativity and confrontation. Widely read and discussed, Postal's book played a crucial role in convincing young linguists of the inadequacies of non-transformational theories. For several years after the release of the manuscript, Gleason recalls, "any effort to say something about Stratificationalism was

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[159]Noam Chomsky, "Formal Properties of Grammars," in R. Duncan Luce, Robert R. Bush, and Eugene Galanter (eds.), *Handbook of Mathematical Psychology* (New York: John Wiley and Sons, Inc., 1963), p 325.

[160]Gleason Jr, *Organization of Language*, p 90, emphasis in original.

[161]*Idem*, p 90.

[162]Sampson, *Stratificational Grammar*, p 10, note 3.

met with a flat assertion that it had been 'proven' inadequate".[163] When stratificationalists produced partial network grammars, he continues, "TG linguists would not inspect them". Within the transformational community, Postal's word was final, and "[s]tratificationalism was dismissed as proven unworkable".[164]

While stratificational grammar suffered in the mid-to-late 1960s at the hand of Postal's power arguments, in the early 1970s stratificationalists turned the tables and built power considerations into a strong and effective criticism of transformational theory. "One of the goals set by modern theoreticians for a linguistic notation", wrote Peter Reich in 1970, "is that it should be powerful enough to handle the complexity found in natural language, *yet not so powerful that it can do anything computable*".[165] A linguistic theory, in other words, must make claims about syntactic structure which, on one hand, provide nontrivial insight into language and, on the other hand, avoid providing so much information so as to undermine their significance. Supporters of transformational grammar promoted their theory as being more powerful than phrase structure grammar – but until the late 1960s little attention was paid to the upwards reach of this power. In a pair of seminal articles published in 1971 and 1973, Stanley Peters (University of Texas at Austin) and Robert Ritchie (University of Washington, Seattle) showed that transformational theory was, in effect, *too powerful*: it allowed "too many different languages [and] could cover languages of the sort everybody was convinced could not be human languages".[166] Formally, the Peters-Ritchie proof showed that every recursively ennumerable set of strings can be generated by a transformational grammar.[167] They further proved that restrictions on the phrase structure component (that is, the base) of a transformational grammar made no difference to the generative capacity of the grammar as a whole. Given a recursively ennumerable language L, no matter how

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[163]Gleason Jr, *Theories in Conflict*, p 84.

[164]*Idem*, p 85.

[165]Peter A. Reich, "Relational Networks," *Canadian Journal of Linguistics* 15/2 (1970), p 109, my emphasis.

[166]Stanley Peters and Robert W. Ritchie, "On Restricting the Base Component of Transformational Grammars," *Information and Control* 5 (1971), Stanley Peters and Robert W. Ritchie, "On the Generative Power of Transformational Grammars," *Information Sciences* 6 (1973), Gleason Jr, *Theories in Conflict*.

[167]Given an alphabet A, a *recursively enumerable language* over A is any recursively enumerable subset of the set of all possible words over A (alternatively, any formal language in which all valid strings can be enumerated by a Turing machine).

the phrase structure rules of a transformational grammar are composed – whether highly restricted or very free – transformations can be added to force the grammar to generate L. Thus, the relative simplicity of the phrase structural and transformational components had no effect on the power of the grammar – and, consequently, the development of formal simplicity procedures for transformational grammars would be very difficult, if not impossible. Together, these arguments dealt a strong blow to transformational theory.

While these articles were not published in mainstream linguistics journals – they appeared in *Information and Control* and *Information Sciences*, which catered to engineers and computer scientists – they still received a great deal of attention from the linguistics community. In contrast to their usual outright rejection of criticism from outsiders, transformational grammarians took Peters' and Ritchie's findings seriously. Describing their papers as "extremely important", Emmon Bach credited Peters and Ritchie with remedying the lack of understanding of the mathematical properties of transformational grammars, and exposing a problem which had "devastating consequences for the study of syntax".[168] For all their advantages, transformational rules were, it turned out, too powerful. Reaction was swift and as the 1970s opened, transformationalists focused their efforts on developing constraints to limit the power of transformations and reestablish confidence in the theory. "We need to find heavy restrictions on the power of transformations", said Bach at the 1971 Georgetown Round Table Meeting – it is, he emphasized, the only way out of the "present impasse".[169] While the progression of this work into the 1970s is beyond the scope of this study, it is important to recognize that the Peters-Ritchie proof represents the first significant setback for transformational grammar between its introduction in 1957 and the early 1970s.[170] Transformationalists faced this dilemma in large part because they had pushed the transformation-as-tool without rigorously investigating all the associated mathematical consequences. While Chomsky gave formal mathematical definitions of finite state grammars and phrase structure grammars in *Syntactic Structures*, neither that

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[168] Emmon Bach, "Syntax Since Aspects," in Richard J. O'Brien (ed.), *Report of the Twenty-second Annual Round Table Meeting on Linguistics and Language Studies* (Washington DC: Georgetown University Press, 1971), p 2, 4.

[169] *Idem*, p 12.

[170] The reader interested in extensions of this work into the 1970s is directed to Matthews, *Grammatical Theory*.

manuscript nor *Aspects* contained a mathematized definition of transformations. As the use of transformations proliferated through the 1960s, transformational grammar progressed "without a study of [its] mathematical consequences".[171] This setback is representative of a broader uneasy relationship between transformational linguists and mathematics, which is explored more fully in Chapter 4.

By the time Peters and Ritchie published their indictment of transformational power in the early 1970s, TG had already gained the support of the majority of academic linguists in America. While the Peters-Ritchie proof was certainly a setback for transformationalists, it did not threaten their dominance on the academic scene: rather than turning to other theories, transformational grammarians worked to solve the problems identified with TG and to reestablish a viable transformation-based theory. Transformations were still "generally assumed" to be the most appropriate tool for tackling syntax – they just needed to be reined in a bit.[172]

* * *

Through the 1960s, transformational grammarians used power arguments to their advantage: led by Paul Postal, they claimed to have rigorously proved that phrase structure grammars were incapable of capturing natural language syntax. With Postal's analysis of other non-transformational linguistic theories, they extended this claim to include their main rival of the 1960s, stratificational grammar, and a handful of less influential syntactic theories. These arguments – bolstered by Postal's reputation and style – were tremendously successful at drawing young linguists to transformational theory and creating a climate of opposition to rival syntactic theories.

The emergence of the Peters-Ritchie proof in the early 1970s was too late to stop the transformational momentum. While Peters and Ritchie showed that transformational theory was, in effect, too powerful, the transformational grammar community was already dominant in American academic linguistics. Rather than turning to other syntactic theories, transformational grammarians chose to retain their theory commitment and work to

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[171]Bach, *Syntax Since Aspects*, p 3.
[172]Gleason Jr, *Theories in Conflict*, p 81.

modify TG so as to restrain its power. In doing so, they hoped to regain the optimism and confidence which characterized transformational grammar through the 1960s.

3.5 External Validity

> The particular phase of the world I am interested in, besides the linguistic data, is the human mind. The special aim, at least in current work in stratificational grammar, is to get closer to an understanding of the mind and how it works. This abstract system that lies behind the linguistic data is supposed to correspond in some way to the system in the mind of the speaker of the language, which for him lies behind the linguistic data that he is able to produce and understand.
>
> Sydney Lamb, in an interview with Herman Parret (1972)[173]

> A general linguistic theory of the sort [described in this book] must [...] be regarded as a specific hypothesis, of an essentially rationalist cast, as to the nature of mental structures and processes.
>
> Noam Chomsky, in *Aspects of the Theory of Syntax* (1965)[174]

So far in this chapter, we have discussed technical manifestations of explanation – data, formalization, and power. We now turn to a less technical explanatory criterion: external validity. In explanatory accounts of scientific knowledge, external validity is often invoked as a method for choosing between theories: if theories A and B both cover a given set of data equally well, the argument goes, then theory A is more highly valued (that is, more explanatory) than theory B if theory A can *also* explain some set of external data which is unexplainable by theory B. In this section, I explore the influence of external validity as an explanatory criterion in the 1960s syntactic theory-choice debates by appealing to the conceptions of working linguists of that era.

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[173]Parret, *Interview with Lamb*, p 185.
[174]Chomsky, *Aspects*, p 53.

In her discussion of syntactic theories *circa* 1980, Edith Moravcsik showcases external validity as a measure for judging syntactic theories – but she does so from a perspective of broad intellectual interest, and not from the perspective of linguists themselves. External validity, she writes, is "the extent to which different [syntactic] meta-theories prove to be instrumental in intellectual and practical endeavors outside the domain of syntax itself".[175] It is to a theory's advantage if it is "instrumental in the characterization of syntax as a whole as being similar to other things in language and in the world in general", she continues, if it "represents human language syntax as sharing common features with the 'syntax' of body language or of other symbolic systems", if it "contributes to the characterization of human language as an instrument, or as a kind of goal-directed behavior", and if it is "instrumental in practical endeavors such as language teaching or translation or language planning".[176] While these areas of application – semiotics, instrumentality, behavioral correlates, and practical utility – are all of potentially great interest, they cast a net much wider than the set of external criteria important to any one syntactic school at any one time. In order to understand the internal dynamics of 1960s syntax, we need to recognize that external validity is not equally valued by all schools of linguistics, and also that there are disagreements within individual schools about the role and utility of external validity. In this section, we explore the two areas of external validity most important to 1960s linguists: computerizability, which was central to the stratificational school, and psychological validity, which was important in both transformational and stratificational grammar.

Computerizability

In the climate of the 1960s, when computers were coming into their own and when the Cold War raged, machine translation and computerizability captured the imaginations and efforts of some American linguists. Stratificational grammar was developed hand-in-hand with machine translation efforts in the 1950s and 1960s, at Berkeley and at Yale. Work in this

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[175]Moravcsik, *On Syntactic Approaches*, p 17.

[176]*Idem*, p 17–18.

area ranged from assembling a computerized Russian dictionary to developing computer-based performance models for language. Computer applications – and the funding they secured from the National Science Foundation – also helped attract students: those who joined the stratificational community in the 1960s, from Peter Reich to Stanley Peters to Alexander Borgida, were particularly interested in connections between computers and linguistic theories, and the stratificational research climate encouraged them to follow these interests in their graduate and professional work. The development of stratificational theory, Wallace Chafe wrote in 1968, "cannot be dissociated from [...] work in computational linguistics".[177]

Stratificational grammarians pursued machine translation as an integral part of their linguistics program. Only an approach which recognizes the "structural strata of language", wrote Sydney Lamb in his 1965 article *The Nature of the Machine Translation Problem*, has the potential to be successful at solving the machine translation problem.[178] Those strata provided a natural method for separating machine translation procedures into a number of stages: stratificational machine translation was based on a series of stratal conversions which joined the input and output languages at the highest stratum (in the four strata version, the sememic stratum). As shown in figure 3.3, the input text (represented as a linear string of alphabetical characters) is converted in turn from the morphemic to the lexemic to the sememic strata, at which point the input is represented as a sememic network (that is, an abstract structure detailing the semantic and communicative content of the input text). This sememic network can now be translated into the corresponding sememic network for the output language. This task is relatively simple since sememic content, which is directly related to meaning, is largely language-unspecific. To obtain the output text, the translated sememic network is re-converted through the strata in reverse order, ending with a linear string of alphabetical characters in the output language.

Any machine translation program built on a linguistic theory which presupposes the

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[177]Chafe, *Review of Lamb*, p 595–596.

[178]Sydney M. Lamb, "The Nature of the Machine Translation Problem," *Journal of Verbal Learning and Verbal Behavior* 4/3 (1965), p 196.

INPUT LANGUAGE | OUTPUT LANGUAGE

| SEMEMIC STRATUM | SEMEMIC STRATUM |

| LEXEMIC STRATUM | LEXEMIC STRATUM |

| MORPHEMIC STRATUM | MORPHEMIC STRATUM |

INPUT TEXT | OUTPUT TEXT
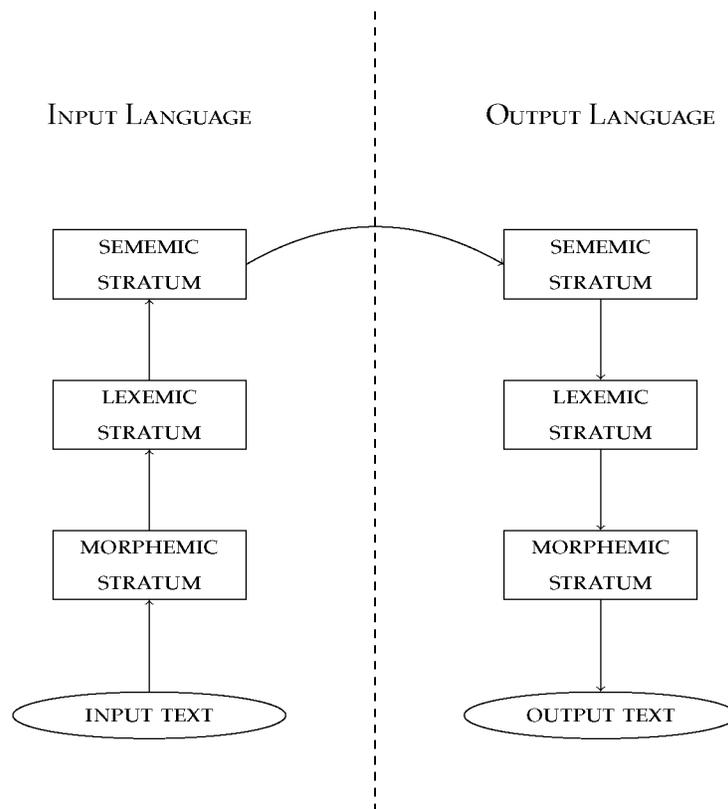
Figure 3.3: Stratificational interpretation of the translation process

existence of linguistic units, stratificationalists argued, will necessarily fail because it cannot properly characterize the linguistic structure of the input text. With its emphasis on morphemes – that is, linguistic units at roughly the level of the word – transformational grammar is not flexible enough to handle the sentence-level and supra-sentence-level information which is essential to accurate translation. Lamb describes unit-based approaches to machine translation, including those based on transformational and constituency grammars, as "word-for-word substitution plus doctoring" approaches, emphasizing that they are ill-equipped to move between different linguistic levels. It is "more than intuition that tells us that TG cannot handle translation in any systematic, non-ad hoc manner", wrote Makkai in 1975: stratificational work with network- and relationship-based linguistic theories has shown that it is "a logical impossibility to achieve any sort of adequate translation

[...] under TG assumption [sic]".[179]

With a high level of confidence in their approach to machine translation, stratificational grammarians emphasized the computer as an explanatory criterion. "It appears", said Lamb in a 1972 interview, "that the computer is flexible enough to act as a *hypothesis-testing device* for linguistic formulations".[180] Here, computers offer an unparalleled tool – one which can compensate for the limited capacity of the human brain to deal with large quantities of information: while a key test for any grammar is that it "should produce certain outputs and it should refrain from producing certain other outputs", such testing for any interesting grammar (that is, for any grammar complex enough to potentially produce an interesting portion of a language) surpasses the computational capacity of the human brain.[181] "The human being's mind plays tricks on him", Lamb explains: "he makes allowances, he makes assumptions, for certain formulations in the grammar which haven't been completely specified. The computer doesn't do that. Therefore, the computer is a valuable hypothesis tester".[182] The implementation of this hypothesis testing method, however, requires that linguistic theories be computerizable (that is, that the theories at stake can be represented in a format amenable to computer entry and manipulation). A linguistic theory which could not be computerized, stratificationalists argued, could not be tested – and hence did not have high explanatory value. Indeed, the stratificational community expended a great amount of effort through the 1960s on computerizing their network structures, primarily through the Yale Linguistic Automation Project.

While the emphasis of the stratificational school on computer applications helped that theory attract a number of followers in the 1960s, the stratificational community failed to popularize computerizability as an explanatory criterion in the broader American linguistic community. While transformational grammar was applied to machine translation at academic and corporate labs in the early 1960s, those at the heart of the theory's development – that is, the transformational grammarians at MIT – had little interest in computer

[179] Makkai, *Stratificational Solutions*, p 37.
[180] Parret, *Interview with Lamb*, p 218, emphasis in original.
[181] *Idem*, p 219.
[182] *Idem*.

applications. Chomsky was particularly dismissive of such applications, describing the view that transformational grammar "is somehow an outgrowth of an interest in the use of computers for one or another purpose, or that it has some other engineering motivation" as being "incomprehensible to me, and [...] in any event, entirely false".[183] Robert Stockwell also emphasized that transformational grammar is not "in any way geared to machines of any kind, either historically or in current development".[184] While transformationalists forced stratificational grammarians to meet their explanatory criteria, the reverse did not occur.

Given the lack of interest of transformationalists in computerizability as an explanatory criterion even as it was being promoted by stratificationalists, it is important to ask why the stratificational community failed to make computers more important in American linguistics. There are three primary reasons, each of which offers insight into the dynamics of theory choice in the 1960s. First, transformational grammarians were very effective at putting data concerns front-and-center within and beyond the borders of their theory. From the late 1950s on, transformationalists continually challenged rival theories to prove themselves capable of handling the TG dataset, which forced rivals to expend resources in an area in which they had only marginal interest. Data, transformational grammarians insisted, was at the heart of syntactic inquiry, and unarguably took precedence over secondary criteria such as computerizability. Stratificationalists did not come close to matching the level of polemics used by transformationalists to apply pressure to rivals (cf. Chapter 5). Further, with its dominance in linguistic pedagogy and textbooks through the 1960s, the transformational school trained young linguists to put data first, and fostered in linguistics students a disregard for practical applications of syntactic theory (cf. Chapter 4). As such, only a small number of linguistics students turned to stratificational theory, which had a reputation for being application-intensive.

Second, this decision on the part of transformational grammarians paid off when the optimism that had sustained machine translation funding from the 1950s to the mid-1960s

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[183]Chomsky, *Topics*, p 9.
[184]Stockwell, *The Transformational Model*, p 3.

turned to disappointment at the end of that period. In November 1966, the Automatic Language Processing Advisory Committee (ALPAC), answering to the National Research Council, released a report condemning American machine translation efforts: despite nearly two decades of work, the Committee asserted, "there has been no machine translation of general scientific text, and none is in immediate prospect".[185] "Unedited machine output from scientific text is decipherable for the most part", they continued, "but it is sometimes misleading and sometimes wrong [...] and it makes slow and painful reading".[186] The Committee recommended that American military and government support for machine translation be discontinued. This report reverberated around the American linguistics community and, as funding began to be cut off, the commitment of stratificational grammarians to machine translation reflected poorly on that theory. In his studies of machine translation in America, W. John Hutchins notes that the ALPAC report left the scientific and linguistics communities "with the firm conviction that MT [machine translation] had been a failure or, at best, very unlikely to be a useful technology".[187] In the years following the release of the report, he continues, "an interest in MT was something to keep quiet about; it was almost shameful" – a situation which turned many American linguists off machine translation and, consequently, off stratificational theory.[188]

Third, stratificational grammarians who were interested in computer applications of linguistic theory worked primarily on the boundaries of the linguistic field: they included machine translators, computer scientists, and mathematicians. In the stratificational community, for example, Peter Reich trained in chemistry, mathematics, and communication sciences, while Alexander Borgida trained and worked as a computer scientist. The theoretical commitments of those on the field boundary had limited effect on the theory-choice debates: they generally worked outside of linguistics departments, they rarely taught lin-

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[185]W. John Hutchins, "ALPAC: The (in)famous report," *MT News International* 14 (1996), p 113, John R. Pierce and John B. Carroll, *Language and Machines: Computers in translation and linguistics (A Report by the Automatic Language Processing Advisory Committee, Division of Behavioural Sciences, National Academy of Sciences, National Research Council)* (Washington, D.C., 1966).

[186]Hutchins, *ALPAC*, p 114.

[187]*Idem*, p 117.

[188]*Idem*, p 111.

guistics courses, and they did not publish in mainstream linguistics journals. Lamb pub-
lished his work on machine translation in the *Journal of Verbal Learning and Verbal Behav-
ior* and in dedicated stratificational grammar anthologies, and much other stratificational
work on language and computers was published in the anti-establishment LACUS journal
and in technical reports issued by Yale University. These publications had little influence
on internal linguistic debates and theory transmission (cf. Chapter 6). Taken together, the
transformational emphasis on data, the ALPAC report, and the concentration of stratifica-
tional efforts on the borders of the linguistic field meant that computer applications never
became an important explanatory criterion outside of SG itself.

Psychological Validity

To outside observers of American linguistics, now as in the 1960s, perhaps the most inter-
esting application of linguistic theory is towards increasing our understanding of human
learning and behavior patterns, or, more fundamentally, the human brain. "[L]ooked at
from the outside", wrote Edinburgh University's James Thorne in 1965, "Chomsky's great
innovation was to shift the center of interest from language as organized data to the or-
ganizing power capable of producing that data".[189] Linguists themselves have also long
been interested in psychological and behavioral correlates of their work, whether or not
it be their primary area of investigation – and, indeed, in the 1960s psychological validity
formed a central part of linguistic explanation for both transformational and stratificational
grammarians. Any proposed linguistic theory, these schools agreed, must have plausi-
ble psychological validity – that is, no theory should contradict known psychological or
neurophysiological principles, and theories should aim to improve our understanding of
language as a psychological construct.

Proponents of transformational grammar were quick to associate Chomsky's early writ-
ings with the search for psychological validity. "[C]ertainly in the long run by far the most
interesting implications of Chomsky's theories will be found in their cohesions with the

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[189]James Peter Thorne, "Review of Paul Postal's Constituent Structure: A study of contemporary models of
syntactic description," *Journal of Linguistics* 1/1 (1965), p 75.

field of human psychology", wrote Lees in his 1957 review of *Syntactic Structures*.[190] A decade later, the chief supporter of transformational grammar in Great Britain, John Lyons, noted that a key attraction of linguistics is "the peculiarly intimate relationship that is said to hold between the structure of language and the innate properties or operations of the mind", and that transformational theory is particularly important in this respect: "it is clear that an understanding of transformational grammar is essential for any philosopher, psychologist, or biologist who wishes to take account of man's capacity for language", he wrote, highlighting the spread of transformational ideas to other disciplines.[191] Closer to home, Jerrold Katz – then at the City University of New York – asserted in 1976 that "the profound contribution of the Chomskyan revolution was to [see] that transformations, mentalistically viewed, implied the existence of unobservable levels of grammatical structure, [and] that these had to be interpreted as constituting parts of the speaker's knowledge".[192] The transformational approach, these linguists proclaimed, would ultimately result in advancing our understanding of human psychology and of the human brain. Compatibility with known psychological features was an essential explanatory criterion – and compatibility with psychological features to be discovered in the future was a necessary guide to linguistic theorizing. This overt emphasis on psychological correlates, combined with strong anti-behaviorist rhetoric, proved key to attracting students to the transformational school, especially from within psychology and philosophy. However, there was a wide range of opinion within the transformational community as to how, in what form, and to what extent, psychological validity should play a role in explanation.

Chomsky's work displays a robust but carefully constrained approach to psychological validity. For Chomsky, it was vital that linguistic theories account for language creativity and acquisition – and any theory incapable of explaining the human ability to understand an infinite number of novel utterances failed a key explanatory test. Constituency grammar, he argued, "failed totally to come to grips with the 'creative' aspect of language use"

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[190] Lees, *Review of Chomsky*, p 406.
[191] Lyons, *Noam Chomsky*, p 4–5.
[192] Katz and Bever, *Empiricism*, p 22.

because it did not "appreciate the degree of internal organization and the intricacy of the system of abstract structures that has been mastered by the learner, and that is brought to bear in understanding, or even identifying utterances".[193] These limitations are revealed, he continues, "in such methodological conditions as the principle of separation of levels, the attempt to define grammatical relations in terms of co-occurrence, and, in general, in the emphasis on elementary procedures of segmentation and classification".[194] To account for language creativity, he concludes in one of his strongest critiques of constituency theory, "it is necessary to go far beyond the restricted framework of modern taxonomic linguistics and the narrowly-conceived empiricism from which it springs".[195] Transformational grammar was designed to correct these flaws by introducing mentalism into American linguistics, and by making explicit psychological claims concerning child language acquisition. Still, while creativity and acquisition were for Chomsky central to linguistic explanation, he did not – as we will see below – embrace the extension of psychological validity to include communicative ability.

Within the transformational community, the spectrum of views on psychological validity reached both above and below the position set out by Chomsky. At one extreme, Jerrold Katz and Paul Postal argued for a fully inclusive adoption of psychological validity as an explanatory criterion including, specifically, the role of language in communication. "[U]nderlying a speaker's ability to communicate", Katz wrote in 1964, "there is a highly complex mechanism [which] operate[s] to encode and decode verbal messages".[196] The aim of linguistic theorizing, he continued, is to "reveal[] the structure of this mechanism and explain[] the facts of linguistic communication by showing them to be behavioral consequences of the operation of a mechanism with just the structure that formulated theory attributes to it".[197] On the same note, Postal criticized constituency grammarians for failing to explain the actual use of language in the human context, and thus leaving "the ability of

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[193]Chomsky, *Current Issues*, p 113.
[194]*Idem.*
[195]*Idem*, p 114.
[196]Katz, *Mentalism in Linguistics*, p 128.
[197]*Idem.*

speakers to produce and understand sentences [...] a mystery".[198] Transformational theory, he implies, will shed light on these communicative abilities. Not all linguists, however, were comfortable with this turn toward psychology. "[T]o me [it] is not at all clear", Paul Garvin said at the 1962 Georgetown Round Table Meeting, "whether or not it is the purpose of a grammar to explicate behavior at all".[199] "I always thought that [...] behavior was described by psychology", he continued: "I do not think the aim of linguistics need to be to [...] account for the behavior of the native speaker".[200] This reaction is understandable in the context of the era, when linguistics departments were still new on the academic scene, and still fighting to be recognized as separate from psychology and anthropology; however, it was shared only by a minority of linguists. For most, the idea of contributing to the growing knowledge of the human brain was exciting and appealing, regardless of disciplinary intrusion.

While Chomsky's theory was in the 1960s and is still today widely considered to have brought to the forefront the question of the psychological reality of syntax, it was stratificational grammar which adopted psychological validity most fully as an explanatory criterion. Chomsky stated repeatedly that transformational theories of language were *not* meant to accurately represent human communicative abilities: a transformational grammar, he asserted in *Aspects*, is "not a model for a speaker or a hearer. It attempts to characterize in the most neutral possible terms the knowledge of the language that provides the basis for actual use of language by the speaker-hearer".[201] "When we say that a sentence has a certain derivation", he continues, "we say nothing about how the speaker or hearer might proceed, in some practical or efficient way, to construct such a derivation".[202] To stratificationalists, this was a restrictive and limiting basis from which to study language – a basis unable in principle to provide an explanatory account of communication. In response, stratificational grammarians put the goal of understanding language production

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[198] Hamp et al., *Transformational Theory (Panel I)*, p 4.

[199] *Idem*, p 9.

[200] *Idem*.

[201] Chomsky, *Aspects*, p 9.

[202] *Idem*.

and comprehension, or encoding and decoding, at the forefront of their work. They aimed to develop "an analogical model for the production and comprehension of speech, a theory that will not only define and describe the texts of the language, but will do so in a way that explains how human beings might themselves produce and understand such texts".[203] Language is not a strictly formal apparatus, stratificationalists insisted, but a living system which operates only through use. "The reason for the existence of language, after all", continues Geoffrey Sampson, "is that it is a device that, for the speaker, converts 'meaning' or thought into speech-sound, or into other physical phenomenon inherently unrelated to that thought (i.e. writing); and for the hearer or reader, performs the reverse conversion process. Stratificational theory aims to provide a fully formal account, in very general terms, of these two processes, called 'encoding' and 'decoding' respectively".[204]

Vital to understanding communication, stratificationalists emphasized, is the ability to account for the bidirectionality of language. By representing language structure as a set of static networks, stratificational theory displays no bias towards either directional process; that is, towards speaking or hearing. Only a theory endowed with this bidirectional explanatory capacity, Lamb argued, could hope to "characteriz[e] the speaker's internal information system that makes it possible for him to speak his language and to understand utterances received from others".[205] A key deficiency with transformational grammar, stratificationalists argued, was its dependence on formally irreversible transformational rules, which impose an implicit directionality, or speaker-oriented bias, on language. By operating on deep structures to produce surface structures, transformations are inherently unidirectional and structure-altering: while a given deep structure and set of transformations produces a unique surface structure, there is no algorithm for reversing the process and producing a unique deep structure from a given surface structure and set of transformations.[206] Since they destroy specific instances of syntactic structure upon application,

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[203]Algeo, *Stratificational Grammar*, p 7.

[204]Sampson, *Stratificational Grammar*, p 8.

[205]Sydney M. Lamb, "The Crooked Path of Progress in Cognitive Linguistics," in Richard J. O'Brien (ed.), *Report of the Twenty-second Annual Round Table Meeting on Linguistics and Language Studies* (Washington DC: Georgetown University Press, 1971), p 101.

[206]Sampson, *Stratificational Grammar*.

transformational rules prevent deep structures from being available for the decoding process. As a result, transformational grammar has "insuperable difficulty" accounting for decoding, or, for the speech-to-experience direction of the human language capacity.[207]

For Lamb, the primary flaw with the transformational approach – the flaw which prevents TG from being bidirectional – is the *process formulation* inherent to transformational theory. Any theory which assumes linguistic structure to consist of rules which act on sets of items – that is, which sees language as a process, not a network of relationships – will necessarily be structure-altering, and hence cannot be "realistic as applied to encoding and decoding".[208] Only by rejecting the item-and-rule approach and by introducing bidirectionality, stratificationalists argued, could linguistics be brought into "immediate and real contact with real human beings and their brain processes".[209] Seen in a broader framework, this dispute is at the crux of a disagreement over the place of communication and communicative ability in language study. Chomsky's rejection of performance data was widely criticized within the stratificational community and by non-linguists as a bastardized conception of language – one which divorced language from communication. "The picture that underlies [...] Chomsky's whole theory of language is that sentences are abstract objects that are produced and understood independently of their role in communication", wrote philosopher John Searle: "indeed, Chomsky sometimes writes as if sentences were only incidentally used to talk with".[210] Linguistics, the stratificationalists warned, would be mistaken to stop with the "adoption of a formalism and liberation of theory from a procedural orientation" provided by early transformational thought, and must broaden its outreach to "provide an account of the linguistic system which is realistic from the point of view of the function of language in communication, [and] the relation of language to the brain".[211] While stratificational theory provided a clear path for the second step, proponents of that theory argued that incorporating such a broad conception of explanation

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[207]Gleason Jr, *Organization of Language*, p 91.
[208]Lamb, *The Crooked Path*, p 104.
[209]Makkai, *Stratificational Solutions*, p 44.
[210]Searle, *Chomsky's Revolution*, p 29–30.
[211]Lockwood, *Stratificational Linguistics*, p 11.

into transformational theory would necessitate "a very considerable elaboration and complication", if it were possible at all.[212] Transformational grammar is "unworkable", the stratificationalists concluded, "if by a 'working grammatical theory' we mean a theory that squarely faces the task of informing us of what people actually do".[213]

Finally, with the proliferation of transformations in American syntax in the mid to late 1960s, stratificational grammarians began to question the plausible psychological reality of transformational rules. As transformations were increasingly designed to account for small, idiosyncratic datasets, stratificationalists accused them of having become "the result of linguists' games" – theoretical tools which failed to, as Wallace Chafe put it, "reflect anything that bears on what is going on in the mind of the speaker of the language".[214] While they were being applied to a vast range of syntactic phenomena, Lamb argued, transformations were "cognitively unrealistic" tools – tools which were limited by their unidirectionality, and which would never succeed in explaining how language is represented in the brain.[215]

At the 1971 Georgetown Round Table Meeting, Lamb accused transformationalists of saying, in effect, "cognitive reality be hanged, look at all the things we can do with transformations!", and forging full steam ahead with little regard for the psychological inadequacy of their theory.[216] As well as capturing Lamb's frustration with transformational theory, this outburst also captures the proliferation – some said promiscuity – of transformations in 1960s American syntax. Transformational grammarians were indeed saying 'look at all the things we can do with transformations!' – and with good reason: through the 1960s, they produced transformational accounts of a great many syntactic processes in many languages, and were buoyed by a shared feeling that all syntactic phenomena could, and eventually would, be explained by means of transformations. Whereas stratificational grammarians spread their efforts among a wide variety of topics from grammaticality con-

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[212] H.A. Gleason Jr, "Realization, Transformations, and Filters," in Edward L. Jr. Blunsitt and Robert J. Di Pietro (eds.), *Third LACUS Forum* (South Carolina: Hornbeam Press Inc., 1976), p 493.

[213] Makkai, *Stratificational Solutions*, p 37.

[214] Parret, *Interview with Chafe*, p 4–5.

[215] Lamb, *The Crooked Path*, p 105.

[216] *Idem.*

tinua to idioms to discourse style, transformational grammarians made rapid progress in one area – the development of transformational accounts of specific syntactic patterns.

Instead of aiming to match the stratificational effort to explain communicative ability, transformationalists labeled communication as outside of the realm of linguistic inquiry. It belonged to performance, they argued, not to competence. This labeling both protected transformational theory and reflected poorly on stratificational theory: because communicative ability was a performance matter, it was not the proper role of transformational grammarians – who were interested specifically in linguistic competence – to study it; and if stratificationalists insisted on studying it, this demonstrated their lack of understanding of the competence-performance distinction and, consequently, of the proper aims of linguistic inquiry. This difference in approach was essential for attracting students to transformational grammar: in that school, projects were clear-cut, data was based on empirical generalizations, results could be achieved by newcomers in relatively little time, and those results would immediately become part of a large and increasingly prolific body of work. While the stratificational commitment to a rich version of psychological reality struck some as "smack[ing] of hubris, or a least of chutzpah", the transformational emphasis on syntactic data made progress in that theory seem immediately at hand.[217]

<p style="text-align:center">* * *</p>

Transformational and stratificational grammarians both placed high value on external explanatory criteria through the 1950s. Transformationalists were interested primarily in a restricted account of psychological validity, while stratificationalists emphasized machine translation, computerizability, and a rich account of psychological validity. The wide view of external validity adopted by the stratificational community certainly attracted a number of adherents to that theory – those interested in computer applications of linguistic theory. It also, however, had detrimental effects on the theory: first, stratificational grammarians failed to make computers matter as an explanatory criterion outside of their own community. By not expending resources in this area and by de-emphasizing it in the classroom,

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[217] Algeo, *Stratificational Grammar*, p 7.

transformational grammarians fostered in linguistics students a disregard for computer applications. Second, in the wake of the ALPAC report, the stratificational commitment to machine translation began to look like a poor choice. Third, the rich view of psychological validity promoted by stratificationalists led them to stretch their efforts and resources over a vast set of areas. In contrast, transformational grammarians maintained a restricted stake in psychological validity and achieved rapid progress in one area – accounting for syntactic data. By labeling communicative ability as a performance matter, transformationalists both provided a theory-based reason for not studying it, and simultaneously cast doubt on its validity as an aim of linguistic research. In short, transformational grammar established a reputation as *the* linguistic theory which was focused, well-organized, and most likely to succeed.

<center>* * *</center>

When it first burst on the scene in 1957, transformational grammar immediately named new explanatory criteria for American linguistics: it set high standards for linguistic theories to meet in areas ranging from data to formalization to power to psychological reality. The transformational program was ambitious and exciting, and as transformationalists delivered on their goals, they created a momentum of optimism and progress – a momentum which was essential to attracting students. By keeping their explanatory criteria at the forefront of discussion, by continually criticizing rival theories for failing to meet those criteria, and by making steady progress on syntactic data through the 1960s, transformational grammarians dominated in the explanatory arena. Constituency grammar and stratificational grammar were forced to play catch-up, and never managed to devote their full resources to their own priorities. "[T]ransformational grammar has raised some important issues and suggested some compelling answers", wrote John Waterman in 1970, and "even its severest critics must concede that the very notion of the transformation itself [...] has provided grammarians with a most powerful tool": indeed, the very success of the transformation-as-tool in the early 1960s set the bar high and forced rival theories to

tackle the TG dataset.[218] By dominating in the explanatory arena, transformational grammar took a significant step towards dominating American academic syntax. Importantly, the role of explanation in the theory-choice debates of the 1960s can only be understood when explanation is considered from the perspective of the working linguists of the era.

The transformational monopoly on explanation did not escape proponents of other theories at the time. "[I]t is clear", wrote Sampson in 1970, that "the goals [of TG] are not the only possible goals the student of language may set himself".[219] He draws a distinction between generative and communicative approaches to language, arguing that "transformational-generative theory may be characterized as by far the most sophisticated theory of generative language-description; whereas stratificational theory is the most highly developed theory known to me of communicational descriptions of language".[220] "In this light", he concludes, "the two theories [...] are seen to be not so much incompatible as irrelevant to one another".[221] This view was echoed by Zellig Harris when he began to demonstrate his string linear grammars in the mid-1960s: "[t]here are several ways of analyzing the structure of sentences", he wrote, "and the applicability of one does not falsify the others".[222] Yet in the workaday world of linguistics, and of scientific disciplines more generally, non-compatible theories in the same field can rarely coexist for long: socio-professional competition dictates otherwise. By dominating explanatory criteria, transformational grammar shifted the balance of expectations in the linguistic field so that any theory hoping to be considered explanatory would have to meet the criteria dictated by TG. This is the great advantage transformational grammarians enjoyed by successfully setting the explanatory stakes so high, and the great challenge faced by rival syntactic theories through the 1960s. It is, ultimately, a key factor in the rise of transformational grammar over rival theories.

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[218]John T. Waterman, *Perspectives in Linguistics*, 2nd edition (Chicago: The University of Chicago Press, 1970), p 106–107.
[219]Sampson, *Stratificational Grammar*, p 8.
[220]*Idem*, p 10.
[221]*Idem*.
[222]Zellig S. Harris, "Transformations in Linguistic Structure," *Proceedings of the American Philosophical Society* 108/5 (1964), p 418.

4 Syntax in the Classroom

"Few libraries held really broad basic collections", went one complaint.[1] Students faced a "lack of easily accessible source material", went another – and there was "not a great wealth of textbooks available", went a third.[2] This was the 1950s, when the wartime successes of American linguistics began to shower unforeseen consequences on the discipline. Following the War, American academic linguistics enjoyed huge growth: universities across the country founded linguistics departments, students scrambled to do graduate work in linguistics, and undergraduate classrooms swelled. Riding on the coattails of the Intensive Language Project, linguistics was elevated to an important and timely subject – one which was active on the front lines of the Cold War, and one which universities were very interested in adding to their basket. These trends perpetuated and intensified in the 1960s, when – in response to the Soviet launch of Sputnik on October 4, 1957 – the American military began to pour money into linguistics research. Between 1960 and 1970, the number of American universities offering degrees or concentrations in linguistics increased more than fourfold; the number of linguistics Ph.D.s granted by American institutions more than tripled; and enrollment in doctoral programs grew by fivefold.[3]

This rapid growth brought with it a host of pedagogical problems: in the 1960s, large numbers of students waited to be taught at the undergraduate and graduate levels, but

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[1] Eric P. Hamp, Fred W. Householder and Robert Austerlitz (eds.), *Readings in Linguistics II* (Chicago: The University of Chicago Press, 1966), p vi.

[2] Martin Joos (ed.), *Readings in Linguistics I: The development of descriptive linguistics in America, 1925–56*, 4th edition (Chicago: The University of Chicago Press, 1968), p u1, Byron W. Bender, "Fred Walter Householder," *Language* 73/3 (1997), p 526.

[3] Newmeyer, *Linguistic Theory in America*, p 55, Mary Evans Hooper, National Center for Educational Statistics, Higher Education Surveys Branch, *Earned Degrees Conferred (Summary Data)*, (Washington, DC, 1972).

there were no set or standard curricula or courses in syntax, few syntax-oriented text-books, and little easily-available research material. The optimism and confidence which had infused American linguists since the War was tempered by the new pedagogical needs which weighed heavily on the discipline through the 1960s. University and student buy-in represented a bright future for academic linguistics, but it also presented immediate challenges. Textbooks needed to be written, courses planned, programs designed, and library collections built. These pedagogical challenges would shape the American academic linguistics profession in the 1960s and have long-term effects on theory-choice. As linguists established a pedagogical structure for their discipline, they influenced the training and commitments of the next generation: what young linguists were taught was crucial to determining their later theoretical leanings. The syntactic theory which could capitalize on the pedagogical market would gain a great advantage in the theory-choice debates.

The importance of pedagogy and training in theory transmission has recently been recognized in the history of science by, among others, David Kaiser's work on postwar theoretical physics in America and Andrew Warwick's study of mathematical physics at Cambridge.[4] Emphasizing the need to understand the relationship between institutional structure, training methods, and the dispersion of theoretical apparatus, Kaiser argues that tacit knowledge – spread by personal contact, and not learnable from written sources – was essential to the spread of Feynman diagrams through America, and throughout the world, in the post-World War II decades. Warwick's investigation of pedagogical methods at Cambridge reveals that new knowledge in mathematical physics was shaped and influenced by the technical skills taught to undergraduate students. In this chapter, I explore the relationship between pedagogy and theory-choice in 1960s American linguistics. This period of linguistics has a defining feature which sets it apart from the theoretical physics studied by Kaiser and Warwick and, indeed, from most other disciplines: through the 1960s, transformational grammarians actively worked to keep their research out of mainstream journals and maintained a tight network of communicants. Those outside of the transfor-

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[4]Kaiser, *op. cit.*, Warwick, *op. cit.*.

mational circle found TG literature hard – and sometimes impossible – to access. In this environment, pedagogy – and especially textbooks, which were widely distributed and available – played a vital role in theory transmission. While transformational grammarians valued private, underground knowledge, their theory still dispersed across America – largely because of its prevalence on the pedagogical scene.

This chapter opens with a discussion of disciplinary growth in American linguistics in the postwar years, setting the stage for an exploration of the consequent pedagogical challenges (section 4.1). By looking at textbooks (section 4.2), canonical examples (section 4.3), and notation (section 4.4), I argue that transformational grammar emerged as an approach to syntax which was, from the pedagogical perspective, easier to teach, learn, and use than its rivals. In section 4.2, I show that transformational theory captured the textbook market early in the 1960s, and so doing, captured a generation of young linguists. Trained in the transformational paradigm, this generation developed a transformational world-view: to them, the transformation-as-tool was the standard way of mediating between linguistic theory and the world of syntactic phenomena. Transformational grammar enjoyed this success on the textbook scene both because of the enthusiasm with which transformationalists approached textbook writing, and because transformations were conceptually more elegant than competing syntactic tools. In section 4.3, I argue that transformational grammar also provided canonical examples – structured, repetitive and satisfying demonstrations – which were put to effective use by textbook authors, giving transformational theory a pedagogical edge over rivals. These canonical examples gave linguists a foothold to enter what was otherwise a difficult and complex theory. In section 4.4, I investigate the visual presentation of the three main syntactic theories of the 1960s. I show that the notational and diagrammatic techniques used in transformational grammar made that theory particularly amenable for both teaching and research. In contrast, rival theories suffered from unrevealing and overly complex notational techniques, which came to overshadow their theoretic content. I conclude that the transformation provided a better pedagogical tool than its rivals at a time when linguistic pedagogy was of real concern and, consequently,

that transformational grammar emerged as the most efficient mechanism for handling the rapid growth in enrollment in university linguistics programs of the 1960s.

4.1 Disciplinary Growth

The 1950s and 1960s saw disciplinary growth in linguistics on all fronts: membership and interest in professional societies blossomed; conferences and symposia became commonplace; departments and programs were established at universities across the country; and students flocked to join. At the Linguistic Society of America, membership ballooned from just over 800 in 1950 to just over 4300 in 1970, as illustrated in Figure 4.1.[5]
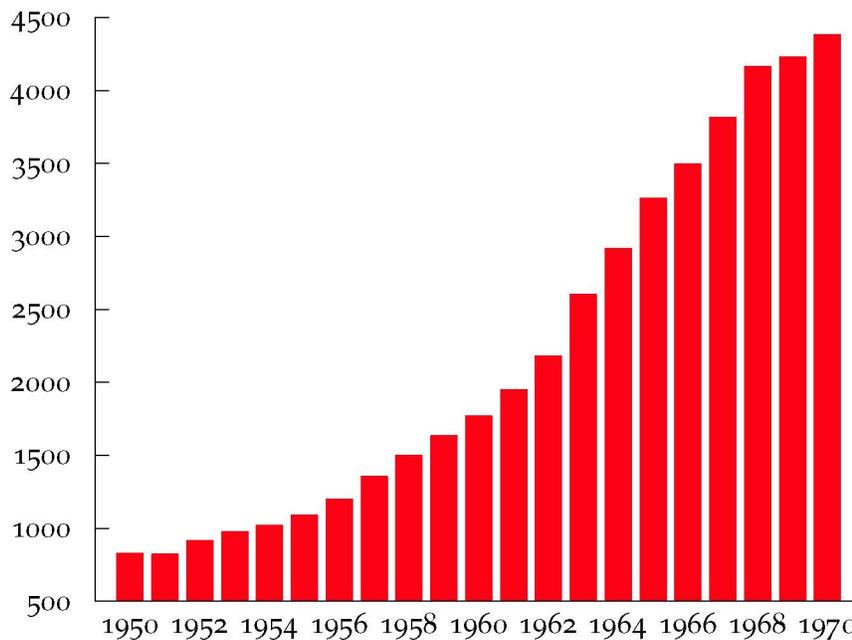
Figure 4.1: Membership of the Linguistic Society of America, 1950–1970

In these years, the Linguistic Institutes run by the Linguistic Society of America each summer drew large numbers of participants: when Chomsky lectured at the 1966 UCLA Linguistic Institute, he gave his presentation in the largest lecture room available – with 320

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[5]LSA, *op. cit.*

seats – and "[e]very seat was always taken, and the aisles were usually filled too".[6] While

Chomsky was a particularly prominent draw, high participation at the Linguistic Institutes

was the norm through the 1960s. Even as they lost their monopoly on linguistics teaching to

universities, the Linguistic Institutes maintained their relevance by attracting sought-after

speakers and providing an environment rich in debate and discussion. Offering courses

in a wide variety of areas from syntactic theory to field methods to dialectology, and with

a reputation as *the* place to be seen and heard, the Linguistic Institutes drew large crowds

through the decade.

The Linguistic Society of America was, however, no longer the only game in town. New

professional societies – many based at universities – sprang up in the 1950s and 1960s, and

linguistics conferences were sponsored by a variety of organizations. Founded in 1951, the

Chicago Linguistics Society held monthly meetings (often featuring a guest speaker) in the

Chicago area through the 1950s. In 1964, it hosted its first regional conference – a tradition

continued annually ever since. By the end of the decade, the conference organizers had no

trouble selling over 1000 copies of each year's conference proceedings. At other universi-

ties from Yale to Berkeley small societies hosted regular meetings, providing faculty and

students with a sense of community. On the national scale, the founding of the Association

for Machine Translation and Computational Linguistics in 1962 marked a key step towards

subject-specialization within the theoretical linguistics community.

Focused conferences were held through the 1960s on subjects ranging from language

universals to syntactic theory to machine translation to language learning. At the influen-

tial Georgetown Round Table Meetings on Linguistics and Language Study, held annually

at Georgetown University from 1950 on, participation increased from just over 170 regis-

trants in 1959 to over 400 a decade later.[7] Linguistics was also featured by the American

Mathematical Society, which in 1960 hosted the *Structure of Language and Its Mathematical*

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[6]Stockwell, *To Linguistics and Back Again*, p 241.

[7]Richard S. Harrell (ed.), *Report of the Tenth Annual Round Table Meeting on Linguistics and Language Studies, 1959* (Washington DC: Georgetown University Press, 1959), p 165–169, James E. Alatis (ed.), *Report of the Twentieth Annual Round Table Meeting on Linguistics and Language Studies, 1969 (Linguistics and the Teaching of Standard English to Speakers of Other Languages or Dialects)* (Washington DC: Georgetown University Press, 1969), p 263–267.

Aspects symposium in Providence (Rhode Island).

On the university scene, growth was also rampant. As linguistics came into its own as a discipline separate from English, classics, and anthropology, linguistics departments were established at universities across America. Beginning immediately after the War, the first such department was founded in 1946 at the University of Pennsylvania, and was soon followed by new linguistics departments at Berkeley (1953), MIT (1961), Indiana (1964), Illinois (1965), University of Texas at Austin (1965), UCLA (1966), and Ohio State (1966), among others. Once founded, these departments grew quickly, often hiring at least one additional linguist per year. In 1962, fewer than 30 American universities offered degrees or concentrations in linguistics. By 1965, this number had doubled; a year later, nearly 100 institutions offered linguistics degrees; and by 1970 prospective linguistics students could choose from degree programs at more than 135 American universities.[8] While in the 1940s "only a handful of persons" held Ph.D.s in linguistics, between 1955 and 1970 American universities conferred more than 750 doctoral degrees in linguistics.[9] In the same time period, more than 2300 students earned masters degrees in linguistics, and 1200 students earned bachelor's degrees in the subject. The growth of linguistics degrees is illustrated in figure 4.2.[10] The relatively small number of bachelors degrees conferred in comparison to masters and doctoral degrees reflects the makeup of 1960s linguistics departments: many did not offer linguistics as a stand-alone undergraduate program, but rather required undergraduates to take linguistics courses as part of a broader social science or humanities degree. At the graduate level, however, linguistics was a focused subject of study.

By the end of the 1960s, linguistics was firmly established in American academia. In the 1969–1970 academic year, 70 American universities had a standalone linguistics department, linguistics center, or committee on linguistics.[11] At these institutions, linguistics

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[8]Allene Guss Grognet, "United States Current Resources in Linguistics," in Thomas A. Sebeok (ed.), *Current Trends in Linguistics (Linguistics in North America)* (The Hague: Mouton, 1973), p 1450.

[9]Joos (ed.), *Readings in Linguistics I*, p v, Hooper, *op. cit.*.

[10]Hooper, *op. cit.*

[11]Allene Guss Grognet and Judith Brown, *University Resources in the United States and Canada for the Study of Linguistics: 1969–1970*, 6th edition (Washington DC: Center for Applied Linguistics and the Secretariat of the Linguistic Society of America, 1970).
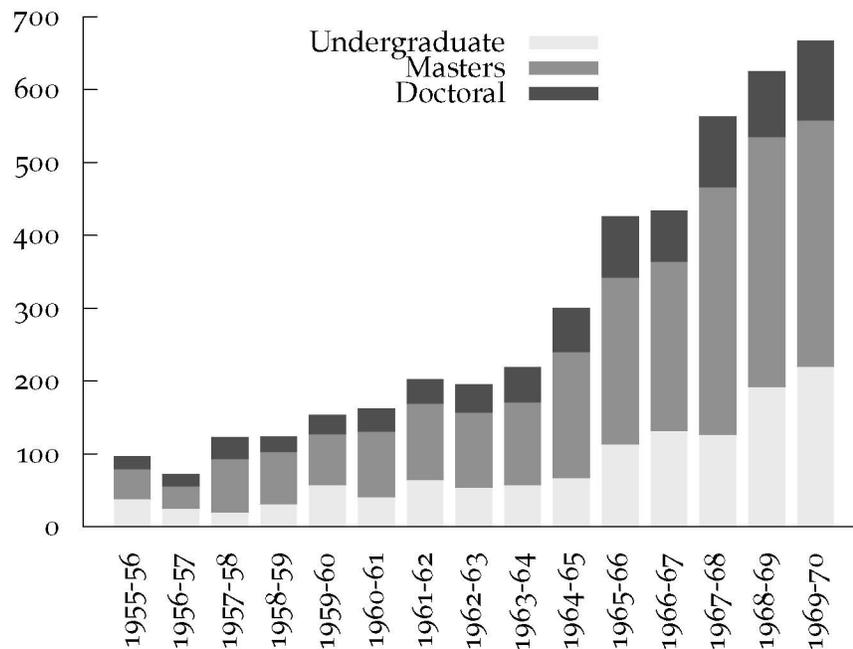
Figure 4.2: Linguistics degrees conferred at American universities, 1955–1970

enjoyed some level of administrative autonomy. Nearly 40 universities had 10 or more full-time teaching staff in linguistics, and a full 100 universities offered at least one course in syntax (or in morphology and syntax), highlighting the centrality of syntactic study to academic linguistics in the 1960s.[12] While these numbers indicate intensive growth over a two-decade period, linguistics did not challenge closely related disciplines in size: from the point of view of doctoral degrees conferred, linguistics' 109 in the 1969–1970 academic year were overshadowed by anthropology's 215, philosophy's 359, and economics' 794.[13] These disciplines, however, were present at American universities from the early 1900s. It is the growth *rate* in linguistics in the 1950s and 1960s, coupled with the pre-professional nature of the discipline prior to World War II, which is exceptional.

The expansion of linguistics at American universities was made possible by the massive funding injected by the American military and civilian government organizations in the

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[12] *Idem.*
[13] Hooper, *op. cit.*

wake of Sputnik. This investment, James McCawley recalls, "made it possible for many universities to start linguistics programs that otherwise would not have been started or would not have been started so early, or to expand existing programs much further than they would otherwise have been expanded".[14] The impact of this funding was nowhere more visible than at the University of Texas at Austin. Prior to 1957, Winfred Lehmann – a central figure at the university for over half a century – recalls, Austin was only halfheartedly committed to linguistics: they attracted Indo-European and classical linguist Henry Hoenigswald early on, but promptly lost him to the University of Pennsylvania. The lack of a graduate program meant that doctoral students in linguistics were awarded Ph.D. degrees in Germanic languages, including "one [student] who didn't know a word of German".[15] It wasn't until the National Defense Education Act of 1958 provided the Texas university with funding earmarked for linguistics that the discipline got off the ground. Passed by Congress in response to the Soviet launch, the National Defense Education Act provided university-level funding for mathematics, sciences, and language studies. The additional funding allowed the University of Texas at Austin to organize conferences and symposia on linguistics, invite guest speakers from America and Europe, and host two consecutive Linguistic Institutes. Lehmann and his colleagues were able to establish a department for linguistics in 1965 – "almost an impossible event at the conservative University of Texas", Lehmann emphasizes, and one wholly reliant on government funding.[16] A few years later, a National Science Foundation Development Grant permitted the new department to hire "scholars in syntax, semantics and experimental phonetics, as well as in psycholinguistics".[17]

While celebrated by many academic linguists, the tremendous growth of the 1950s and 1960s precipitated three main pedagogical problems. First, as the wartime legacy of Amer-

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[14] McCawley, *Madison Avenue*, p 233.
[15] Winfred P. Lehmann, "Linguistics at Wisconsin (1937-41) and Texas (1949-??): A retrospective view," in Boyd H. Davis and Raymond K. O'Cain (eds.), *First Person Singular: Papers from the Conference on an Oral Archive for the History of American Linguistics (Charlotte, N.C., 9–10 March 1979)* (Amsterdam: John Benjamins B.V., 1980), p 187.
[16] *Idem*, p 188.
[17] *Idem*, p 189.

ican linguistics, the cachet of machine translation in the Cold War era, and the growing prominence of Chomsky's rejection of behaviorism attracted unprecedented numbers of students to linguistics, universities faced the problem of teaching these students. Newly established linguistics departments were thrown in at the deep end and forced to create – virtually from scratch – entire undergraduate and graduate curricula. Faced with huge expectations, these departments had few resources to draw on and few models to follow. Second, students at universities with new linguistics programs had difficulty accessing source material, including journal articles and research results. This lack of source material was compounded by a lack of textbooks addressing the new core of linguistic theory – syntax. The wide open textbook market would prove central to determining the theoretical training and commitments of young linguists. Finally, there was a pressing need to fill the newly founded linguistics departments with teaching staff. Jobs were available across the country, and universities commonly courted and hired graduate students even before they had completed their doctorates. Linguistics departments were soon flooded by young staff eager to perpetuate their theory choices through teaching and mentoring. In the following sections, we investigate the influence of these pedagogical challenges on the theory-choice debates of the 1960s.

4.2 Textbooks

As the Texas Board of Education revisited the teaching of evolution in the state's science curriculum in early 2009, moderates scored a partial victory when they voted to drop the 20-year-old requirement that science classes explore the strengths and weaknesses of all theories – evolution and creationism alike. This victory was tempered by conservative causes, championed by Board of Education chair Don McLeroy, mandating teachers to highlight unexplained aspects of the fossil record. The real winners, however, are American science textbook publishers. The decisions of the Texas Board, the *New York Times* wrote in January 2009, "will have consequences far beyond Texas": as one of the largest textbook buyers in the country, the state's science decisions "will influence the writing of

the next generation of biology texts".[18] Just as the content of America's high school biology textbooks is today determined by the leanings of the Texas Board of Education, the content of America's university-level linguistics textbooks was in the 1960s determined by the theoretic commitments of a small group of linguists. And just as the biological theories American high school students learn are determined by those textbooks, so too the syntactic theories American linguistics students learned in the 1960s were determined by the linguistics textbooks available. Textbooks have the capacity to influence and shape the learning of generations of students – a reality well understood by moderate and conservative campaigners alike, and a reality omnipresent on the American academic linguistics scene of the 1960s.

In the 40 year span between 1930 and 1970, three streams of American linguistics textbooks can be identified. Prior to the Second World War, Leonard Bloomfield's *Language* (1933) was the staple university-level text. In its elaboration of the Descriptivist linguistics program, it was unrivaled. An entire generation of linguists – from Charles Hockett to Fred Householder to Sydney Lamb to Robert Stockwell – was trained, formally or informally, with Bloomfield's manual. "There were no textbooks" other than *Language* in the pre-War years, Martin Joos wrote, and hence Bloomfield's text served in courses running the gamut of language study, from literature to the history of English to the scientific investigation of language.[19] The monopoly of Bloomfield's *Language* was broken in the 1940s by the appearance of a small number of other linguistics texts, including Kenneth Pike's *Phonetics* (1943) and *Phonemics* (published in 1947, but available in mimeograph form from 1943) and Eugene Nida's *Morphology* (published in short in 1946, and in long in 1949).[20] Written explicitly for the training and use of missionaries, translators, and anthropologists, these "[h]ighly practical" manuals were adopted at universities as well as at SIL International and

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[18] James McKinley, "Split Outcome in Texas Battle on Teaching of Evolution," *New York Times* 24 January (2009).

[19] Joos (ed.), *Readings in Linguistics I*, p 37.

[20] Kenneth Pike, *Phonetics: A critical analysis of phonetic theory and a technique for the practical description of sounds* (Ann Arbor: University of Michigan Publications, 1943), Kenneth Pike, *Phonemics: A technique for reducing languages to writing* (Ann Arbor: University of Michigan Publications, 1947), Eugene A. Nida, *Morphology: The descriptive analysis of words* (Ann Arbor: University of Michigan Press, 1949).

the American Bible Society, where Pike and Nida held leading roles.[21] With their emphasis on learning through problem-solving, they offered a hands-on counterpart to Bloomfield's text, and were particularly valuable for training linguists in the field methods necessary for recording and analyzing unknown languages. Even with these additional textbooks, at the end of the 1940s there was still "not a great wealth of textbooks available" for linguists, and university students could not depend on textbook sources.[22]

With the rise of syntax in the 1950s, Pike's and Nida's phonologically and morphologically-oriented texts were soon insufficient for the needs of the discipline. Between 1951 and 1958, these new needs were met by the release of four broad-based textbook treatments of linguistics. The texts – Zellig Harris' *Structural Linguistics* (1951), H.A. Gleason's *An Introduction to Descriptive Linguistics* (1955), Archibald Hill's *Introduction to Linguistic Structures: From sound to sentence in English* (1958), and Charles Hockett's *A Course in Modern Linguistics* (1958) – made the teaching of linguistics "a far simpler and dramatically changed problem from the days when Bloomfield's *Language* had to serve as the sole tool for all purposes".[23] These textbooks shared a common purpose: they were intended to provide a comprehensive up-to-date alternative to Bloomfield's *Language* for those teaching in the university classroom, and to address syntax as part of a complete treatment of linguistic science. Indeed, they were developed directly from university courses the authors taught at the University of Philadelphia, the University of Texas at Austin, the Hartford Seminary Foundation, and Cornell. Harris' text matched most closely the structure set out by Bloomfield – John Waterman has described it as a "rigorous and book-length attempt to organize all of American structuralism into a single body of theory and practice" – and Hockett saw his book as "essentially a commentary on and updating of Bloomfield's *Language*".[24] While these texts were products of the Bloomfieldian program, the later ones were tempered by a relaxation of Descriptivist philosophical tenets. They packaged and presented 1950s linguistics to an expectant community which was looking for replacements for Bloomfield's

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[21]Waterman, *op. cit.*, p 97, Bender, *op. cit.*.
[22]Bender, *op. cit.*, p 562.
[23]Hamp, *General Lingustics*, p 166.
[24]Waterman, *op. cit.*, p 98, James W. Gair, "Charles F. Hockett," *Language* 79/3 (2003), p 603.

manuscript, which itself celebrated its 25th anniversary in the year of publication of Hill's and Hockett's texts.

Despite the increased importance of syntax in the post-World War II years, these were not syntax textbooks *per se*; rather, they were sweeping works which aimed to cover all aspects of language study, including syntax, in a single volume. The authors all treated syntax with the leading paradigm of the day – immediate constituency grammar – and the optimism and confidence with which American linguists approached syntax in the 1950s infused the texts. Responses were strong: Harris' book was described as being "universally laud[ed]"; Gleason's text was said to "stand[] up under the heavy wear and tear of student use"; and Hockett's book was "widely used".[25]

While these textbooks gave a boost to university teaching, linguistics programs and departments were still hampered by a "lack of easily accessible source material for students".[26] In the 1950s, few university libraries had comprehensive linguistics collections, and linguists working or studying outside of main centers had difficulty obtaining mainstream journals.[27] In response, the American Council of Learned Societies – the body which had been responsible for the mobilization of linguists during the War – sponsored the publication of a linguistics reader. Edited by the University of Wisconsin's Martin Joos, the reader was first published in 1957. Joos was a veteran of large-scale linguistics projects, from fieldwork for the *Linguistic Atlas of the United States and Canada* to wartime work as a cryptanalyst for the United States Signal Security Agency to the production of English-language learning textbooks for speakers of Serbo-Croatian.[28] The 1957 reader contained 43 articles chosen by a poll of linguistics teachers and intended to "illustrate[] the development of descriptive linguistics in America during its formative period [1925–1956]".[29] Ranging from Morris Swadesh's early study of phonemics (*The Phonemic Principle*, 1934) to

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[25]Eugene Dorfman, "Review of Zellig Harris' Methods in Structural Linguistics," *Modern Language Journal* 38/3 (1954), p 159, Frederick B. Agard and William G. Moulton, "Review of H.A. Gleason's An Introduction to Descriptive Linguistics," *Modern Language Journal* 32/3 (1956), p 470, Gair, *op. cit.*, p 603.

[26]Joos (ed.), *Readings in Linguistics I*, p u1.

[27]Hamp, Householder and Austerlitz (eds.), *Readings in Linguistics II*.

[28]Archibald A. Hill (ed.), "Martin Joos (Obituary)," *Language* 55/3 (1979).

[29]Joos (ed.), *Readings in Linguistics I*, p u1.

Charles Hockett's classic discussion of arrangements and processes in linguistic analysis (*Two Models of Grammatical Description*, 1954), the articles captured the essence of Bloomfieldian linguistics. The reader was met with immediate and overwhelming demand, even if some disagreed with the editorial comments Joos appended to the articles: the first edition was exhausted within a year, and the reader had reached four editions by 1968.[30] First released in the same year as the publication of Chomsky's *Syntactic Structures*, this reader represents the culmination of the American Descriptivist linguistics program.

Together, the new textbooks and Joos' reader satisfied the growing demand for pedagogical and source materials in the 1950s. But by the end of the decade it became apparent that these works, too, would need to be replaced. The playing field had again changed: with the crowning of syntax as the king of linguistic subdisciplines and the emergence of several competing syntactic frameworks, no university linguistics program could omit the teaching of syntax. As early as 1959, Fred Householder argued that Hockett's book "could have been improved by the use of transformations and the generative approach" – a call which would be repeated and broadened as syntactic rivalries increased.[31] As excitement and interest in syntax grew, the lack of emphasis on syntax in major pedagogical works was no longer acceptable.[32]

The need for syntax-specific teaching tools was answered in the 1960s with the publication of a plethora of syntax textbooks. Eschewing the all-inclusive model of the 1950s, these new texts focused specifically on syntax, offering students problem-based instruction in the analysis of sentence-level phenomena. These textbooks would be vital to the success of Chomsky's linguistic program: by dominating the early textbook market, transformational grammarians brought their theory to the pedagogical mainstream, and did so in a manner detrimental to rival theories.

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[30] George L. Trager, "Review of Martin Joos' (ed.) Readings in Linguistics I: The development of descriptive linguistics in America, 1925–56," *Studies in Linguistics* 13 (1958).

[31] Fred W. Householder, "Review of Charles F. Hockett's A Course in Modern Linguistics," *Language* 35/3 (1959), p 506.

[32] Bolinger, *Syntactic Blends*, Fred W. Householder, "Review of Ronald W. Langacker's Language and Its Structure: Some fundamental linguistic concepts," *Language* 45 (1969), Stanley Starosta, "Review of John Lyons' Introduction to Theoretical Linguistics," *Language* 47/2 (1971), Kenneth L. Hale, "Review of Emmon Bach's An Introduction to Transformational Grammars," *International Journal of American Linguistics* 31/3 (1965).

The most successful academic syntax textbook of the 1960s – and the first widely dis-
tributed transformational grammar text – was Emmon Bach's *An Introduction to Transfor-
mational Grammars*, published by Holt, Rinehart and Winston in 1964. Born in 1929 in Ku-
mamoto, Japan, Bach received all of his degrees from the University of Chicago, culminat-
ing with a doctorate in 1959. His textbook grew out of a syntax course he taught at the
University of Texas at Austin, where he worked in the early part of his career. The impetus
for the book, Bach wrote, was to rectify the "lack of a simple step-by-step presentation of
the concepts, techniques, and problems" of transformational grammar.[33] He intended the
text as an easily accessible introduction to counter the existing transformational literature,
which he accurately described as "buried in fairly technical discussion – often difficult to
obtain – or masked in polemics".[34] Uncommonly for the era, Bach's book was indeed rela-
tively free of polemics – a feat made possible by Bach's lack of direct association with MIT,
from where the polemical movement was emerging (cf. Chapter 5).

As the earliest mainstream transformational textbook, Bach's book had great potential to
transform university classrooms – and it lived up to this potential, enjoying wide distribu-
tion and praise. "[L]inguists have felt for some time the need of an introductory book on
transformational theory, which would summarize all that has been discussed so vividly
during the past half dozen years", wrote Fred Peng in a 1965 review: "[a]t last, such a
book exists. It not only gives the reader a systematic and well-organized presentation of
transformational theory, but also fulfills the need for a simple step-by-step presentation of
the concepts, techniques and problems of the theory".[35] The book rectifies "[t]he lack of
a general, up-to-date text on the subject" which plagued the discipline in the early 1960s,
wrote Kenneth Hale, then at the University of Arizona (Tucson), in *The International Journal
of American Linguistics*, and "constitutes a successful attempt to present, in a single volume,
a coherent account of the transformational generative framework".[36] Hale was one of sev-

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[33]Bach, *An Introduction to Transformational Grammars*, p v.

[34]*Idem.*

[35]Fred C. C. Peng, "Review of Emmon Bach's An Introduction to Transformational Grammars," *Lingua* 13
(1965), p 291.

[36]Hale, *Review of Bach*, p 265.

eral linguists who expressed their pleasure at the lack of polemics in Bach's book. The textbook, Christine Montgomery concurred in her *Language* review, was free of the "messianic tone which has characterized much of the transformational literature" – a deliberate decision on Bach's part, and one which contributed to the widespread adoption of his book on campuses across America.[37]

Piggybacking on the work of Chomsky and his MIT colleagues, Bach's textbook paints transformational grammar as a necessary replacement for immediate constituency grammar. Calling ICG "only a more sophisticated version of [...] the schoolroom drill [...] of assigning grammatical labels to parts of the sentence", "essentially inadequate to describe a language like English", and "enormously complicated", Bach argued that constituency theory inherently lacks the explanatory power necessary for the analysis of natural language.[38] He concluded, and guided his readers to conclude, that "the description of natural languages is considerably simplified by the addition of a transformational component [and] that various important relations between sentences and types of constructions can be adequately explained only by transformational rules".[39]

While this rhetoric was commonplace in transformational literature in early 1960s, Bach's book adds one important element: practice problems intended to acquaint students with the application of transformational theory to concrete language situations. Covering a wide variety of languages from Norwegian to Latin to Japanese, these problems were designed to showcase the transformation-as-tool. Montgomery praised Bach's practice-oriented approach, calling his problem sets "ingenious" and remarking that he uses "problems which are continuations of others presented earlier, requiring incorporation of additional data and reworking of the rules to accommodate such data. By this device, the student is led to appreciate the effects of a failure to consider the total picture; at the same time, he learns techniques for revising and expanding formulations as new data are gathered, and devel-

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[37]Christine A. Montgomery, "Review of Emmon Bach's An Introduction to Transformational Grammars," *Language* 41/4 (1965), p 632.

[38]Bach, *An Introduction to Transformational Grammars*, p 33, 65, 172–173.

[39]*Idem*, p 172.

ops some ability for predicting what revisions may be necessary".[40] Bach's book initiated a new norm, and syntax textbooks published in its wake maintained an exercise-based-learning approach.

Bach's textbook also stood out for its treatment of the formal and mathematical underpinnings of transformational grammar. The formal basis of Chomsky's work – communicated primarily in the technical journals *Information and Control* and *The IRE Transactions on Information Theory* – was far beyond the training of most linguists of the 1960s. It included a demonstration of the inability of finite-state processes to capture English syntactic structure, the elaboration of the Chomsky hierarchy of formal languages, and the formalization of phrase structure grammars. Dealing with this work required particular care from textbook authors: in this medium, intended for beginning students, formal and mathematical arguments could be neither completely ignored nor fully incorporated. Bach tread a middle line, presenting the transformation-as-tool independent of the theory's underpinnings, while also emphasizing the importance of mathematics to modern linguistic science. This approach ensured that students with no mathematics background would, by the end of the book, be able to apply transformations to real-world linguistic data. Bach's treatment of this tricky subject was richly rewarded by commentators: in his 1967 review of *Aspects of the Theory of Syntax*, Heles Contreras noted that the technical content of *Aspects* was "not intended for beginners" and would be "difficult to fully appreciate" without first having read Bach's textbook; later, in 1980, Frederick Newmeyer wrote that Bach's book made transformational grammar "accessible and interpretable to beginning students".[41]

Despite this deemphasis of mathematics in his presentation of transformational techniques themselves, Bach used a later chapter of his book to provide a basic introduction to the mathematical concepts of modern linguistics – namely, set theory and logic. Here, he also offered a simplified rendition of Chomsky's mathematical arguments for a tripartite grammar. The application of mathematical methods to linguistics, Bach argued, would be "perhaps the most lasting results of the linguistic research of the last decade" – and hence

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[40] Montgomery, *Review of Bach*, p 635–636.
[41] Contreras, *op. cit.*, p 111, Newmeyer, *Linguistic Theory in America*, p 51.

no student could "afford to ignore" the relevant mathematical concepts.[42] His careful sep-

aration of the transformation-as-tool from mathematical linguistics, however, meant that

students could learn to use the former while passing over the latter. It also meant that

Bach's textbook was ideally set up to teach practical use of transformational grammar –

and, indeed, this is how the book was primarily used in university classrooms.

Following Bach's textbook, a plethora of syntax texts was released in the mid-to-late

1960s – the majority of which promoted transformational grammar as the correct approach

to sentence structure. Andreas Koutsoudas' *Writing Transformational Grammars: An intro-*

duction (McGraw-Hill Book Company, 1966), Francis Dinneen's *An Introduction to General*

Linguistics (Holt, Rinehart and Winston, 1967), Terence Langendoen's *The Study of Syntax:*

The generative-transformational approach to the structure of American English (Holt, Rinehart

and Winston, 1969), and others were backed by mainstream American academic publish-

ers and enjoyed wide distribution across the nation. Through these texts, transformational

grammar captured the pedagogical market and ensured that students across America were

trained in the transformational paradigm. Because of the immaturity of linguistics as an

academic discipline prior to the 1960s, these new textbooks did not need to compete with

older ones; rather, they formed the core of the first syntax programs at many universities.[43]

These syntax textbooks were interested not only in promoting transformational gram-

mar, but also in demonstrating the inadequacy of rival syntactic theories. They routinely

portrayed constituency grammars as incapable of properly handling natural language, re-

peating the arguments popularized by Chomsky and Lees. In his text, Georgetown Uni-

versity's Francis Dinneen calls constituency grammars "cumbersome" and "clums[y]", and

asserts that transformational grammars provide means for "correcting weaknesses in their

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[42]Bach, *An Introduction to Transformational Grammars*, p 143–144.

[43]In a decade when syntax-oriented textbooks – and, specifically, transformational grammar textbooks –
were *de rigueur*, Ronald Langacker's *Language and Its Structure: Some fundamental linguistic concepts* (Harcourt,
Brace and World, 1967) stands out as a comprehensive language study text. With sections on phonology, lan-
guage universals, sociolinguistics, and language history in addition to syntax, Langacker presented a broad
overview of linguistics to which, he argued, "every well-educated person should be exposed". Ronald W. Lan-
gacker, *Language and Its Structure: Some fundamental linguistic concepts* (New York: Harcourt, Brace and World,
Inc., 1967), p v.

[ICG's] presuppositions and methods".[44] Far from presenting the two theories as alterna-

tives, the syntax textbooks of the 1960s subordinated constituency theory to transforma-

tional theory, teaching students that Chomsky's linguistics program alone provided a way

forwards.

While the 1960s saw an explosion in linguistics textbook publishing, the reverberations

of this explosion were not widespread compared to other disciplines. In the same decade,

David Kaiser describes in his recent work on theoretical physics, American physics pub-

lishers "had begun to publish everything from summer school lectures, to conference pro-

ceedings, to informal lecture notes and reprint series" – and, as a result, such items no

longer "circulate[d] in haphazard, mimeographed form", but rather became part of the

mainstream literature market.[45] In contrast, the informal circulation of research notes

and papers, dittoed and mimeographed and distributed through mailboxes and depart-

mental lounges, remained common in linguistics into the 1970s. As such, published lin-

guistics textbooks constituted a key mechanism of knowledge dispersion: in an era when

access to underground transformational literature required a personal connection to MIT,

mainstream-published textbooks brought transformational theory to the masses and en-

abled free access to the theory regardless of connections and institutions. The underground

linguistics culture of the 1960s is discussed in detail in Chapter 5.

The transformational textbooks of the 1960s were noted for their wealth of problems

and exercises based on languages from all over the world – a characteristic which would

be instrumental to training young linguists to use and manipulate the transformation-as-

tool. Andreas Koutsoudas' *Writing Transformational Grammars* aimed explicitly to "bridge

the gap between the theory of generative grammar and its practice" by teaching the reader

"how to *write* transformational grammars".[46] With problem sets from 47 languages in-

cluding, in addition to Indo-European languages, Colloquial Lebanese, Lalana Chinantee,

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[44]Francis P. Dinneen, *An Introduction to General Linguistics* (New York: Holt, Rinehart and Winston, 1967), p 355, 365.

[45]Kaiser, *op. cit.*, p 253.

[46]Ronald W. Langacker, "Review of Andreas Koutsoudas' Writing Transformational Grammars: An intro-duction," *Language* 44/1 (1968), p 98, Andreas Koutsoudas, *Writing Transformational Grammars: An introduction* (New York: McGraw-Hill Book Company, 1966), p vii, my emphasis.

Fanti, Hungarian, Indonesian, Kaiwa-Guarani, Karok, Korean, Maori, Buriat Mongolian, Swahili, Tagalog, Tairora, Thai, Turkish, and Zapotec, Koutsoudas' text is a hands-on training manual intended to teach uninitiated students to capture sentence patterns with transformational rules in a mechanical step-by-step fashion. A typical problem set from this text presents a small dataset of grammatical and ungrammatical sentences representing a particular syntactic construction in the target language, along with English glosses, and asks the student to develop transformational rules to account for the data. The example below, from Koutsoudas' exercises on or-constructions, requires the student to uncover restrictions on or-compound sentences in Mandarin Chinese.[47] Target languages were frequently chosen to be maximally unfamiliar to students, emphasizing the idea of transformations as manipulable tools. Data sets from several languages were given for each construction, forcing the student to work repeatedly with the same basic structures: for example, Koutsoudas presents conjunction data from Mandarin Chinese, Thai, Korean, French, Modern Greek, and English.

Or-constructions in Mandarin Chinese (Yale Romanization):

1. ta chyù   *He is going.*

2. ta lái   *He is coming.*

3. nǐ chyù   *You are going.*

4. ta chyù nǐ chyù   *Is he going or are you going?*

5. ta chyù ta lái   *Is he going or is he coming?*

6. ta chyù shr nǐ chyù   *Is he going or are you going?*

7. ta chyù háishr nǐ chyù   *Is he going or are you going?*

8. shr ta chyù shr nǐ chyù   *Is he going or are you going?*

9. háishr ta chyù háishr nǐ chyù   *Is he going or are you going?*

10. háishr ta chyù háishr ta lái   *Is he going or is he coming?*

   But not:

   * háishr ta chyù nǐ chyù

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[47] Koutsoudas, *op. cit.*, p 232.

* shr ta chyù nǐ chyù

* háishr ta chyù shr nǐ chyù

* ta chyù ta chyù

The problem sets in 1960s syntax textbooks were often deliberately falsified, or "controlled", to give idealized representations of linguistic phenomena.[48] "[T]he solutions for the problems [in the text] do not necessarily make valid structural claims about the languages from which the data are taken", wrote Koutsoudas, because "[t]he emphasis here is on writing reasonable rules, i.e. rules that are technically sound and that make reasonable claims about the data given, and not on whether a given solution makes reasonable claims regarding the structure of the language".[49] These textbook problem sets were paradigms – generally accepted example constructions with generally accepted solutions developed to help students practice using the transformation-as-tool, but not necessarily representative of real-world linguistic problems. This idealized hands-on practice was intended to build familiarity with transformational techniques and would become central to the emergence of an understanding of the transformation as a manipulable scientific tool, akin to molecular model sets or Feynman diagrams. Syntax rule-writing was a skill that did not come naturally to students. Through controlled problem sets, they learned to "master certain fundamental aspects of the mechanics of rule-writing, to employ a specific set of conventions consistently, and to consider simultaneously a complex set of factors in arriving at an explicit description of the limited corpus".[50] Problem sets, many syntacticians agreed, were necessary to train students to be comfortable with the newly-developed notational, diagrammatic, and rule-based techniques.

While learning through idealized examples became common in textbooks and courses, it was not universally approved of as a pedagogical method. "Students should be immersed in an actual grammar as soon and as thoroughly as possible", Kenneth Hale argued – and, he continued, exercises consisting of "small problems, based on parts of languages, fail to

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[48]*Idem*, p viii.

[49]*Idem*.

[50]Langacker, *Review of Koutsoudas*, p 107.

impress upon students the important point that the solution of a particular problem should be evaluated in the context of the total description".[51] This view was put into practice at the University of Arizona, where Bach's textbook was paired with a set of exercises from Papago (an Amerindian language spoken in the Sonoran Desert) in order to allow students to develop "a fairly substantial portion of the grammar of the language" at the same time as learning the basics of transformational grammar.[52] The pedagogical emphasis on rule writing was also criticized by Robert Hall. In his 1964 textbook *Introductory Linguistics* (Chilton Books), Hall argued that the rule-based nature of transformational grammar threatened to "degenerate into an arid, artificial game of inventing rules for constructing series of abstract formulae […] with little necessary relation to the facts of language as it is spoken".[53] The linguistics of the 1960s was, for Hall, dangerously close to devolving into a formalized, a-human subject.

These contrary voices aside, however, the consensus of the majority was that idealized problem sets from a wide variety of languages were particularly instructive since they forced students to apply rules and transformations mechanically to unknown language data, thus building up the ability to manipulate the technical portions of transformational grammar. In this sense, linguistics students across America were trained to use transformations as a fundamental tool for envisioning and analyzing language. Within a few years, transformations became the standard way in which students mediated between linguistic theory and the world of syntactic phenomena – the establishment of a linguistic worldview instigated in large part by pedagogy. This massive emphasis on transformations would, with the Peters–Ritchie proof of the early 1970s (cf. Chapter 3), provoke a backlash against TG on the basis that linguists were too eager to use transformations – an eagerness caused by a generation overly trained to read transformations out of linguistic data.

That transformational grammar was experiencing rapid growth was commonly recognized through the 1960s. Only a year after Bach's text was published, new work was already

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[51] Hale, *Review of Bach*, p 267.
[52] *Idem*, p 268.
[53] Robert A. Hall Jr, *Introductory Linguistics*, 1st edition (Philadelphia: Chilton Books, 1964), p 266–267.

considered to "have rendered much of his material on transformational theory obsolete".[54]

The pace of work meant that it was impossible for textbook authors to transmit a clean and complete version of transformational theory – and hence they concentrated on two main ideas: first, the transformation-as-tool and, second, the superiority of transformational grammar over rival theories. The first taught students to manipulate and apply transformations as a basic linguistic tool – skills which would remain relevant even as the details of transformational theory continued to change. In the second, the most common victim was constituency grammar, which was painted as an outdated and inadequate theory whose time had come to be replaced by transformational grammar. Far from taking a neutral position on theoretical questions, these texts actively promoted transformational theory as the only way of "overcoming many of the formal and empirical defects" of constituency grammars.[55] The publication of these textbooks in the middle of the theory-choice debates was vital to the eventual dominance of transformational grammar: by training students to use the transformation-as-tool, these texts ensured that transformational techniques became second nature to a generation of American linguists.

The market for linguistics textbooks in 1960s America was largely uniform, with one exception: at MIT, only authors with close links to that institution were looked upon favorably. While Bach's book was widely used at universities across the country, it was "officially ignored" at MIT – a result of its author having never been part of the MIT establishment.[56] Bach completed all of his degrees at the University of Chicago, studied as a Fulbright scholar, and subsequently took a teaching position at the University of Texas at Austin. Inside MIT, syntax courses were based primarily on unpublished lecture notes prepared by faculty. This changed with the 1971 publication of Marina Burt's book *From Deep to Surface Structure: An introduction to transformational syntax* (Harper and Row Publishers), designed to enhance the ability of students to understand, manipulate, and order large numbers of transformational rules. Based on an introductory transformational grammar course taught at MIT

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[54]Montgomery, *Review of Bach*, p 632, Hale, *Review of Bach*, Bach, *An Introduction to Transformational Grammars*.

[55]Bach, *An Introduction to Transformational Grammars*, p 168.

[56]Bach, *Interview*.

by Morris Halle and John Robert Ross between 1967 and 1970, Burt's book was uniquely oriented to the MIT environment. The rules at the core of the book – the passive, reflexive, imperative, and do-support transformations – were selected not because they were representative of the current state of research in TG, but because they were, Burt argued, "exemplary of the basic insights achieved by advocates of the transformational approach", and hence "essential for any student interested in learning some fundamentals of transformational grammar".[57] Students have particular difficulty "acquir[ing] the skills necessary to comprehend a reasonably complex argument in syntax", wrote Halle, Ross and Chomsky in the preface, and hence focused rule-based exercises such as those offered in Burt's book are essential for undergraduate training in linguistics.[58]

The antipathy with which MIT linguists regarded non-MIT-establishment textbooks was reciprocated with the publication of Burt's manuscript. Led by George Lakoff, faculty and students at the University of Michigan released *Where the Rules Fail: A student's guide – an unauthorized appendix to M.K. Burt's From Deep to Surface Structure* (published by the Indiana University Linguistics Club in 1972), a self-proclaimed "anti-establishment" missive which aimed to highlight "some of the failures of the rules in Burt's book".[59] While the explicit thrust of G. Lakoff's reply was to argue that students should be presented not with set and standard rules, but with "the field of linguistics as a living discipline, where not even one rule of one language is fully understood [and] where most analyses are hopelessly far away from their goals", the book is implicitly an early attack on transformational grammar by a group of linguists who would later lead the generative semantics movement (cf. Chapter 5).[60] The irreverent style of the book – for example, the suggestion to replace the "arbitrary" feature +REFLEXIVE by the feature +CHOCOLATE – is indicative of the biting, witty style which would come to be characteristic of opponents to Chomsky's program in the 1970s.[61]

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[57] Marina K. Burt, *From Deep to Surface Structure: An introduction to transformational syntax* (New York: Harper & Row Publishers, 1971), p U3.

[58] *Idem*, p U1.

[59] George Lakoff (ed.), *Where the Rules Fail: A student's guide – An unauthorized appendix to M.K. Burt's From Deep to Surface Structure* (Indiana University Linguistics Club, 1972), p U3.

[60] *Idem.*

[61] *Idem*, p ii.

The success of transformational grammar in capturing the pedagogical market in the 1960s was greatly aided by an abject lack of competition from one of its main rivals – stratificational grammar. While the two theories were developed at the same time, no stratificational textbooks appeared on the American linguistics scene until 1972, nearly a decade after the publication of the first transformational textbook. David Lockwood, author of the 1972 text, promoted stratificational theory as a viable and strong alternative to transformational grammar. He recognizes, however, the toll taken by the absence of stratificational textbooks in the 1960s: "relatively few linguists are at present equipped to teach a course in stratificational linguistics", he wrote in his preface, and "few students are able to learn it on their own".[62] Only at universities with strong faculty support for the theory, such as Lockwood's own Michigan State University, did students learn stratificational grammar. In 1973, stratificational theory received a second pedagogical boost with the publication of the anthology *Readings in Stratificational Linguistics* (edited by Lockwood and Adam Makkai), which was explicitly intended to "serve as a textbook for graduate or advanced undergraduate courses dealing with the comparison of modern linguistic theories and stratificational grammar specifically" – but it was already too late for the theory, which had been far out represented on the pedagogical scene for a decade.[63] The book which should have opened stratificational grammar to the pedagogical market was Sydney Lamb's *Outline of Stratificational Grammar*, published in 1966. Lamb's manuscript suffered, however, from a "very disappointing" lack of clarity and organization: rather than providing a comprehensive introduction to stratificational theory, one reviewer commented that "[t]he reader must look to earlier articles by Lamb for a fuller understanding of the book".[64] Lamb's *Outline* failed to serve as a practical pedagogical tool, and was not widely adopted as a textbook. By not capitalizing on the open syntax textbook market of the 1960s, stratificational grammarians failed to make their theory accessible to students across America.

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[62] Lockwood, *Stratificational Linguistics*, p v.
[63] Makkai and Lockwood (eds.), *Readings in Stratificational Linguistics*, p v.
[64] Palmer, *Review of Lamb*, p 287.

Early in the 1960s, the linguistics textbook market – and, specifically, the syntax textbook market – was wide open. Within a few years, several textbooks specializing in transformational theory were widely available on campuses across America. By capturing this market, transformational grammarians gained control over the training of a generation of young linguists. This generation was immersed in the transformational paradigm throughout their undergraduate and graduate studies, and consequently developed a transformational worldview: to them, transformations and syntactic analysis went hand-in-hand.

The effectiveness of textbooks in training young linguists in transformational theory was greatly enhanced by two features internal to that theory: canonical examples and visual appeal. Canonical examples gave students a foothold to enter what was otherwise a technically complex theory, and the notational techniques used in transformational grammar distinguished it from rival theories. Together, these features made transformational grammar a pedagogically effective theory – one which was easier to teach, learn, and use than rivals. These features are discussed in the following two sections.

4.3 Canonical Examples

Canonical examples – short, simple, and demonstrative applications of transformations – were ubiquitous in late 1950s and 1960s transformational literature. From the beginning, presentations of transformational theory focused on providing a foothold for students and other newcomers by illustrating the technical character and capacity of the theory. Repeatedly used in published works, underground literature, oral presentations, textbooks, and classroom lectures, canonical examples were essential to building an interface between transformational theory and linguists, and to providing soundbites of immediate insight into the theory. In this section, I argue that canonical examples were vital to the rise of transformational grammar on the pedagogical scene. First, canonical examples were particularly important in light of the nature of the primary TG sources: *Syntactic Structures* and *Aspects of the Theory of Syntax* were considered dense, difficult, and highly technical works. Second, because of their continuous repetition over a decade, canonical examples

became shared knowledge among linguists – common ground which could be assumed as a starting point for any debate or discussion.

The main canonical examples of the 1960s are the passive, negation, and interrogative transformations; more broadly, they also include the nominalization and imperative transformations. The transformational community referred to canonical examples as "classic" or "prototype" transformations, and as "perfect, clear, telling example[s]" which captured the core of the transformational paradigm.[65] The paradigmatic canonical example – the passive transformation – enjoyed prime real estate in textbooks and technical manuscripts. In textbooks, this transformation was presented in up to four ways: as full rewrite rules indicating pre- and post-transformational structural analysis (figure 4.3), as abbreviated rewrite rules (figure 4.4), as a derivational tree (figure 4.5), and in English prose (figure 4.6).[66] These four representations of the same transformational rule demonstrate various notational and diagrammatic techniques, showcase the ability of transformations to capture linguistic data, and provide a working example of the transformation-as-tool. As a pedagogical tool, canonical examples taught students to recognize alternate notations and to envision transformational rules in a variety of ways. Coupled with problem sets, they allowed students to practice writing transformational rules for structurally similar constructions, developing the ability to apply transformations by analogy.

The importance of canonical examples to the success of transformational theory is three-fold. First, they provided students and linguists with a foothold to enter what was otherwise a difficult theory. By highlighting the analysis of specific syntactic constructions, they enabled newcomers to grasp the syntactic devices underlying transformational grammar without needing to tackle the mathematical motivation for the theory. In their abstract form, transformational rules were considered "difficult to read and understand" – but once this abstraction was stripped away and the rules presented in simplified form, they became

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[65] Lakoff, *The Way We Were*, p 970, D. Terence Langendoen, *The Study of Syntax: The generative-transformational approach to the structure of American English* (New York: Holt, Rinehart and Winston, Inc., 1969), p 161, Bach, *An Introduction to Transformational Grammars*, p 62.

[66] Figures 4.3 through 4.6 are adapted from Bach, *An Introduction to Transformational Grammars*.

Pre-Transformational Structural Analysis

$$NP - Aux - V - NP$$

Structural Change and Post-Transformational Structural Analysis

$$X_1 - X_2 - X_3 - X_4 \rightarrow X_4 - X_2 + be + en - X_3 - by + X_1$$

Figure 4.3: Full rewrite rules for T_{PASS}

$$NP - Aux - V - NP' \rightarrow NP' - Aux + be + en - V - by + NP$$

Figure 4.4: Abbreviated rewrite rules for T_{PASS}
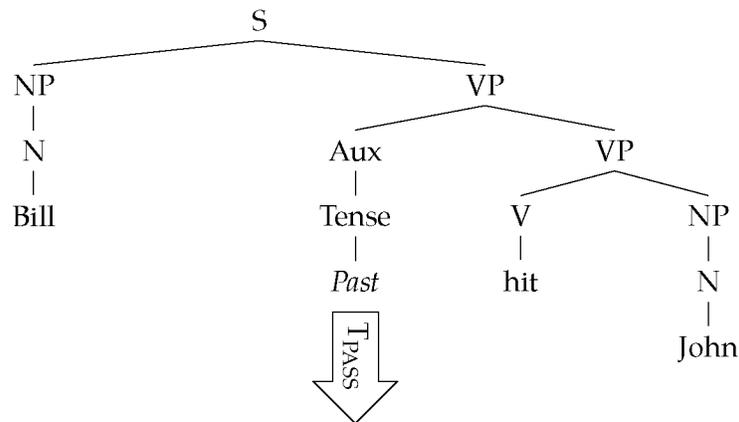
John + past + be + en + hit + by + Bill

S Sentence
Aux Auxiliary
N Noun
NP Noun Phrase
V Verb
VP Verb Phrase

Figure 4.5: Derivational tree for T_{PASS}

A passive may be formed from any string that may be analyzed without remainder into four parts: (1) a noun phrase followed by (2) the auxiliary (i.e., the complex of elements leading to past tense, person marker, modals, and so on) followed by (3) a verb (actually a verb of a certain class must be specified) followed by (4) a second noun phrase. The passive counterpart to each such sentence is formed by switching noun phrases (X_1 – the first segment of the analyzed string – and X_4), by attaching $be + en$ (the past participle formant) to the auxiliary, and by placing by before the last noun phrase.

Figure 4.6: Prose description of T_{PASS}

much easier to understand, use, and manipulate.[67] A student who understood the transformational treatment of active-passive sentence pairs, for example, could apply the theory to similar syntactic constructions by analogy. In this way, the transformation emerged as a tool which could be used to manipulate linguistic material, but which was in effect autonomous from the technical basis of the theory.

The clear separation between the transformation-as-tool (that is, an operation which rearranges the constituency structure of a hierarchical string of morphemes) and the mathematical basis of transformational theory (that is, Chomsky's work on formal language hierarchies, finite-state processes, and the formalization of rewrite rules) provided by canonical examples became a defining feature of 1960s transformational grammar. Through this decade, the typical working linguist accepted and used transformations as technical tools while all but ignoring Chomsky's formal and mathematical work. Lacking training in logic, mathematics, and computer science, linguists were not easily able to read this work – and, as a result, it was "scarcely known" within the linguistics community.[68] The consequences of this selective ignorance were positive for transformational grammar in the 1960s: the isolation of the transformation as a syntactic tool opened TG to all linguists, regardless of their background. Transformational theory gained a large number of adherents whose buy-in would not have been possible without the use of canonical examples. By the early 1970s, however, the short-shifting of the previous decade began to impact negatively on transformational theory. Transformations, Stanley Peters and Robert Ritchie proved, were too

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[67] Hayes, op. cit., p 95.
[68] Gleason Jr, Theories in Conflict, p 88.

powerful: they could capture not only all human languages, but also a large number of superfluous artificial languages (cf. Chapter 3). The unlimited power of transformations was only discovered when the mathematical basis of the theory was investigated seriously.

While linguists by and large ignored the details of Chomsky's mathematical work in the 1960s, they did not ignore its existence: they saw this work as confirming the scienticity and prestige of transformational grammar, and held it up as a key reason for choosing TG over rival syntactic theories. "There was great faith in the early TG paradigm that a formal mathematical model would solve many, even unforeseen, problems just by setting in motion and letting it run", wrote the University of Chicago's J. Peter Maher – a faith sustained by the push towards scientization which consumed linguistics in the era.[69] Transformational grammarians subscribed to "the general modern fashion of seeing 'scientific' as an inherent in mathematization" and, without necessarily having read the mathematical works from which TG emerged, they pointed to this foundation as legitimatizing the scientific claims of the theory.[70] Mathematics, Bach recalls, was used as "propaganda" to distinguish transformational grammar from rival syntactic theories.[71]

When presented in the classroom, mathematical approaches to linguistics were usually watered down. Bach's 1964 textbook, for example, included a "concise summary" of mathematical set theory and a "simplified presentation" of Chomsky's mathematical arguments, designed to give students a digestible version of this work.[72] Similarly, William Cooper's manuscript *Set Theory and Syntactic Description* presented a "brief but digestible" introduction of mathematical concepts relevant to linguistics.[73] Importantly, these presentations of mathematical linguistics were kept separate from syntax *per se*: in syntax courses, the transformation-as-tool was taught through canonical examples, while the mathematical underpinnings of the theory – if taught at all – were an add-on, not essential to syntactic analysis itself.

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[69]Maher, *op. cit.*, p 15.

[70]*Idem.*

[71]Bach, *Interview.*

[72]Montgomery, *Review of Bach*, p 636.

[73]Victoria A. Fromkin, "Review of William S. Cooper's Set Theory and Syntactic Description," *International Journal of American Linguistics* 32/2 (1966), p 195–196.

Second, canonical examples were instrumental in convincing students and established linguists of transformational grammar's claim to superiority over immediate constituency grammar. These examples demonstrated one of the most effective arguments for TG: that transformational theory could capture syntactic patterns which could, in principle, not be captured by constituency theory. Canonical examples showcased the ability of the transformation-as-tool to account for syntactic phenomena in a manner which, as Francis Dinneen wrote in his 1967 textbook, "corresponds generally to the simplest classification and to our intuitive classification as well".[74] By highlighting active-passive, positive-negative, and declarative-interrogative sentence pairs, canonical examples brought to the forefront the "deeper connexions" between sentences which were so vital to convincing linguists of the need for transformational theory.[75] After his presentation of the canonical active-passive transformation, for example, Bach explicitly draws a comparison with constituency theory, asserting that the rival theory can only handle the active-passive construction in "enormously complicated" ways.[76] Canonical examples – short, compact, and visually striking – summarized the argument for the inadequacy of constituency grammars quickly and with a great deal of demonstrative power.

Third, canonical examples reduced the learning-curve for transformational theory by clearly illustrating Chomsky's rejection-by-partial-incorporation of phrase structure rules. The body of a canonical example can be divided into two parts: a phrase structure (or constituency) part, and a transformational part. The first part was already familiar to linguists who had worked with constituency theory through the 1950s, and hence the novelty was restricted to the second part. In this way, canonical examples made transformational grammar easier for linguists to follow than stratificational grammar, which lacked any tie-in to previous American linguistics work. Because of its novelty value, the learning-curve for stratificational theory was particularly steep – a fate transformationalists avoided by their partial incorporation of phrase structure rules. From the perspective of transforma-

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[74]Dinneen, *op. cit.*, p 377.
[75]Lyons, *Theoretical Linguistics*, p 249.
[76]Bach, *An Introduction to Transformational Grammars*, p 65.

tional grammar, this advantage is ironic: while Chomsky and his colleagues worked hard to destabilize constituency grammar, the incorporation of constituency-type rules in the transformational framework gave TG the advantage of familiarity.

On the pedagogical scene, canonical examples also provided a fertile starting point for the development of problem sets. By expanding from a given English canonical example to a related syntactic construction in English, or to an equivalent construction in another language, textbook authors were easily able to develop multitudes of exercises to train students to write transformational rules. After presenting the canonical past tense and negation transformations for English, for example, Bach's text asks students to develop the corresponding past tense and negation rules for Japanese, based on an idealized data set.[77] As productive pedagogical tools, canonical examples guided the training of a generation of American linguists.

Stratificational grammar – a key rival to transformational theory through the 1960s – did not make effective use of canonical examples. Because stratificational theory was so alien to the American linguistic norm, and because Lamb's notation system was particularly daunting, stratificationalists were not able to present individual syntactic analyses in concise soundbites. While an introductory presentation of the canonical active-passive transformation could take as little as half a page, the presentation of the stratificational treatment of active-passive pairs consistently required several pages. Further, while transformational grammarians could present basic syntactic analyses diagrammatically, stratificationalists commonly required prose explanation in addition to network diagrams. Finally, the multiple strata at the core of Lamb's theory, together with the diffusion of syntax over all strata, prevented any simple illustration of stratificational syntactic analysis. As the number of strata increased through the 1960s, syntactic analysis only became more complex. Ultimately, stratificational grammarians were unable to divorce syntactic analysis from the technical basis of their theory: any discussion of syntactic constructions, however elementary, needed to be preceded by an explication of relationship structure, strata, tac-

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[77] *Idem*, p 86–87.

tics, and network diagrams. As such, stratificationalists were not able to match the gains made by transformationalists by separating syntactic analysis from formal theoretical underpinnings.

Tellingly, stratificational grammar trailed transformational theory in all areas related to canonical examples. The amenability of transformational grammar to canonical examples allowed that theory to provide a foothold for newcomers; separate syntactic analysis from technical background; develop pedagogical tools and problem sets; and train students efficiently and productively. Stratificational grammar, in contrast, had no simple mechanism for presenting basic syntactic analyses – and, consequently, newcomers found the theory forbidding; sentence structure could not be taught independently of technical complexities; pedagogical tools were slow to be developed; and student training was limited and slow.

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The efficiency and productivity with which textbook authors used canonical examples in the 1960s gave transformational grammar a pedagogical edge over rival syntactic theories. Critically, they allowed the transformation-as-tool to be understood and used autonomously from the formal underpinnings of the theory. Transformational theory can be likened to "an iceberg, nine-tenths of which is under water, inaccessible to view", Michael Halliday said at the 1962 International Congress of Linguists, held in Cambridge (Massachusetts)[78] – an image which clearly illustrates the role of canonical examples: the one-tenth of the iceberg above water – the visually striking and easily understood aspects of transformational grammar – are the canonical examples, while the submerged bulk of the iceberg – difficult to grasp and not necessary for syntactic analysis *per se* – represents the formal and mathematical underpinnings of the theory. With this separation, canonical examples greatly simplified the teaching and learning of a complex theory, and were instrumental to the dominance transformational grammar achieved on the American peda-

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[78]Horace G. Lunt, "Discussion of Noam Chomsky's The Logical Basis of Linguistic Theory," in Horace G. Lunt (ed.), *Proceedings of the Ninth International Congress of Linguists (Cambridge, MA, August 27–31 1962)* (The Hague: Mouton & Co., 1964), p 988.

gogical scene in the 1960s.

4.4 Notation

> So geographers in Afric' maps
>
> With savage pictures fill their gaps
>
> And o'er unhabitable downs
>
> Place elephants for want of towns
>
> Jonathan Swift's indictment of 17th-century cartographers[79]

As has recently been emphasized in the history of science literature, the visual attributes of a theory can be as important to the success or failure of that theory as intellectual or scientific merit. In any syntactic theory, sentence diagramming – that is, the visual presentation of linguistic structure – acts as the primary interface between data and theory for the working linguist. The notational techniques used to describe and analyze syntactic constructions contain inherent assumptions about the structure and functionality of those constructions, and can restrict or otherwise interfere with the linguist's ability to manipulate data. Indeed, visual appeal played a central role in the theory-choice debates of the 1960s. In this section, I argue that the visual presentation of transformational grammar, and the notational innovations associated with its founder, set that theory apart from its main rivals and made it particularly amenable to pedagogical purposes.

At its most basic, syntactic data consists of sets of strings of sounds from the sound-inventory of a given language, L, divisible into two (usually nonoverlapping) subsets: those strings of sounds which are wellformed to a native speaker of L, and those which are not wellformed. Each construction is typically presented in the alphabet of language L, along with an English translation and a lexemic and grammemic gloss, as shown in the sample dataset below. Any additional notational or diagrammatic features, such as the various techniques shown for constituency, transformational, and stratificational grammar in

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[79]Zoe Marsh and G.W. Kingsnorth, *An Introduction to the History of East Africa* (Cambridge: Cambridge University Press, 1957), p 56.

Chapter 2, constitute an application of a theoretical framework to the data. As these syntactic theories were developed, notation styles became fundamentally linked to theoretical convictions, and grew into lenses through which all data was filtered. Sentence diagrams, H.A. Gleason recalls, became "inextricably entangled with our ideas of the facts [they were] supposed to convey – both their selection and their interpretation".[80] Notational choices established conventions, and conventions came to embody explanation. As notational techniques became secondhand to linguists, Gleason continues, "some of our thinking [was] transferred to them".[81] For the three main syntactic theories of the 1960s, notation, diagrammatic techniques, and visual presentation came to define both their public face and the technical worldview of their practitioners.

Sample syntactic dataset: Perfect forms of Hungarian verbs with verbal prefixes

1.

Péter   megeszi   az   almát.

Peter   has-finished-eating   the   apple-ACC

Peter has eaten up the apple.

2.

Péter   megette   az   almát.

Peter   finished-eating   the   apple-ACC

Peter ate up the apple.

3.

Péter   meg fogja enni   az   almát.

Peter   will-eat-completely   the   apple-ACC

Peter has eaten up the apple.

4. * Péter megenni fogja az almát.

ACC Accusative form

* Not wellformed to a native speaker

In the 1940s and 1950s, when immediate constituency grammar reigned, notation did not play an important role in syntax. Constituency grammarians used a variety of diagrammatic techniques, basing their choices on personal preference, ease of typesetting,

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[80]Gleason Jr, *Theories in Conflict*, p 153.
[81]*Idem.*

and pedagogical utility (cf. Chapter 2). There was little debate over variations in notation, and representation was thought to be far removed from theory. In early transformational linguistics, too, notational systems varied greatly between authors – and in stratificational grammar, diagrammatic techniques remained non-standardized for nearly a decade. "In the early days of generative grammar", wrote Cambridge linguist P.H. Matthews in 1982, "it was stressed that notation did not, in principle, matter".[82] Indeed, in his 1957 review of *Syntactic Structures*, Robert Lees asserts that "a valid grammatical statement is just as valid whether it is affirmed in an abstruse algebraic notation or in plain words".[83] But in the early 1960s, this hitherto "apparently trivial" aspect of linguistic theory grew into a central element of formalization and theoretical commitment.[84]

As work on machine translation and acoustic experimentation increased and drew attention to numerical and instrumental procedures, there grew "a feeling in linguistics that informality will not do".[85] In response, linguists began to tailor their syntactic notation for use with computer applications. The transition from a *laissez-faire* approach to notation to the belief that notational techniques were integral parts of linguistic theories was complete by the early to mid-1960s. "[O]rdinary language is often so vague", Emmon Bach wrote in 1964, and therefore is not suitable for a discipline which aims to make precise and accurate claims about syntactic phenomena.[86] As formalization and mathematization swept into linguistics, syntactic notation itself came under close scrutiny.

Two key early notational developments were introduced by Chomsky: the formalization of rewrite rules and the popularization of derivational trees. The advantages of formal rewrite rules over boxed constituency diagrams – a notational technique popular among constituency grammarians in the 1950s (cf. Chapter 2, figure 2.16) – were immediately exclaimed upon: boxed constituency diagrams, Fred Householder wrote in *Language* in 1959,

[82] P.H. Matthews, "Formalization," in David Crystal (ed.), *Linguistic Controversies: Essays in linguistic theory and practice in honour of F.R. Palmer* (London: Edward Arnold, 1982), p 4.

[83] Lees, *Review of Chomsky*, p 331.

[84] Yuen Ren Chao, "Graphic and Phonetic Aspects of Linguistic and Mathematical Symbols," in Roman Jakobson (ed.), *Structure of Language and Its Mathematical Aspects: Proceedings of Symposia in Applied Mathematics (held in New York City, 14–15 April 1960)* (Providence: American Mathematical Society, 1961), p 76.

[85] Matthews, *Formalization*, p 8.

[86] Bach, *An Introduction to Transformational Grammars*, p 146.

are "all very well for analyzing sample sentences and clarifying relationships, but are very clumsy for comparison and generation".[87] "The same relationships can be indicated in a form more suitable for easy manipulation by a series of generative (or 'rewrite') formulas", he continued, and, in comparison to the older technique, rewrite rules are "(a) more formalized, (b) more general (and can be made much more so), and (c) easier to manipulate".[88] Constituency grammarians quickly abandoned boxed constituency diagrams and other older notational techniques, adopting in their place formal rewrite rules. On the surface, the results were only positive: rewrite rules provided constituency grammarians with a formal, highly regarded notational device – one which, unlike their older techniques, would need "to be expanded only a little to be capable of generating a large percentage of the sentences of English".[89] They allowed constituency grammarians to present more data in less space, to easily abstract from specific sentences to generalized constructions, and to develop standardized conventions about the relationships between data, notation, and linguistic phenomena. Still, constituency theory benefited relatively little from this notational development. The rewrite rule notation was firmly associated with its founder – Chomsky – and consequently the accolades for the technique reflected on his linguistic theory – transformational grammar. The atmosphere surrounding the introduction of rewrite rules was one which had at its center Chomsky's case for the inadequacy of constituency grammar as a syntactic theory. While rewrite rules were adopted by constituency grammarians, this did little to attract attention to the theory. The rules themselves were positively associated with transformational theory. In the early 1960s, prestige was accorded to the *development* of clear, simple diagrammatic techniques – and not merely to the use of such techniques.

Chomsky's early work also popularized derivational trees as a method of graphically representing syntactic structures (cf. Chapter 2, figure 2.2). With their visual simplicity and their roots in mathematical graph theory, trees were soon considered "a more efficient means of representation than the older diagrams".[90] Soon, Gleason recalls, trees

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[87] Householder, *Review of Hockett's Modern Linguistics*, p 506.
[88] *Idem.*
[89] *Idem.*
[90] Garvin, *Review of Jakobson I*, p 167.

S
NP VP
NP and NP refused S
NP S NP S to look at me
Det N who thought S Det N
the woman
The man that I wanted S who was with him
to receive a tip
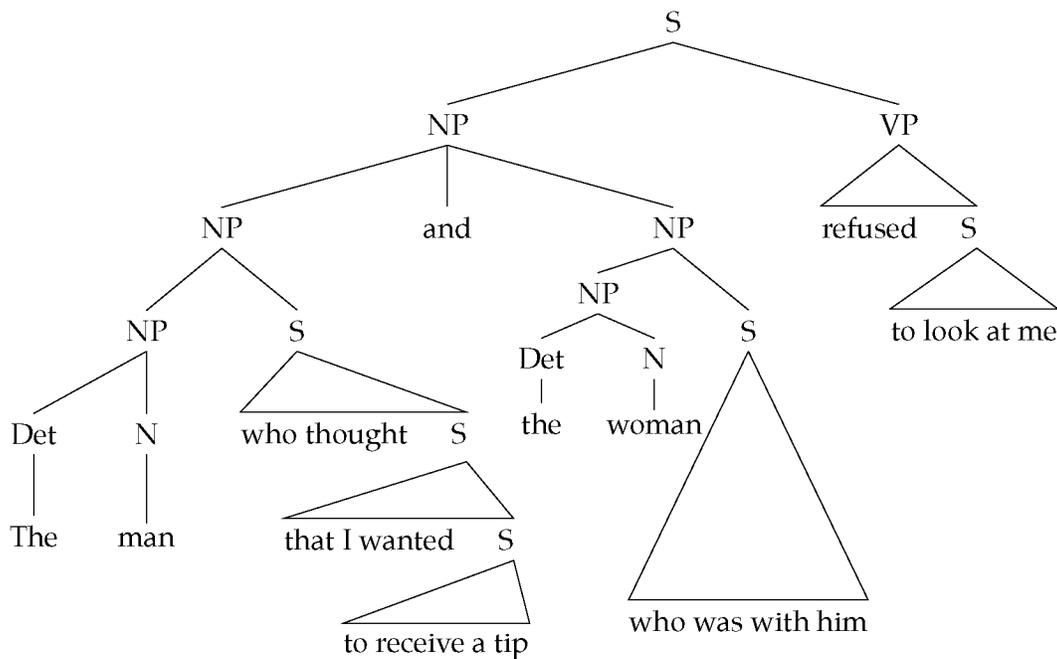
S Sentence
Det Determiner
NP Noun Phrase
N Noun
VP Verb Phrase
V Verb

Figure 4.7: "Open Triangle" tree structure (adapted from Langacker 1967)

were "sprouting up everywhere".[91] "It took quite a while for anyone to notice what was happening, and some never did", he continues: rather than being incidental to the theory, "more and more they [trees] became the subject of discussion. They had moved into centerstage".[92] Trees functioned not only as notational devices; they also reconfigured vocabulary and established new ways of looking at, and thinking about, syntactic structure. In the linguistics community, the use of terms such as *branching*, *node*, and *root* to describe syntactic structure – vocabulary pulled directly from the tree metaphor – became commonplace. Trees, along with their natural diagrammatic extensions, began to act as shortcuts, allowing linguists to represent complex structure with just a few lines. Open triangles fre-

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[91]Gleason Jr, *Theories in Conflict*, p 156.
[92]*Idem.*
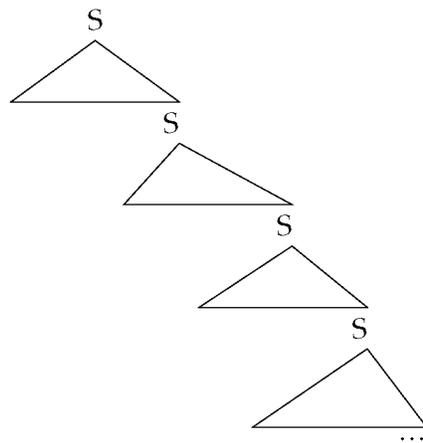
S Sentence

Figure 4.8: Cascading triangles (adapted from Langacker 1967)

quently replaced fully detailed tree structures, as shown in figure 4.7. More dramatically, tree structures were manipulated in order to explore syntactic structure itself. Linguists began to conceive of syntactic phenomena in terms of associated tree diagrams – not as linguistic constructions ultimately based in acoustic or introspective language data, but as manipulable shapes on a page. The University of California San Diego's Ronald Langacker, for example, uses the diagram shown in figure 4.8 in his 1967 discussion of embedding to depict (potentially infinite) syntactic embedding. Here, all that remains of the tree is the topmost node (S) and an open triangle – all linguistic detail is omitted, and the construction is fully represented by an image which itself suggests the embedding pattern.

Further, an entire arithmetic was built around tree structures, allowing for the syntactic processes of concatenation, deletion, and replacement to be represented by tree-addition, tree-subtraction, and tree-substitution (figures 4.9, 4.10, 4.11). These diagrams, which are adapted from Andreas Koutsoudas' 1966 textbook *Writing Transformational Grammars: An introduction*, visually represented syntactic manipulations by linking them with familiar logical operations.[93] Figure 4.12, for example, shows Koutsoudas' presentation of the Thai conjunction, which explicitly depicts a transformational rule with tree operations.

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[93]Figures 4.9 to 4.11 adapted from Koutsoudas, *op. cit.*.
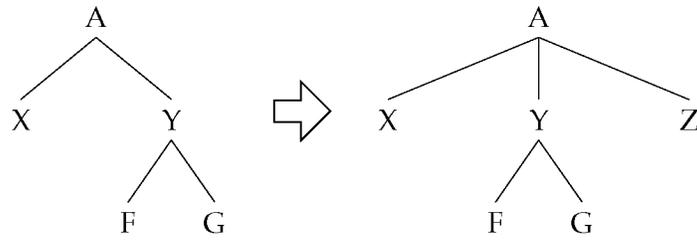
Figure 4.9: Concatenation (Tree-addition)
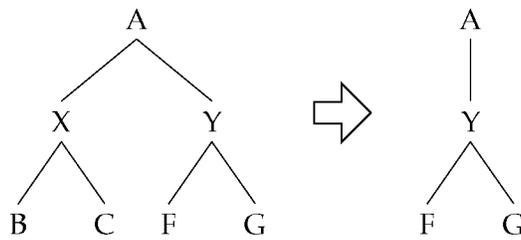
Figure 4.10: Deletion (Tree-subtraction)
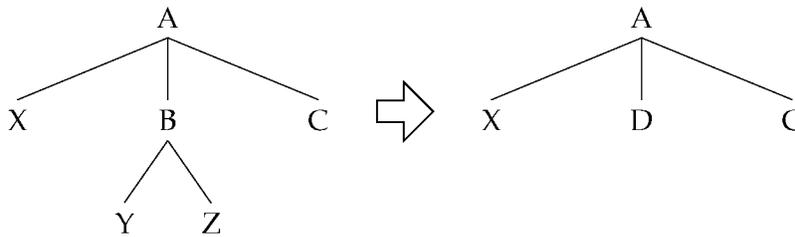
Figure 4.11: Replacement (Tree-substitution)
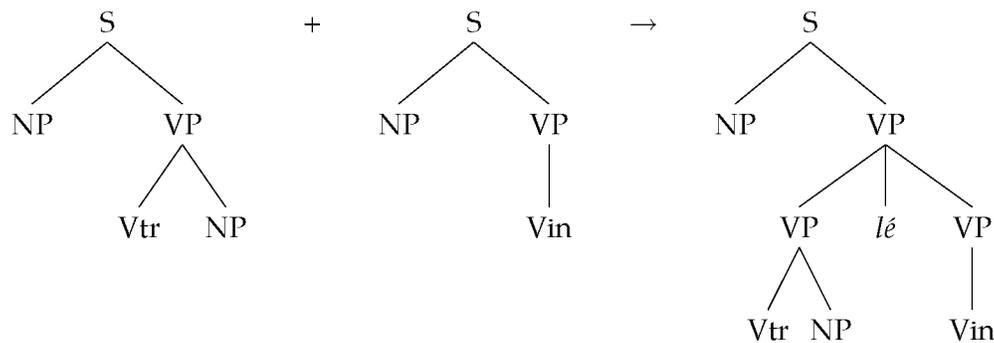
S Sentence
NP Noun Phrase
VP Verb Phrase
Vin intransitive verb
Vtr transitive verb
lé Thai conjunctive element

Figure 4.12: The Thai conjunction: tree addition (adapted from Koutsoudas 1966)

Finally, trees filtered into discussions of all aspects of linguistic theory, from morphology to syntax to semantics. In their 1964 *An Integrated Theory of Linguistic Descriptions*, Jerrold Katz and Paul Postal used trees to display the various semantic interpretations of morphemes. As shown in the semantic tree given in figure 4.13, any interpretation of the word *bachelor* can be obtained by tracing a continuous line from the top node to a terminal point. Chomsky and others also used trees for representing the distribution of syntactic features ([+ human], [+ abstract], etc.) (figure 4.14), and the order of application of transformational rules (figure 4.15).

Most linguists didn't recognize the impact this new diagrammatic technique was having on their work. Soon, characteristics of trees as graphic objects were imposed on linguistic data and began to influence syntactic analysis. Students were taught to manipulate trees formally even before being presented with syntactic data. Research questions were posed with the implicit assumption that tree structures would play a fundamental role in answering those questions. Extensions to theory were made by analogy with tree models: as noted by Ann Stewart, transformational grammarians developed their theory in part "by

```
                          bachelor
                             |
                           Noun

              Human                      Animal

         Male                               Male
                  Having a 4-year
     Adult    Young  academic college           Young
                      degree
      |        |                                     Seal
    never    Knight                                    |
   married     |                                 when without a
          serving under                          mate at breeding
          another's standard                          time
```
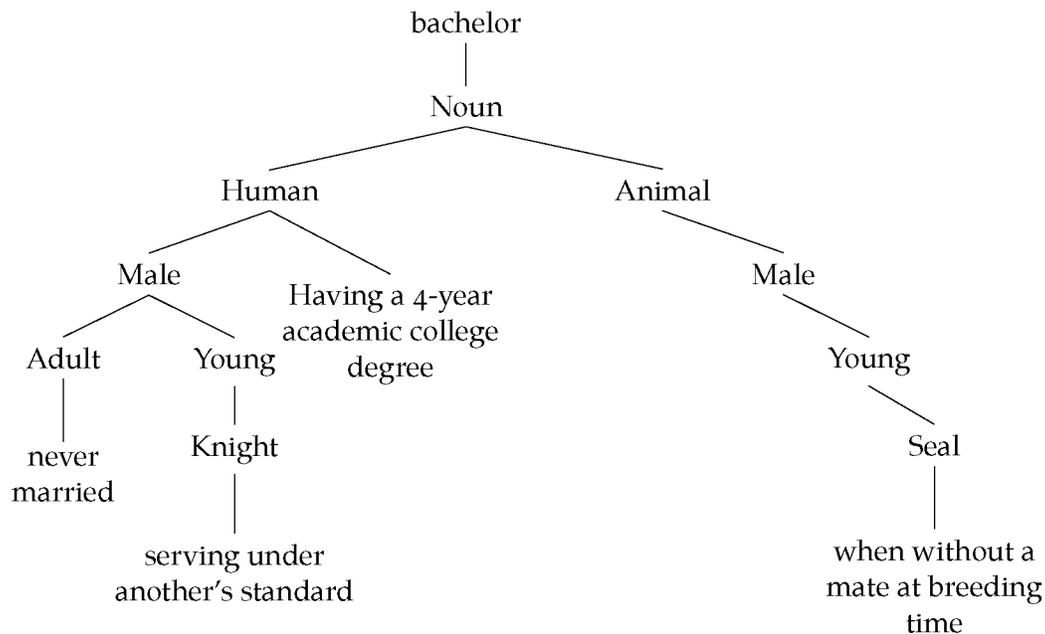
Figure 4.13: Tree representation of semantic interpretation (adapted from Katz & Postal 1964)

taking tree-related metaphors at face value (e.g. pruning, grafting, raising)".[94] Pruning, for example, is introduced in John Robert Ross' MIT doctoral dissertation, *Constraints on Variables in Syntax* (1967). It is a technique used to 'clean up' trees after the application of transformational rules. In cases when a transformation moves or deletes one or more constituents dominated by a node N, that node can be left "simply hang[ing] vacuously from the tree", causing problems for the cyclic application of transformations and the interpretation of surface structure.[95] In such cases, the node N can be pruned (that is, removed) to prevent complications. This selective removal of nodes, and the consequences for semantic interpretation and diagrammatic consistency, was widely debated in the late 1960s. As this type of notationally-motivated work expanded, tree structure and syntactic structure became nearly synonymous.

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[94]Raimo Anttila, "Review of Ann Harleman Stewart's Graphic Representation of Models in Linguistic Theory," *Language* 54/2 (1978), p 403.

[95]Jane J. Robinson, "Dependency Structures and Transformational Rules," *Language* 46/2 (1970), p 281.

COMMON

COUNT

ANIMATE

ANIMATE ABSTRACT HUMAN Egypt

HUMAN book virtue dirt John Fido

boy dog
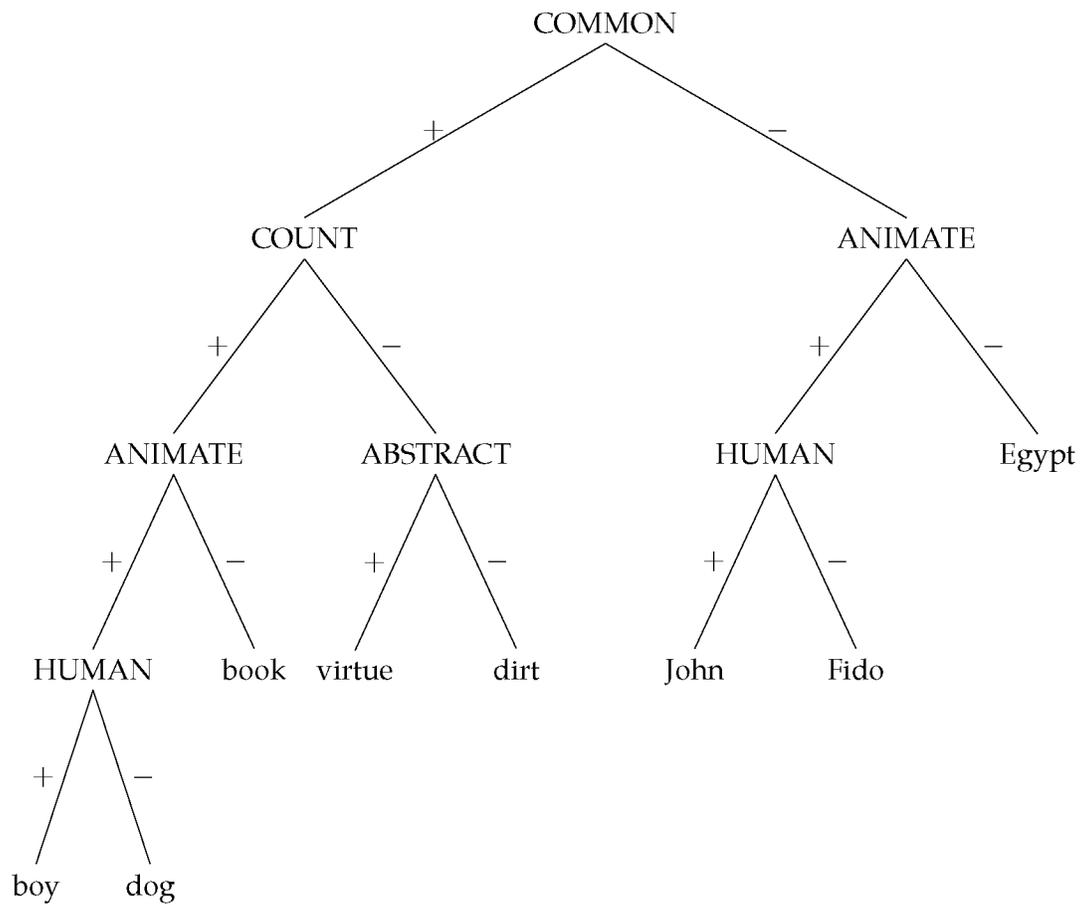
Figure 4.14: Tree representation of syntactic features for nouns (adapted from Chomsky 1965)

$T_E - T_F$

$T_B - T_C - T_D$

T_A

(3) (2) (1)
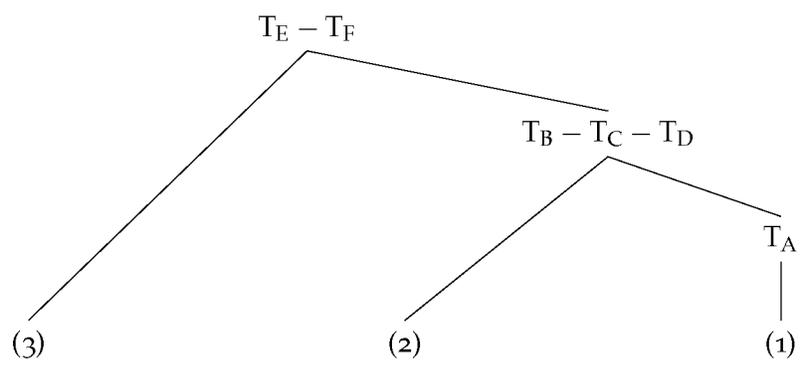
$T_A \ldots T_F$ Transformations

Figure 4.15: Tree representation of transformational rule ordering (adapted from Chomsky 1965)

Simple to use and manipulate, trees were widely adopted by transformational grammarians. Importantly, the visual clarity and versatility of tree structures helped offset the formal underpinnings of transformational theory. Trees – especially in conjunction with canonical examples – enabled linguists to understand and use transformations confidently without tackling their underlying justification. For example, pairs of tree diagrams showing the pre- and post-transformation versions of a construction gave a clear visual image of the effect of applying transformational rules to syntactic data. Chomsky's notational innovations also provided an effective and efficient method for representing syntactic patterns. Compared to the older notational techniques of the 1950s, rewrite rules and trees clearly demonstrated the empirical generalizations which underpinned the TG dataset. As such, this notation constituted a strong base on which transformationalists built their explanatory data criteria. Finally, the visual appeal afforded by tree structures made the transformation-as-tool particularly easy to use. This is in sharp contrast to stratificational grammar, where diagrammatic techniques were complex and strongly integrated into the broader framework of the theory.

* * *

Sydney Lamb's stratificational network diagrams, developed and released in the mid-1960s, were designed to provide visual interpretation to a theory which rejected the existence of linguistic units and elements. They depicted the relationship basis of linguistic structure through networks of lines and nodes, and labels were only included as a heuristic for the reader (cf. Chapter 2). While stratificationalists lauded Lamb's diagrams for their amenability to simplicity metrics, these diagrams were far from an asset to stratificational theory. Rather, they were detrimental to the growth and perpetuation of Lamb's linguistic program. Through the 1960s, stratificational diagrams were criticized for being complex and inelegant. Lamb's graphic notation caused "the eye [to] boggle", wrote Berkeley's Wallace Chafe in his 1968 review of Lamb's *Outline of Stratificational Grammar*.[96] With their resemblance to circuitry diagrams, he continued, they ought to appear in electrical engi-

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[96]Chafe, *Review of Lamb*, p 601.

neering textbooks, not linguistics articles. Charles Hockett – until the late 1960s an advocate of stratificational grammar – complained that network diagrams were "too cumbersome", that they "got in the way and diverted attention", and that they forced "a multitude of pseudo-questions on us".[97] Informally referred to as *Lambograms*, these diagrams gained a reputation as overly complex and unnecessarily difficult to use.

Where transformational diagrams were celebrated for being compact, easy to read, and manipulable, stratificational diagrams were frequently criticized on all these fronts. Network diagrams were visually cluttered and often filled several pages – a sharp difference from the simple layout and logical continuity of transformational diagrams. The application of these diagrams to multiple strata, and the diffusion of syntax over all strata, meant that the syntactic representation of any sentence consisted of several stacking circuitry diagrams. Further, the functioning of the nodes in Lamb's diagrams was not self-evident, and required repeated explication in articles and manuscripts. Linguists outside the school openly questioned the utility of stratificational diagramming techniques and, consequently, the validity of the theory as a whole. Lamb has become "so entangled in his ingenious patterns of lines and nodes [...] that he has lost sight of what a linguistic theory ought to be doing", wrote Chafe, who concludes that Lamb is "more interested in refining his notational system than he is in language".[98] Perhaps the ultimate statement on stratificational diagrams came in Thomas Priestley's *A Concise History of Modern Art*, published under the pseudonym Fom Pop in *Studies Out of Left Field*, an irreverent collection of underground papers originally released in 1971. Priestley's caricature of network diagrams with a Picasso-inspired Lysistrata beautifully captures the reputation of the diagrams in the 1960s: excessive, unnecessary, and difficult to read.

While Priestley's statement is in retrospect amusing, the consequences for stratificational theory were anything but. Stratificational diagrams proved hard to understand, and harder to teach. Network diagrams quickly gained a notorious reputation among faculty and students. Writing in 2008, Lamb admits that his notation "had an important negative im-

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[97] Hockett, *Review of Lamb*, p 151.
[98] Chafe, *Review of Lamb*, p 602.

pact" on the reception of stratificational theory in 1960s America.[99] "It was seen as [some-thing] foreign, therefore offputting, [something] that would require effort to learn to use and appreciate" – and, faced with the choice between stratificational and transformational diagrams, American linguists were not prepared to put in that effort.[100] The crux of the problem, Lamb asserts in retrospect, is that whereas "[o]ther notations used in linguistics were based on the already familiar", stratificational notation had no connection to previous methods.[101] The learning curve to enter the theory was relatively too steep.

Stratificational schools also diverged considerably in their use of notation. When reading the stratificational literature, David Lockwood notes, linguists were "discouraged by the lack of correspondence in terminology and concepts among the various works".[102] Strati-ficationalists at the Hartford Seminary Foundation, for example, only rarely used Lamb's network diagrams, and others preferred algebraic notation, or a combination of algebraic and graphic notation. Unlike transformational grammar, which was "relatively homoge-nous during its early period of development, when it was building the base of support it has come to enjoy", Lockwood argued in his 1972 textbook, stratificational grammar "never went through a period in which it was homogenous and monolithic" – giving transfor-mational theory a significant advantage in terms of learning and teaching.[103] The lack of uniformity in stratificational thinking was caused primarily by the existence of several schools from an early point in the theory's development, and from the late elaboration of network notation. While stratificational theory had been under development since the 1950s, Lamb's network diagrams were only first publicly presented in 1965, at the Univer-sity of Michigan Linguistic Institute. The gradual conversion from prose description and algebraic notation to network diagrams had a significant negative impact on the perception of stratificational grammar. In contrast, transformational grammar was led from a single center – MIT – and by a single leader – Chomsky – and maintained consistent and simple

[99]Lamb, *Personal Communication*.
[100]*Idem.*
[101]*Idem.*
[102]Lockwood, *Stratificational Linguistics*, p 286.
[103]*Idem.*

notation throughout its early years.

The weaknesses of stratificational diagrams reflected on the theory as a whole. As he grew disillusioned with the theory in the late 1960s, Charles Hockett painted stratification-alists as having become ensnared by their own notation: Lamb, he wrote, "has become so enslaved by his own frame of reference that [...] he can no longer distinguish between the object of linguistic investigation and the terminological and symbolic machinery we use in that investigation".[104] The effort required to use network notation meant that this notation itself had become a primary object of investigation. "It is much as though a pathologist were to develop the notion that slides and lenses are vectors of disease", Hockett concluded.[105] What had begun as criticism of network notation grew into a criticism of the fundamental assumptions of stratificational theory. While the stratificational language design assumptions were inherent in their notational techniques, Hockett continued, proponents of that theory were failing to provide any "empirical argument in support of this formulation of language design as over against others".[106] Stratificational grammar as a whole was rejected as poorly conceived and overly complex.

* * *

The visual appeal of transformational grammar, and specifically the relative visual advantage of TG over rival syntactic theories, translated directly to the pedagogical scene. The clarity provided by the transformational notation system made the fundamentals of that theory easy to teach. Transformational diagrams, Kenneth Hale asserted in 1965, are particularly easy for "beginning students" to understand.[107] In contrast, stratificational diagrams were difficult to understand and difficult to teach – and the theory was not a pedagogical success, only taught at a few universities and without a dedicated textbook until the early 1970s.

The success of transformational grammar in the university classroom – due in large part to the relative simplicity and clarity of transformational notation – was central to the even-

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[104] Hockett, *Review of Lamb*, p 147.
[105] *Idem.*
[106] *Idem.*
[107] Hale, *Review of Bach*, p 266.

tual dominance of that theory in American academic linguistics. A generation of young linguists was trained in a pedagogical environment which preferentially taught transformational theory. This pedagogical success was not restricted to the academic linguistics scene: through the 1960s, transformational grammar was increasingly adopted by language teachers in elementary schools, high schools, and colleges. Books such as Paul Roberts' *English syntax: A book of programmed lessons (An introduction to transformational grammar)* (Harcourt, Brace and World, 1964) and Owen Thomas' *Transformational Grammar and the Teacher of English* (Holt, Rinehart and Winston, 1965) applied transformational theory to improve students' abilities in composition, comprehension, and grammar (cf. Chapter 6). Critically for the broader success of the transformational framework, a link was drawn between pedagogical applicability and truth value: the implicit teachability of transformational grammar, Hale concluded, "says something significant about the comparative naturalness of the transformational framework for the description of natural languages".[108]

<p style="text-align:center">* * *</p>

As the 1960s opened, American academic linguistics was undergoing sweeping changes: linguistics departments were being founded at universities across the country; students were flocking to the discipline; and syntax was quickly becoming the subject of choice. By the end of the decade, over 100 universities offered one or more courses in syntax. With their emphasis on phonology and morphology, the linguistics textbooks of the previous decades no longer met the needs of the discipline. The textbook market – and with it the pedagogical market – were wide open. By capturing these markets, transformational grammar captured a generation of young linguists. This generation was trained in the transformational paradigm and developed a transformational worldview.

In the environment of the 1960s, Chomsky's transformations emerged as an ideal pedagogical tool – one which was well-designed for teaching and learning; one which was visually more elegant and more versatile than its competitors; and one which was quickly incorporated into textbooks and pedagogy. The pedagogical effectiveness of the transformation-

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[108]*Idem.*

as-tool was by no means assured: it was dependent on time and situation, both of which worked in its favor. The particular state of American academic linguistics in the 1960s – faced with unprecedented growth at universities, a real need to teach large numbers of students, and a lack of established textbooks – created an environment which selected for the syntactic tool which could best be used to train young linguists. Between its amenability to canonical examples and its visual appeal, the transformation-as-tool fulfilled the needs of students and teachers. In comparison to stratificational and constituency grammars, transformational theory offered a superior tool – one which was easier to teach, learn, and use, and one which consequently won over the pedagogical market.

Far from being side concerns, the disciplinary and pedagogical changes in 1960s American linguistics directly affected the theory-choice debates of that decade: the transformation-as-tool enabled linguists to deal with the particular structure and needs of their discipline, thereby ensuring that a generation of linguists would be trained in the transformational framework. Together with the propagation of transformationally-oriented linguistics departments in America, this meant that transformational grammar quickly became institutionalized on the nation-wide academic scene. Soon, stratificational grammarians were "greatly outnumbered" by proponents of Chomsky's transformational program.[109] The dominance of transformational grammar can only be understood in light of the influence of pedagogy on theory-choice and theory-diffusion in the 1960s. By not investigating pedagogy, existing literature on the rise of transformational grammar misses the specific role of the transformation as the most efficient available pedagogical tool for handling the emergence of linguistics as a separate academic discipline, and the impact of notational and diagrammatic techniques on the training of young syntacticians.

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[109]Lamb, *Personal Communication*.

5 Private Knowledge, Public Tensions

[I]f you weren't on the right mailing lists you could be seriously out-of-date in a few months.

Robert Stockwell, in *From English Philology to Linguistics and Back Again* (1998)[1]

[O]f the eighty transformationalist papers listed forty are available in microfilm or mimeograph at best, and upwards of fifteen are scarcely available at all...

P.H. Matthews, in the *Journal of Linguistics* (1972)[2]

These young men, in the first instance, talk only or chiefly to each other; exchanging Xeroxes and dittoed copies for a long time before communicating their paradoxes to the outside world.

Fred Householder, in *Linguistic Speculations* (1971)[3]

At the time, it was a sign of belonging to the inner circle of transformational theory – a shared culture which fostered a generation of linguists and shaped their work. Decades later, transformationalists looked back on it with a mixture of pride and disbelief, chutzpah and bemusement. It is, undisputedly, a defining aspect of early transformational grammar and one of the most colorful features of 1960s American linguistics. From 1957 to 1968, transformational grammar operated in an underground culture: work was narrowly circulated in mimeographed and dittoed form among a select group of linguists centered at

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[1] Stockwell, *To Linguistics and Back Again*, p 238.
[2] P.H. Matthews, "Review of Roderick A. Jacobs and Peter S. Rosenbaum's (eds.) Readings in English Transformational Grammar (1970)," *Journal of Linguistics* 8/1 (1972), p 125.
[3] Fred W. Householder, *Linguistic Speculations* (Cambridge UK: Cambridge University Press, 1971), p xi.

MIT. Those with close connections to that institution, where Chomsky and his colleagues worked, had privileged access to new research and theoretical results. In these years, transformational grammar was not primarily transmitted through mainstream publication routes: MIT transformationalists shunned academic journals, deliberately kept their work private, and maintained a tight network of communicants. It was common practice, James Fidelholtz recalls, to "circulat[e] papers only to a select few persons" – and, as a result, transformational work was "generally unobtainable for scores of scholars interested in the study of language".[4] Despite this underground culture, transformational grammar diffused across the country and, by the mid-1960s, dominated American academic linguistics on a nation-wide scale. While transformational grammarians valued the private transmission of knowledge, and while outsiders had difficulty accessing this knowledge, still the theory spread and gained influence. This chapter aims to resolve this apparent paradox: in light of its underground culture, it is essential to understand *why* and *how* the transformational paradigm achieved its dominance.

The existing literature on the rise of transformational grammar, described in Chapter 1, is seriously flawed because it does not take into account the prevalence of underground literature in the 1960s: it cannot explain the paradoxical success of this theory in light of its private culture. While some commentators – chief among them, Randy Allen Harris – have written on the underground culture of the generative semantics movement in American linguistics, which was active from the late 1960s to the early 1970s, this movement is separate from the initial rise of transformational grammar in the 1960s.[5] The two cases are very different, and need to be understood in their own historical contexts. By investigating the role and place of underground culture in transformational theory between 1957 and 1968, this chapter fills a lacuna in the existing historical literature.

I argue that three vital links are needed to fully explain the apparent paradox of the rise of transformational grammar in the 1960s: discipline dispersion and informal net-

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[4]Ney, *Private Knowledge*, p 143–144, James L. Fidelholtz, "Review of James D. McCawley's (ed.) Notes from the Linguistic Underground," *Language* 54/4 (1978), p 932.

[5]Harris, *Generative Semantics*, Harris, *Linguistics Wars*.

works, pedagogical appeal, and oral transmission. The 1960s saw the establishment of linguistics departments at universities across the country – departments which needed to fill teaching and research positions. Many of these departments were soon staffed by young transformational grammarians trained at MIT, who brought with them their connections to that institution and to its underground literature circulation network. Many shared this literature around their new academic homes, providing students with access to underground work. Further, the dominance of transformational grammar on the pedagogical scene meant that students were trained in the transformational paradigm regardless of access to underground literature – training made possible in large part by the publication of widely-distributed and easy-to-access textbooks on transformational theory. These textbooks formed an aboveground alternative to private knowledge, and were a key mechanism of knowledge dispersion in an otherwise underground culture. Finally, transformational grammar was also transmitted at conferences, colloquia, and Linguistic Society of America Linguistic Institutes, which were open to students and faculty regardless of theory affiliation. By attending lectures and workshops, even those outside of the transformational inner circle could learn about new transformational research. Together, these arguments explain *why* transformational grammar prevailed over rival syntactic theories, and *how* transformational grammar dispersed across America despite its pervasive underground culture. They also show that the success of transformational theory in the 1960s needs to be understood as the result of a carefully-balanced approach to private and public knowledge.

In order to frame these arguments, we first have to gain an understanding of the underground culture itself: its origins, motivations, and mechanisms. This chapter begins by placing transformational grammar's underground culture in context: section 5.1 explores the content and subtypes of this literature, its role in the transformational community, its distribution, and its impact on the American linguistics scene as a whole. Section 5.2 focuses on understanding the emergence, perpetuation, and proliferation of the underground culture. I show that rapid theoretical change, MIT's intellectual atmosphere, and job

market demands were all central to the development and maintenance of this culture. In section 5.3, I detail my explanation of the rise of transformational grammar in light of its underground culture. I show that three factors are needed to explain this apparent paradox: the dispersion of transformational grammarians from MIT and the subsequent formation of informal networks at universities across the country; the access to transformational theory provided by aboveground textbooks; and the oral transmission of the theory at conferences, colloquia, and Linguistic Institutes. Finally, in Section 5.4 I discuss the fate of underground culture after 1968, including the gradual emergence of underground literature in mainstream publications in the following decades.

The underground culture of transformational grammar not only caused problems for linguists unable to access transformational literature in the 1960s; it continues today to be particularly tricky for the historian. The defining characteristics of underground literature – unpublished, copied on poor quality paper, private, and circulated in small numbers – mean that this literature is now, decades later, difficult to locate and access. Nonetheless, it is possible – and immensely rewarding – to study. The historian's task is made easier by four factors: first, a selection of underground literature was published in monographs, anthologies, and Festschrift in the 1970s and 1980s. Supported by the very linguists who kept the literature private in the 1960s, these publications include papers which are otherwise not available from collections and archives. Second, several transformationalists active in the 1960s have recently commented on the underground culture in memoirs and in the prefaces and introductions to the aforementioned publications. The comments of, among others, James McCawley, Fred Householder and Robin Lakoff, provide first-hand recollections from linguists who were directly involved in the development and perpetuation of underground literature. They are particularly valuable for establishing original dates of writing and circulation, understanding motivations, identifying pseudonyms, and illuminating the mood of the era. Third, the Archibald A. Hill Library at the University of Texas at Austin contains an extensive collection of original underground literature documents. These documents arrived in Austin in the 1960s via an informal distribution

network headed by Stanley Peters. One of the largest linguistics collections on the continent, the Archibald A. Hill Library is a vital source for the history of American linguistics, and is uniquely well-endowed with underground documents. Finally, interviews with two linguists were particularly valuable for understanding the distribution network of underground literature – a feature difficult to reconstruct from other sources. Emmon Bach and Robert Stockwell, who worked at the University of Texas at Austin and UCLA, respectively, in the 1960s, graciously shared their memories of that era.

These four factors greatly assist the historian interested in studying transformational grammar's underground culture – but there remains a crucial difficulty. Since underground literature was by definition unpublished and informally circulated, no formal records were kept of it. Consequently, it is difficult to know how much of this literature has not resurfaced – and hence difficult to judge how much material the historian is missing. While the anthologies published in the 1970s and 1980s contain a few score influential underground documents, they by no means capture the whole body of underground work. Working with this literature is an exercise in, as Peter Novick puts it in his seminal study of objectivity and the American historical profession, "nailing jelly to the wall".[6] Like Novick, I take the advice of English historian G. Kitson Clark, who instructs historians tempted to make generalizations as follows: "do not guess, try to count, and if you cannot count, admit that you are guessing".[7] Missing literature cannot be counted – and so I will offer an informed guess. Based on a comprehensive examination of the period, my guess is that the majority of 'major' underground documents – articles, research papers, and lengthy commentaries – are available through anthologies or archives. I also guess that the majority of 'minor' underground documents – a category which includes notes, letters, marginalia, and brief commentaries – remain inaccessible, having been thrown away at the time or lost in the intervening decades. 'Minor' documents – so named for their relative length, not their importance – were, by their physical nature, not destined for permanence. The

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[6] Peter Novick, *That Noble Dream: The 'Objectivity Question' and the American historical profession* (Cambridge: Cambridge University Press, 1998).

[7] *Idem*, p 8.

documents – 'major' and 'minor' – which I have been able to access, however, combined with commentaries, recollections, and interviews, provide satisfactory and confident insight into the underground culture of the 1960s. This is all the historian – who, after all, has no control over the fate of documents during earlier decades – can ask.

5.1 Underground Literature in Context

Transformational grammar was immersed in an underground culture for more than a decade from its inception: between 1957 and 1968, documents ranging from research papers to doctoral dissertations to scrawled commentaries circulated privately within a narrow group of linguists centered at MIT. The main form of publication in this period, Emmon Bach remembers, was the ditto – and, indeed, underground literature took the form of handwritten documents, mimeographs, letters, and dittos.[8] This body of literature was by no means second-class or unimportant; rather, it included seminal works in transformational theory. Many key theoretical advances in 1960s transformational grammar were first communicated on the underground circuit, and remained there for years. This culture of private knowledge persisted until 1968, after which a proliferation of new linguistics journals guided transformational literature into the mainstream. This opening section places transformational grammar's underground culture in context: it explores the content, role, distribution, and impact of its literature, and identifies three subtypes of underground documents.

The underground culture arose quickly, and even by 1958 – only a year after Chomsky's *Syntactic Structures* was published – the private circulation of unpublished and pre-published papers was, Robert Stockwell recalls, "very active".[9] This literature circulated among a tight group of transformationalists, and outsiders had little, if any, access to underground documents. The cohesion and solidarity of this group have frequently been remarked upon by members of the group itself and by historians, and the monikers be-

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[8] Bach, *Interview*.
[9] Stockwell, *To Linguistics and Back Again*, p 238.

stowed have ranged from the mysterious to the religious: Fred Householder called it a "closed inner group" and "the fraternity"; Robin Lakoff called it "a group of dedicated co-conspirators"; Regina Darnell named it the "inner circle"; Randy Allen Harris called it "the faithful"; and Robert Hall referred to the student members as Chomsky's "disciples".[10] The defining attitudes and mentalities of this group – close, confident, driven, optimistic, and ambitious – were recognized by both insiders and outsiders, and will be central to understanding the underground culture they fostered and perpetuated.

In our period of interest – from 1957 to 1968 – three subtypes of underground literature need to be distinguished. First are manuscripts written in forms and styles appropriate for publication, but which were deliberately circulated privately and informally. Ranging from unpublished Ph.D. theses to substantial research articles to short replies, notes, and letters, these form the bulk of the underground transformational literature. They include, among others, Noam Chomsky's *The Logical Structure of Linguistic Theory* (written in 1955, published in 1975), Robert Lees' *What Are Transformations?* (written in 1960, published in 1976), and John Robert Ross' *Constraints on Variables in Syntax* (written in 1967, published in 1986). The common denominator among these works was their unavailability from mainstream publication outlets: they circulated as "unpublished mimeos, Xeroxes, and dittos" through the 1960s.[11] Together, these works laid out the fundamentals of transformational theory, established seminal research results, and guided the daily work of transformational grammarians.

Chomsky's *The Logical Structure of Linguistic Theory* is the earliest piece of underground literature in the transformational school and, in fact, predates the school itself. Chomsky completed his first version of this massive manuscript – it ran to 800 pages – in the spring of 1955, when he was a Junior Fellow at Harvard. It contains an early enunciation of transfor-

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[10] Householder, *Review of Makkai*, p 170, Lakoff, *The Way We Were*, p 943, Regina Darnell, "Introduction," in Arnold M. Zwicky, Peter H. Salus, Robert I. Binnick, and Anthony L. Vanek (eds.), *Studies out in Left Field: Defamatory essays presented to James D. McCawley on the occasion of his 33rd or 34th birthday*, 2nd edition (Philadelphia: John Benjamins Publishing Company, 1992), p 112, Harris, *Generative Semantics*, p 145, Robert A. Hall Jr, "Some Recent Developments in American Linguistics," in *American Linguistics 1925–1969: Three essays* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1976), p 71.

[11] Sadock in James D. McCawley (ed.), *Notes from the Linguistic Underground* (New York: Academic Press, 1976), p xi.

mational theory, including detailed discussions of topics only briefly covered in *Syntactic Structures*, such as simplicity measures and the inadequacy of finite state grammars. One chapter of this manuscript, entitled *Transformational Analysis*, became Chomsky's University of Pennsylvania doctoral thesis. In the two decades between 1955 and its formal publication by Plenum Press in 1975, *Logical Structure* circulated informally through Harvard and MIT. During the fall semester of 1955, the Harvard Libraries made two microfilms of the manuscript, one of the spring 1955 version and one of a slightly revised version. "It is these two microfilms and the duplicated 1955 version that have been distributed over the years", Chomsky wrote in the introduction to the 1975 publication: "I have not kept count, but there must be well over several hundred copies".[12] Still, in the early 1960s it remained difficult to obtain a copy: speaking at a 1962 lecture at the University of Michigan (Ann Arbor), Leiden University's E.M. Uhlenbeck complained of having "no access" to the manuscript, and Indiana University's Fred Householder was only able to obtain "the first three chapters" by means of a loan.[13] By far the most-copied and most widely distributed piece of underground literature, *Logical Structure* gradually became increasingly accessible. By 1965, Bach noted that "unfortunately it is still unpublished, but enough has been extracted [...] to make its major results readily available".[14] This early availability distinguished the manuscript from the bulk of underground literature, as did the wide knowledge of its existence: other underground documents led much quieter lives, and those outside the transformational inner circle were often not aware of their release or contents.

Two of the most influential underground literature documents in this first subtype are doctoral dissertations: John Robert Ross' *Constraints on Variables in Syntax* (MIT Ph.D. thesis, 1967; published in 1986) and Paul Postal's *Some Syntactic Rules in Mohawk* (Yale Ph.D. thesis, 1962; published in 1979). (While officially registered as a doctoral student at Yale,

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[12] Chomsky, *Introduction*, p 2–3.

[13] Fred W. Householder, "A Sketch of How I Came to be in Linguistics," in Boyd H. Davis and Raymond K. O'Cain (eds.), *First Person Singular: Papers from the Conference on an Oral Archive for the History of American Linguistics (Charlotte, N.C., 9–10 March 1979)* (Amsterdam: John Benjamins B.V., 1980), p 199, E.M. Uhlenbeck, "An Appraisal of Transformation Theory," *Lingua* 12/1 (1963), p 3.

[14] Emmon Bach, "Review of C.I.J.M. Stuart (ed.) Report of the Fifteenth Annual (First International) Round Table Meeting on Linguistics and Language Studies," *Word* 21 (1965), p 281.

Postal worked at MIT's Research Laboratory of Electronics.) Ross' thesis studied language-specific and language-universal constraints on grammatical operations. On the basis of these constraints, he showed that phrase markers can be partitioned into syntactic islands – subdomains whose boundaries act as barriers, or maximal domains of applicability, for various types of syntactic rules. Postal's dissertation presented a transformational analysis of Mohawk syntax which, in the subsequent years, formed the basis for Postal's efforts to rigorously prove that phrase structure grammars lacked the power necessary to account for natural language (cf. Chapter 3). Despite being seminal studies – Postal has described Ross' thesis as "one of the most important [works] in the history of so-called generative-transformational grammar" – and despite influencing core transformational research in the 1960s, these manuscripts circulated only on the underground network for over a decade.[15]

The second type of underground literature prevalent in the late 1950s and 1960s is the institutional, or laboratory, report. This includes, most prominently, the MIT Research Laboratory of Electronics Quarterly Progress Reports and the Harvard Computation Laboratory Reports. These reports served to keep the Cambridge and Boston linguistics communities up-to-date on current research and results, and were circulated narrowly within those communities. The Research Laboratory of Electronics Progress Reports were released regularly – four times a year in January, April, July and October – and consisted primarily of short, succinct statements of recent research results. The Progress Reports served not only the MIT linguistics community, but also the MIT groups for, among others, physical electronics, plasma dynamics, low temperature physics, microwave spectroscopy, molecular beams, and neurophysiology. In contrast, the Harvard Computation Laboratory issued lengthy reports on an irregular basis. The pinnacle of the Harvard Reports was George Lakoff's 1965 Ph.D. dissertation, *On the Nature of Syntactic Irregularity*. G. Lakoff's study of exceptions and transformational rules circulated underground for five years, during which time it was frequently cited in the underground and aboveground literature. Called "an underground classic" by James McCawley, *On The Nature of Syntactic Irregularity* was distributed

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[15]Postal in Ross, *Infinite Syntax!*, p xvii.

at first as a Harvard Report, and later on via xeroxed copies of that report – often times, second or third hand copies.[16] It was one of the first underground documents to be published formally, and was released by Holt, Rinehart, and Winston under the title *Irregularity In Syntax* in 1970.

The third and final type of underground literature are manuscripts which were deliberately written in forms inappropriate for publication, and circulated privately. Witty, satirical, and often downright rude, this style of underground literature is associated most prominently with the generative semantics movement (circa 1968–1974). In the late 1960s, however, it straddled the border between regular transformational literature and generative semantics. Postal's *Linguistic Anarchy Notes*, for example, commented on the progress of transformational research and exhibited syntactic and semantic phenomena which were unexplainable by transformational theory in its present state.[17] Inspired by discussions at the 1967 San Diego Syntax Festival (an informal conference held at the University of California at San Diego), the *Linguistic Anarchy Notes* were first circulated in 1967 "in photocopies that Postal mailed out to his drinking companions", and later in second and third generation copies.[18] Postal's notes inspired Jerry Morgan's *Cryptic Notes* (later renamed WAGs, or *Wild-Ass Guesses*), which tackled problems including reference and pronominalization. With its support for the generative semantics position, Morgan's work belongs to both the transformational grammar and generative semantics underground literature movements. The style and tone of these notes – of which their titles are representative – would come to be a trademark of the generative semanticists (cf. Section 5.4).

Between 1957 and 1968, these three types of underground literature included many significant advances in transformational research. "There are a number of important linguistic issues", wrote James McCawley in 1976, "that receive a more thorough and incisive treatment in [underground literature] than in anything hitherto published above ground".[19]

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[16] McCawley in George Lakoff, *Irregularity in Syntax* (New York: Holt, Rinehart and Winston, Inc., 1970), p v.

[17] Paul M. Postal, "Linguistic Anarchy Notes," in James D. McCawley (ed.), *Notes from the Linguistic Underground* (New York: Academic Press, 1976).

[18] McCawley in *Idem*, p 201.

[19] James D. McCawley, "Introduction," in James D. McCawley (ed.), *Notes from the Linguistic Underground*

This included work on instrumental phrases, the definition of grammaticality, syntactic islands, and extensions to non-Indo-European languages, among other issues. When Ross' *Constraints on Variables in Syntax* was published in 1986 – 19 years after completion – Postal commented that it had "influenced and stimulated in one way or another a massive amount of work".[20] Despite remaining unpublished for nearly two decades, Ross' manuscript was cited extensively in both underground and aboveground literature in the 1960s, and in the aboveground literature of the subsequent years. All the key figures in early transformational grammar, from Chomsky to Lees to Postal to McCawley, produced underground documents. This culture of private knowledge was, as we will see, a mechanism for group cohesion, a statement against mainstream publishing outlets, and a reflection of rapid research progress.

The rate of underground work peaked between 1964 and 1968, stimulated by the release of Chomsky's *Aspects of the Theory of Syntax* in oral form in 1964 and in written form in 1965. In these years, underground documents exceeded mainstream publications in transformational grammar. McCawley – himself a contributor to and distributor of underground literature – identifies two key reasons for this increase. First, in the same period – from 1964 to 1968 – transformational grammar solidified its position as the dominant syntactic theory in American academic linguistics. No longer compelled to constantly defend their theory and criticize rivals in public fora, transformational grammarians were able to devote more time and effort to "putting their theoretical house in order".[21] Since the underground culture scene was designed for theoretical work, this increase in research led directly to an increase in underground literature. Second, the transformational community ballooned in size in the mid-1960s. As the first generation of transformational doctoral students graduated from MIT, they took jobs at universities across America. Whereas transformational theory had previously "been for all practical purposes confined to Building 20 at MIT", in the mid-1960s it established a presence across the country, and quickly gained a large

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(New York: Academic Press, 1976), p 3.
  [20] Postal in Ross, *Infinite Syntax!*, p xvii.
  [21] McCawley, *Linguistic Underground*, p 5–6.

number of new graduate students and young faculty members.[22] With a significant increase in the number of transformationalists came an increase in the amount of research – and, consequently, in the amount of underground literature. As we will see, this dispersion of transformational grammarians from MIT to other universities also had the effect of widening the circulation of underground documents.

Underground literature clearly acted as an important means of research communication among transformational grammarians through the 1960s. It also had a second, equally important, function: it acted as a mechanism for deflecting criticism of transformational theory in public fora. At conferences and at Linguistic Society of America Linguistic Institutes, transformationalists commonly responded to criticism by citing or referring the critic to an underground literature document – a document whose existence, Gleason recalls, "the objector had no way of knowing about, and which he would for some years have no opportunity to see".[23] Speaking at the 1962 International Congress of Linguists in Cambridge (Mass.) for example, Chomsky referred his audience to Postal's unpublished work on the limitations of phrase structure grammars, and to his own unpublished work on problematic constructions.[24] For those outside the transformational circle, it was impossible to maintain critical arguments without access to key literature. Soon, Gleason continues, "[d]ebate on this level was no fun" – and, after a while, he "began going to the LSA [Linguistic Society of America] only out of a sense of duty".[25] Gleason's reaction was not unique: as early as 1964, non-transformationalists became fed up with the underground literature scene and with transformational polemics (cf. Section 5.2). There was a noticeable decline in the willingness of critics to publicly challenge transformational theory. "No more opponents came riding into Cambridge eager to joust with the champion [i.e., Chomsky]", wrote Robin Lakoff in her memoirs – a decline in critical opposition directly attributable to the underground culture.[26]

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[22]*Idem*, p 6.
[23]Gleason Jr, *Theories in Conflict*, p 65–66.
[24]Chomsky, *Current Issues*.
[25]Gleason Jr, *Theories in Conflict*, p 66.
[26]Lakoff, *The Way We Were*, p 968.

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Having forgone mainstream publication routes and their associated distribution mecha-nisms, transformational grammarians devised their own methods for distributing and cir-culating underground literature. Between 1957 and 1964, when transformationalists were concentrated at MIT, underground documents were passed hand-to-hand in offices, hall-ways, and colloquia rooms. In these years, when the community was relatively small and geographically constrained, distribution was easy. The situation became complicated as transformational grammarians began to disperse out of MIT, filling positions at universi-ties across the country. This dispersion peaked in the mid-1960s, simultaneous with the acceleration of underground work.

Outside of MIT, distribution relied on personal connections with that institution. As young transformationalists earned their degrees at MIT and traveled to teaching jobs across the country, they brought with them their connection to their theory's home base. They re-ceived underground documents through the mail and could, at their discretion, distribute them at their new institution. In this way, universities separated from MIT by vast distances were able to access the underground literature pot.[27] At the University of Texas at Austin, for example, Stanley Peters provided the necessary link with MIT: having completed a bach-elor's degree in mathematics at MIT in 1963 and several subsequent years of graduate work in linguistics at the same institution, he arrived in Austin to take up an assistant professor position in linguistics in 1966.[28] At Austin, Peters continued to receive underground lit-erature, and distributed it around his new academic home. The same role was played by Terence Langendoen at Ohio State University: immediately after receiving his Ph.D. from MIT in 1964, Langendoen moved to Ohio, where he became the fulcrum of an informal dis-tribution network for underground documents.

At Chicago, James McCawley presided over the circulation of underground work. Born in Glasgow in 1938, McCawley arrived in the United States in 1944. He entered the Uni-

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[27] Bach, *Interview*, Robert P. Stockwell, *Personal Communication with Janet Martin-Nielsen*.

[28] Not uncommonly for the era, Peters did not finish his doctorate before accepting an academic position; in fact, he never completed the degree.

versity of Chicago at the age of 16, and graduated in four years with a Masters in mathematics. After attending his first formal course in linguistics at the 1961 Linguistic Society of America Linguistic Institute, he undertook graduate work with Chomsky at MIT, receiving his doctorate in 1965. From that point on he gained a larger-than-life reputation for speaking "(at least) Dutch, German, Yiddish, Swedish, French, Spanish, Portuguese, Russian, Hindi, Hungarian, Mandarin, and Japanese".[29] A year before finishing his Ph.D., he took a position as an assistant professor of linguistics at Chicago. His involvement in underground culture as a contributor and as a distribution-point spanned the transformational literature of the 1960s and the later generative semantics movement, in which he played a leading role. For years, he was "close to the top of the distribution list for most people's 'underground' papers", and he developed a reputation as an active distributor of underground work: at Chicago, he recalls, colleagues would attempt to circulate papers "by placing copies of them in my mailbox, in the belief that they would then automatically become part of an oral tradition".[30] These informal university networks were, as we will see, essential to ensuring the access of students to underground literature.

Outside of MIT, distribution networks were almost exclusively controlled by a linguist who had completed graduate work or post-graduate research at MIT. The situation at UCLA, however, marks an important exception: at the California school, Robert Stockwell was able to access underground literature in transformational grammar despite having never attended MIT. Stockwell completed his three degrees at the University of Virginia and subsequently worked at the Foreign Service Institute School of Languages before taking a position in the UCLA English department in 1956. (At Stockwell's instigation, UCLA established an inter-departmental program in linguistics in 1960, a center for research in languages and linguistics in 1963, and a linguistics department in 1966.) Stockwell was "converted" to transformational grammar by Chomsky's presentation at the 1958 Texas Conference, organized by Archibald Hill at the University of Texas at Austin.[31] Upon return to UCLA,

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[29]John Lawler, "James D. McCawley," *Language* 79/3 (2003), p 614.

[30]Fidelholtz, *op. cit.*, p 930, McCawley, in Jerry Morgan, "Cryptic Note II and WAGS III," in James D. McCawley (ed.), *Notes from the Linguistic Underground* (New York: Academic Press, 1976), p 337.

[31]Stockwell, *Personal Communication*.

he read "everything on transformational-generative theory that had been published and a great deal that had not" – a task made difficult, he recalls, because "if you weren't on the right mailing lists you could be seriously out-of-date in a few months".[32] Working in California – over 4000 kilometres from the East Coast – and with no connections to MIT, Stockwell spent five years with no access to the 'right' mailing lists. In the 1963–1964 academic year, he visited MIT as part of a sabbatical leave. There, he "had carte blanche to walk into any class", attended lectures given by Noam Chomsky, Paul Postal, and others, and "got acquainted with everyone [he] wanted to know".[33] But the real benefit of the visit came afterwards: "I made enough friends such that I got on all the mailing lists", he recalls, and "after I came back to UCLA I distributed the stuff widely among my colleagues and students".[34] The connection between UCLA and MIT was solidified when Barbara Hall Partee, having completed her Ph.D. under Chomsky at MIT in 1965, accepted a position at the California school that same year.

The relationship between MIT and distributors of underground literature at other universities was informal and fluid: not everyone received all literature, and circulation networks had no central organization. The only requirement for staying on mailing lists was active participation in transformational research and "sending back comments and questions".[35] At Austin, Chicago, and UCLA, underground literature was distributed quite freely, but because of its informal nature it lacked the "guarantee" provided by mainstream journals and the present-day electronic Linguist List.[36] At the University of Massachusetts, sign-up sheets were regularly posted for those interested in receiving underground papers.[37] Some underground literature was also distributed through university linguistics clubs including, most prominently, the Chicago Linguistic Society (founded 1951) and the Indiana University Linguistics Club (founded 1967–1968). The Indiana Club, for example, was responsible for the distribution of John Robert Ross' *Constraints on Variables in Syntax*.

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[32]Stockwell, *To Linguistics and Back Again*, p 238.
[33]Stockwell, *Personal Communication*.
[34]*Idem*.
[35]*Idem*.
[36]*Idem*.
[37]Bach, *Interview*.

For those with personal connections to MIT, obtaining underground literature was usually easy: MIT transformationalists were, Stockwell recalls, "unfailingly generous" and never declined requests for unpublished papers.[38] For those outside the transformational inner circle, however, the situation was entirely different: underground documents were difficult, and often impossible, to access. Educated at Wheaton College (Illinois) and the University of Michigan, James Ney worked in the 1960s in Florida (1961–1962), Okinawa, then under United States Civil Administration (1962–1964), and Michigan State University, East Lansing (1964–1969). For him, and for fellow outsiders, underground transformational literature was "generally unobtainable".[39] Transformational documents, he recalls, "seemed to be circulating in unpublished form among a coterie of linguists who constituted the inner circle of the early transformational movement" – an inner circle which actively restricted access to their work.[40] For those in Europe, the situation was similarly dire. Working in the Netherlands, E.M. Uhlenbeck complained in 1963 that "[n]ot all of Chomsky's 13 publications on syntax are generally available. To his thesis called 'The Logical Structure of Linguistic Theory' quoted by himself and by Lees several times, I had no access. His two contributions to the 3rd and 4th Conference on Problems in the Analysis of English held in Texas were also not accessible. The same goes for his article on 'Explanatory Models in Linguistics'".[41] This lack of accessibility, Uhlenbeck continued, had led to an "annoying gap in my documentation [which] will exert a distorting influence on my rendering of his [Chomsky's] ideas".[42] P.H. Matthews, working in Cambridge, was more affronted, arguing the underground culture of the 1960s constituted "a serious discourtesy to all but a handful of readers", impeding progress in the field and restricting the circulation of knowledge.[43] Scholars outside of the transformational circle, whether in the United States or in Europe, were excluded from key developments in transformational grammar and, consequently, were unable to meaningfully criticize that theory or contribute to its

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[38]Stockwell, *Personal Communication*.
[39]Ney, *Private Knowledge*, p 143–144.
[40]*Idem*.
[41]Uhlenbeck, *An Appraisal of Transformational Theory*, p 3.
[42]*Idem*.
[43]Matthews, *Review of Jacobs and Rosenbaum*, p 125.

advancement.

Not only was it hard to access underground literature: for those outside the transformational inner circle, this work – when it could be accessed – was hard to read and understand. The exclusivity of underground literature was reinforced by practices which implicitly restricted full comprehension to insiders. Citations, references, and footnotes often referred "only to unpublished papers and oral communication", and initialization (the practice of referring to authors only by their initials, and not by their full names) was rampant.[44] In John Robert Ross' *Constraints on Variables in Syntax*, for example, 32 documents in his 67-item bibliography – just under half of the items he references – are listed as "unpublished paper", "unpublished mimeograph", "mimeographed", "unpublished Ph.D. thesis", "working paper", or are listed as appearing in technical project reports.[45] He includes nine of his own works, none of which had been published: one unpublished paper, one unpublished mimeograph, his master's thesis, one unpublished ditto, two Harvard Computation Laboratory Reports, and three "to appear" articles.

For non-transformationalists, underground literature was hard to access and hard to understand – and it was also hard to know when new items were released and distributed, and what documents were circulating at any time. In the 1960s, the Center for Applied Linguistics made two efforts to normalize this situation. Founded in 1959 under the instigation of Stanford linguist Charles Ferguson, the Center for Applied Linguistics was designed to serve as a liaison between academic linguists and "the practical world of language education and language-related concerns".[46] Originally funded by the Ford Foundation, and soon after by the National Science Foundation, the Center for Applied Linguistics enjoyed a million-dollar annual budget and a 100-person staff through the 1960s.[47] In this decade, the Center served, among other respects, as a clearinghouse for the collection, distribution, and analysis of linguistic knowledge. In 1965, the Center published a manuscript entitled *Information Sources in Linguistics: A bibliographical handbook*, intended to assist upper under-

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[44]Householder, *Linguistic Speculations*, p ix.
[45]John Robert Ross, *Constraints on Variables in Syntax* (Ph. D. diss.), MIT, 1967.
[46]CAL50 Center for Applied Linguistics, Retrospective: The CAL Story.
[47]Ferguson, *Long-term Commitments*, p 50.

graduate and graduate students by "meet[ing] the need for a handbook describing existing sources of information in linguistics".[48] This handbook was by no means restricted to transformational literature: it covers historical linguistics, structural linguistics, phonology, syntax, transformationalism, stratificationalism, and tagmemics, among other topics. True to its aims, only a minority of the sources listed are journal articles. By far more prevalent are monographs, bibliographies, conference proceedings, books, maps, and surveys. To combat the underground culture in transformational grammar specifically, the Center for Applied Linguistics also established the Program for Exchange of Generative Studies (PEGS) in the mid-1960s – a program designed to provide "rapid dissemination of un-refereed papers in the field of generative grammar".[49] Neither the bibliographical handbook nor the Program for Exchange of Generative Studies, however, had any significant influence on transformational grammar's underground culture. Despite these attempts to improve access to and circulation of linguistic knowledge in America, outsiders remained unable to break into the transformational culture.

Finally, the characteristics of underground literature also spilled over into aboveground literature. In their mainstream journal articles and monographs, transformational grammarians frequently referenced and referred to underground documents, making it difficult for those outside the transformational inner circle to enter the literature and to follow up on aboveground work. In his 1968 article *Instrumental Adverbs and The Concept of Deep Structure*, published in the journal *Foundations of Language*, for example, George Lakoff references five works, only two of which are aboveground publications.[50] The remaining three references consist of the author's Ph.D. thesis, *On The Nature of Syntactic Irregularity*, only available on the underground scene; a report from the Harvard Computation Laboratory; and a "mimeographed" paper by James McCawley. Anthologies fared no better: of the 80 papers on transformational grammar listed in the bibliography of Roderick Jacobs'

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[48] Frank Rice and Allene Guss (eds.), *Information Sources in Linguistics: A bibliographical handbook* (Washington DC: Center for Applied Linguistics, 1965), p iv.

[49] A. Hood Roberts, "International Information Flow in Linguistics," *Linguistic Reporter* 10/6 (1968), p 2.

[50] George Lakoff, "Instrumental Adverbs and the Concept of Deep Structure," *Foundations of Language* 4/1 (1968).

and Peter Rosenbaum's *Readings in English Transformational Grammar* (1970), reviewer P.H. Matthews noted, "forty are available in microfilm or mimeograph at best, and upwards of 15 are scarcely available at all".[51] These are referred to in the bibliography as "unpublished paper", "untitled paper", "remarks delivered at", and so on. Matthews also highlights the narrow range of authors listed in the bibliography: of the 94 items, 48 are written by the contributors themselves, and of the remainder 32 are "by writers in an identical vein, mostly students or former students at MIT" – a range which accurately portrays the tightness and solidarity of the transformational community in the 1960s.[52]

<p style="text-align:center">* * *</p>

The underground culture of transformational grammar thus played a central and defining role in the early development of that theory. It effectively acted as a surrogate for mainstream academic publishing: transformational grammarians regularly shunned traditional journals and chose to circulate research results on the underground circuit. Having explored the types, content, role, and distribution of this literature, we now turn to understanding the motivations underpinning the underground culture.

5.2 Underground Literature in Perspective

The underground literature culture was a defining part of transformational grammar for over a decade, arising quickly in the late 1950s and persisting until the close of the 1960s. The existence of such a culture is, seemingly, at odds with the value placed on academic freedom and open scientific knowledge in the mid to late 20th century. In other disciplines from biology to physics, publication in mainstream journals and the open circulation of knowledge was the norm. Among other advantages, it was considered necessary in order to claim intellectual priority. In the post-World War II years, American physicists frequently circulated preprints of their papers – but, unlike the transformational grammar literature, physics preprints constituted public, not private, knowledge. Preprints (that is,

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[51]Matthews, *Review of Jacobs and Rosenbaum*, p 125.
[52]*Idem.*

early copies of papers proposed or accepted for publication in mainstream journals) were designed to "bring news of new work immediately" to the physics community.[53] Before sending his article *The S Matrix in Quantum Electrodynamics* to *Physical Review* in 1949, for example, Freeman Dyson prepared 100 copies of the manuscript for open distribution at a New York American Physical Society meeting.[54] In contrast, transformational grammarians were riding on what they saw as the cutting edge of linguistics, and yet shunned mainstream journals and publication mechanisms. Understanding the appeal of private knowledge for transformationalists is essential to understanding the rise of transformational theory. This section explores the reasons for the development and perpetuation of an underground culture in 1960s transformational grammar. I argue that three factors are necessary for understanding this culture: rapid theoretical change and a paucity of mainstream journals; MIT's intellectual atmosphere and the transformational community's polemics; and job market demands.

The rapid pace at which transformational theory was changing and evolving in the 1960s is commonly recognized by both contemporaneous linguists and historians, and the connection to underground culture is well-established. The theory was simply changing too quickly for transformationalists to wait out the journal publication cycle, which typically took a minimum of several months from submission to appearance in print. "[C]hange in linguistic theory is so rapid that nowadays you have to specify not only whose theory you are talking about, but what year's model you have in mind", wrote the University of Florida's John Algeo in the *Journal of English Linguistics* in 1969: "Chomsky's *Syntactic Structures* of 1957 is already being referred to as 'classical transformational theory', thus placing it in the same category as *Oedipus Rex* and the Phidian Jove".[55] Algeo diagnosed transformational grammar as suffering from Detroit syndrome – the same disease afflicting those who trade in their car for a new model every year.[56] Through the 1960s, transformational grammar grew by leaps and bounds, continually supplanting itself. The theory and its

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[53]Kaiser, *op. cit.*, p 80.
[54]*Idem.*
[55]Algeo, *Stratificational Grammar*, p 1.
[56]*Idem.*

practitioners were imbued with a pressing desire for speed and new results. "One had the feeling of grammar in the making", Dwight Bolinger recalls: "we were out exploring a freshly turned field, with everyone retrieving specimens before the bulldozers moved in".[57] At conferences, lectures, and colloquia, the course of an afternoon could see the birth and death of a handful of theoretical modifications. "It called for fast thinking on one's feet", Bolinger continues – especially in the wake of Chomsky's *Aspects of the Theory of Syntax*, which accelerated the rate of change of an already fast-moving theory.[58] In this climate, to wait for publication in a journal would have meant intellectual catastrophe for the linguist: he would have been left months or years behind the rest of the transformational community.

Even as transformational theory was changing rapidly, the American linguistics profession was underserved by mainstream journals. The three mainstays of post-World War II linguistics publishing – *Language, Word,* and *The International Journal of American Linguistics* – did not have enough capacity to keep up with the growing profession. Even as linguists turned to other journals – including *Modern Language Review, Journal of English Linguistics,* and *Foundations of Language* – the amount of theoretical linguistic research being produced in the United States far exceeded collective journal capacity for this work. None of these journals were interested in restricting themselves solely to articles on syntax, or even on theoretical linguistics more broadly. To varying extents, their editorial policies favored publishing on a wide range of subjects from anthropological linguistics to historical linguistics to language style to linguistic theory. The 1964 publication year of *Language*, for example, boasted 23 articles (excluding obituaries and book reviews), nine of which were on historical or comparative linguistics, four on phonology and/or morphology, two on sociolinguistics, three on transformational grammar, zero on stratificational grammar, and five on a variety of other topics. That the premier American academic linguistics journal of the era only published three articles on transformational theory in all of 1964, and none on its chief rival syntactic theory, is representative of the very little space allotted to

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[57] Bolinger, *First Person*, p 29.
[58] *Idem.*

syntacticians in mainstream journals through the 1960s. By the mid to late 1960s, the disparity between the number of syntacticians in America and the number of spots for their work in journals had reached "epidemic proportions".[59] This disparity provided ample motivation for transformational grammar's underground culture, especially as the transformational community ballooned in the middle of the decade. In this way, underground literature fulfilled a necessity – providing an alternative outlet for transformationalists to release and distribute results – while also acting as a mechanism of group cohesion and identity.

Problems with mainstream journals were not limited to a paucity of slots for theoretical articles: the journals themselves faced technical and financial problems which made them unreliable and, at times, inaccessible. Universities with fledgling linguistics programs suffered from delays in acquisition and subscription delivery, and issues of *Language* and other journals were frequently not on shelves. "Even if the individual researcher knows of a particular journal and succeeds in finding a library which carries it", A. Hood Roberts wrote in the *Linguistic Reporter* in 1968, "he may find that the most recent issue available to him is two years out-of-date".[60] Publication lags and financial difficulties plagued the industry through the mid to late 1960s. The March 1967 issue of *Language*, for example, was not published until 1968, and the December 1965 issue of *Word* was released in 1967. *Word* faced the brunt of the financial difficulties – at the peak of its problems, Fred Householder called the journal "a dubious vehicle" which could be up to five years behind schedule.[61] Between this set of delays and the small number of places for syntax articles, transformational grammarians judged mainstream journals to be incapable of fulfilling their requirements.

Transformationalists did not entirely abandon mainstream journals during the 1960s: this medium of communication and knowledge distribution was deemed suitable for articles which did not bear on immediately-current theory and which were not time-dependent. Most prominently, members of the transformational community published

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[59]Di Pietro, *New Vistas*, p 16–17.

[60]Roberts, *International Information Flow*, p 2.

[61]Householder, *Review of Makkai*, p 170.

dozens of book reviews in mainstream journals through the decade. Nonetheless, the majority of their research was restricted to the underground scene.

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Equally important to the development of underground culture was the intellectual atmosphere within the transformational grammar group at MIT. "The sense of camaraderie was there from the start, in TG's 'us against the world' format", Robin Lakoff wrote in her memoirs – a camaraderie which quickly grew to value loyalty, confidence, and privacy.[62] The first generation of transformational grammarians – most of them graduate students or young researchers, and many coming to linguistics for the first time from a variety of other disciplines – were united by the feeling of being a part of a radically new approach to syntax, by their association to the prestigious Research Laboratory of Electronics and to Chomsky, and, soon, by their vitriolic argumentation style and tactics. Intelligent and confident, they quickly formed a tight, close-knit group. They have been described by themselves and by historians as "unusually strong-minded, even abrasive", "ambitious", "cock[y]", and "provocative" – traits which were encouraged by MIT's intellectual atmosphere and research-intensive reputation.[63] This atmosphere, combined with the early success of transformational theory, led to the formation of a closed inner circle – and to the development and perpetuation of an underground culture.

By the early 1960s, MIT transformational grammarians had become increasingly disinterested in communicating their research results to the broader American linguistics community: they "talk[ed] only or chiefly to each other".[64] Commitment to the group meant that members valued the private transmission of underground knowledge more highly than publication in mainstream journals. The prevailing view within the group, James McCawley recalls, was that "anyone capable of appreciating their work probably read the MIT Quarterly Progress Report and the Harvard Computation Laboratory Reports anyway, so what need was there to publish anywhere else?".[65] This statement sums up the transfor-

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[62] Lakoff, *The Way We Were*, p 963.
[63] *Idem*, Harris, *Linguistics Wars*, p 68, Lightner and Zwicky in Sadock and Vanek (eds.), *op. cit.*, p xii.
[64] Householder, *Linguistic Speculations*, p xi.
[65] McCawley, *Linguistic Underground*, p 2.

mationalist attitude and their valuation of private knowledge in a nutshell: for the transformational grammarians working at the Research Laboratory of Electronics, the world revolved around MIT. There was sufficient intellectual stimulation within the group that outside interaction was deemed unnecessary. Indeed, the MIT linguistics group grew by leaps and bounds, and by 1967 it boasted 21 academic and research staff and 33 graduate students.[66] Outsiders were seen as incapable of fully understanding transformational research, and unworthy of being granted access to this work.

The underground culture and the group mentality of early transformational grammarians were self-reinforcing. The transformational community was organized into a hierarchy which encouraged group cohesion and the maintenance of private knowledge. "There were the inner circle, the various outer circles, Limbo, and Bad Guys", Robin Lakoff recalls – and those who penetrated into the inner circle had substantially better access to transformational research results.[67] Indeed, the one-term MIT course on non-transformational linguistics was colloquially known as "the Bad Guys".[68] Establishment credentials were predicated on group solidarity and research results – and results were expected to be released as part of the body of underground literature. Linguistics graduate students at MIT, for example, were required to conduct their thesis research in the transformational framework, regardless of whether they were funded by grants intended for transformational grammar *per se*, or grants intended for other programs such as machine translation.[69] Adherence to the culture of private knowledge, then, was necessary for success within the group. In a 1972 interview, Wallace Chafe drew a political metaphor, asserting that "[t]here is a single line, a sort of party line, which has to be followed in order to belong to the field of [transformational] linguistics".[70] It was a party which valued private knowledge more highly than public knowledge – and which could afford to do so in large part because of its size, early success and secure funding situation.

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[66]Research Laboratory of Electronics, *Quarterly Progress Report 1967*, p 273.

[67]Lakoff, *The Way We Were*, p 943–944.

[68]Koerner, *The 'Chomskyan Revolution'*, p 173, note 18.

[69]Yngve, *Early Research*, p 66.

[70]Parret, *Interview with Chafe*, p 4.

Underground culture was also a key point of attraction for students. Graduate students in transformational theory, Fred Householder recalls, were attracted to "the style of argument, from second-order implied premises that are assumed to be obvious to the initiate, with footnoted references only to unpublished papers and oral communication".[71] Students worked, it is important to remember, at the cutting edge of transformational research through the 1960s: they were not producing insignificant or secondary results, but were, in contrast, making seminal inroads in transformational theory. From Robert Lees' aboveground work on English nominalizations to the underground doctoral dissertations produced by Paul Postal, John Robert Ross, and George Lakoff, graduate students produced the bulk of transformational ideas – many of them significant – through the decade. The commitment of these students to underground culture, and to the trappings of transformational culture more broadly, was fostered by three factors, which we will discuss in turn: a rejection of the past, Chomsky's personal influence, and transformational polemics.

In the early years of transformational grammar, group solidarity was built on a rejection of what had come before – that is, of the Descriptivist, or Bloomfieldian, linguistics of the previous three decades. Transformationalists presented their theory as a replacement for the flawed constituency theory of the 1950s, and painted Bloomfieldians as the "common enemy" against which they were battling.[72] Constituency grammarians, Lees wrote in 1957, merely "reorganiz[ed] the data into a new kind of library catalog" – but transformational grammarians offered a "serious attempt[] [...] to construct within the tradition of scientific theory-construction a comprehensive theory of language".[73] Chomsky and his early colleagues made it their mission to speak out, frequently and repetitively, about the flaws of 1950s syntax. From the 1958 Texas Conference on, no Descriptivist paper went unchallenged. The climate created was one in which commitment to the transformational community was interpreted as a sign not only of accepting transformational theory, but also of an outright rejection of previous work. Frederick Newmeyer emphasizes the im-

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[71]Householder, *Linguistic Speculations*, p viii.
[72]Lakoff, *The Way We Were*, p 963, Gleason Jr, *Theories in Conflict*.
[73]Lees, *Review of Chomsky*, p 377.

portance of public performance: "[s]eeing the leaders of the field [that is, the leaders of the Descriptivist program] constantly on the defensive at every professional meeting", he writes, "helped recruit younger linguists far more successfully and rapidly than would have been the case if the debate had been confined to the journals".[74] For young linguists, joining the transformational community meant renouncing what had come before – and hence the transformational community alone had to provide shared purpose, community structures, and a sense of belonging. There was no admissible history, no admirable predecessors, for students to look back on. Purpose, support, and identity were provided by the transformational inner circle, built on and reinforced by underground culture.

By the early 1960s, students of transformational grammar at MIT were no longer required to read linguistics literature from the 1940s and 1950s: the work of Bloomfield, Wells, and Harris was absent from the curriculum, and a generation of students matured knowing these names only from "contemptuous discussions by Postal or Chomsky".[75] The Descriptivist conception of linguistics, Lees wrote in *Language*, is merely a "reordering of the data [...] according to an arbitrary set of descriptive labels", a study done "without giving any internal linguistic justification" and intended to be "just a classification of utterance fractions so that they may be successively mentioned from the first to the last page of the grammar in some manner other than randomly".[76] While this description is arguably representative of early Descriptivism, it does not present an accurate portrait of the post-World War II work of constituency grammarians, who clearly stated both internal and external justifications for their analyses. In his 1967 review of Chomsky's *Current Issues in Linguistic Theory* and *Aspects of the Theory of Syntax*, published in *American Anthropologist*, Sydney Lamb asserted that transformational students did not know "what neo-Bloomfieldian linguistics was really like", and that they had been "led to the false impression that all linguists before Chomsky [...] were hopelessly misguided bumblers, from whose inept clutches Chomsky has heroically rescued the field of linguistics".[77]

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[74] Newmeyer, *Linguistic Theory in America*, p 50.
[75] Householder, *Linguistic Speculations*, p viii.
[76] Lees, *Review of Chomsky*, p 376.
[77] Lamb, *Review of Chomsky*, p 414.

This practice soon spilled over to present-day literature: transformational grammarians, Householder recalls, "simply never read the non-transformational literature" – including the work of Lamb and his fellow stratificationalists and the theoretical work of constituency grammarians.[78] The resulting selective ignorance reinforced the sense of a closed group of practitioners, and also provided a measure of protection against outside criticism. By deliberately staying away from issues important to other schools, such as computerizability and supra-sentence phenomena, transformationalists shielded themselves doubly from criticism: first, they could reply to critics by citing underground literature to which the critics had no access and, second, they could reject outside suggestions without devoting time and energy to considering them in detail. This was "usually not out of malice", Householder insisted, but rather out of "mere ignorance": as implied by their underground culture, transformational grammarians simply paid little attention to what went on outside their own world.[79] Despite Householder's apologetic explanation, however, non-transformationalists were not amused: in his aggressive 1980 commentary on transformational grammar, J. Peter Maher asserts that "if scholarship is thought of as including knowledgeable awareness of the work of others, TG is clearly lacking".[80]

The rejection of non-transformational ideas is best seen through the reaction of transformational grammarians to books written by linguists advocating other points of view. Entire books were ignored by the transformational community – books which gained followings and had influence in other linguistic schools. Chief among these are Charles Hockett's 1968 *State of the Art* (a sweeping criticism of transformational theory first presented in 1966, and published two years later) and Fred Householder's 1971 *Linguistic Speculations* (a compendium of Householder's views on language, from the characterization of languages to grammatical rules to dialectology). "It would be foolishly optimistic to suppose that this book will convince many of Chomsky's followers", wrote F.R. Palmer in his 1969 review of *State of the Art*, because transformationalists are "not open to discussion of

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[78] Householder, *Review of Makkai*, p 170.
[79] *Idem.*
[80] Maher, *op. cit.*, p 12.

the kind presented by Hockett; the reply will simply be that he is quite wrong in his basic assumptions".[81] Indeed, George Lakoff called *State of the Art* "very much a voice out of the past, doting on dead issues and nursing old misunderstandings" in 1969.[82] "[T]his book lacks any substantive discussion of actual linguistic data", he continued, and the result is a "confused and inconclusive discussion [and] an empty book, of no empirical linguistic significance at all".[83] In his 1980 celebratory history of transformational grammar, Frederick Newmeyer asserts that "the impact of *State of the Art* has been nil" – a statement which accurately reflects the transformational community's reaction to the manuscript, but which fails to recognise that it was highly influential among non-transformational linguists.[84] In a particularly satisfying description of the situation, Gleason refers to "the limbo of books that are not recognized to exist".[85]

The MIT linguistics group, and the American transformational grammar community more broadly, pivoted around one larger-than-life personality: Noam Chomsky. Exerting "overwhelming influence" on his students and colleagues through the 1960s, Chomsky was himself responsible for creating an environment which valued group solidarity and private knowledge.[86] Young transformationalists "all felt they owed allegiance deeper than professional connection to Chomsky", recalls Robin Lakoff – an allegiance which, at times, "verged on worship".[87] Less flamboyantly, but equally illustratively, Robert Stockwell remarks that "Chomsky has the best mind I have ever been privileged to access" – a mind which made "[t]he years from 1958 to the mid 70's [...] the best of my entire intellectual life".[88] There are few academics who have been as prolific: in the early 1990s, he was declared one of the most cited living scholars by the *Arts and Humanities Citation Index* and the *Social Science Citation Index*, and he had supervised or contributed to supervising nearly

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[81] Frank R. Palmer, "Review of Charles F. Hockett's The State of the Art," *Language* 45/3 (1969), p 621.
[82] George Lakoff, "Review of Charles Hockett's The State of the Art (1968)," *Foundations of Language* 5 (1969), p 1.
[83] *Idem*, p 1, 10.
[84] Newmeyer, *Linguistic Theory in America*, p 47.
[85] Gleason Jr, *Theories in Conflict*, p 200.
[86] Lakoff, *The Way We Were*, p 941.
[87] *Idem*, p 963.
[88] Stockwell, *Personal Communication*.

150 graduate students.[89] A skilled and compelling thinker and leader, Chomsky built a group of linguists who were deeply committed to their theory and their community. The concentration of transformationalists at MIT in the early 1960s also served to reinforce the sense of group belonging: it wasn't until 1964 that newly-graduated Ph.D.s began to leave MIT to take jobs at universities across the country. Those first years – from 1957 to 1964 – were critical to building the transformational community into a tight knit group – a group which retained its connections and commitment to its theory and to Chomsky even as it spread to other institutions.

Chomsky's star rose quickly, and in the 1960s an endorsement from the leader of transformational theory – then still in his thirties – could raise a student's status and be essential to securing a job. As the intellectual father figure among transformationalists, Chomsky's words carried tremendous weight. In writing and in speech, Chomsky repeatedly asserted that transformational grammar was the only syntactic theory worth pursuing and, consequently, that the transformational school was the only linguistic school worth belonging to. When asked in an interview conducted by Ved Mehta who he thought were "the leading figures in the field as a whole, anywhere in the world [outside transformational grammar]", Chomsky replied, "there aren't any. Most of the interesting work in linguistics is now being done here in the United States, and most of it is being done by transformationalists".[90] This response is representative of the attitude Chomsky instilled in the MIT linguistics group: linguistics would advance only through the study of transformational theory, and belonging to the transformational community offered the only chance of progress and success. It was, indeed, 'us against the world': a worldview and attitude which rewarded group cohesion and commitment.

Those outside the transformational school reacted vocally and vehemently to this attitude. In a 1966 article in *Romance Philology*, Sydney Lamb drew a vivid comparison: "while the Sectarian [transformational grammarian] looks at rival theories with the sole intent of seeking out flaws, in his zealous effort to prove that there exists only one true faith, the

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[89]Barsky, *op. cit.*, p 192.
[90]Mehta, *op. cit.*, p 91.

Open Mind approaches the work of others for the purpose of discovering contributions to future linguistic theory".[91] From as far away as the Netherlands, E.M. Uhlenbeck noted that "there is a deplorable tendency towards dogmatism on the side of those who embrace the transformational religion" – a dogmatism underscored by "a tendency to overstate their case and in some cases an [overwhelming] amount of self-confidence".[92] Where non-transformationalists took the most offense, however, was with regards to transformational polemics. I have alluded to the polemical nature of transformational discourse throughout this study, and I now turn to exploring it in detail. These polemics were an outgrowth and intensification of the attitudes discussed here, and they provide important insight into the underground culture which pervaded transformational grammar in the 1960s.

From the Greek word *polemikos* – for war – polemics imply both written and verbal attacks, as well as the art of controversial discussion. In all these manifestations, the transformational grammar discourse of the 1960s fits this mold. Transformationalists routinely labeled rival syntactic theories as trivial, unsubstantiated, and uninteresting – and any opponent who identified a weakness in transformational theory was attacked. At the forefront of these polemics were the leaders of early transformational grammar: Chomsky, Paul Postal, and Robert Lees. They quickly gained a reputation for confrontation and viciousness, earning the nicknames "Young Turks" from Charles Hockett, and the transformational "mafia" and "tightly-knit controlling elite" from Robert Hall.[93] As a group, Hockett recalls, they were "armed with a vitally important idea and with enormous arrogance, winning converts and making enemies as much through charisma as by reasonable persuasion".[94] Robin Lakoff describes the discourse in her memoirs: "I remember well the times that non-transformationalists would speak at MIT", she writes, and "[r]ather than trying to charm, conciliate, find points of connection, the circle at MIT regularly went for blood. Points were made by obvious public demolition; the question or counter example

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[91] Lamb, *Epilegomena*, p 33, capitalization in original.

[92] E.M. Uhlenbeck, "Some Further Remarks on Transformational Grammar," *Lingua* 17 (1967), p 315.

[93] Robert A. Hall Jr, *American Linguistics 1925–1969: Three essays* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1976), p 111, Hall Jr, *Review of Hockett*, p 76, note 15, Julia S. Falk, "Turn to the History of Linguistics: Noam Chomsky and Charles Hockett in the 1960s," *Historiographia Linguistica* 30/1 (2003), p 157.

[94] Falk, *Turn to the History of Linguistics*, p 157.

that brought the offender to his knees was repeated for weeks or months afterward with relish".[95] These polemics forced young linguists to choose sides quickly: they could align themselves with the transformational school and be on the producing side of the polemical discourse, or select a rival syntactic theory and place themselves in the firing range.[96]

The canonical example of transformational polemics in the oral setting is found in Chomsky's lectures to the Linguistic Institute held at Indiana University in the spring of 1964. Released in 1966 by Mouton publishers as *Topics in the Theory of Generative Grammar*, these lectures were widely attended and gained a notorious reputation within the American linguistics community. Chomsky's discourse is harsh, personal, and sweeping. Criticisms which had been raised against transformational grammar, he told his audience, were "arbitrary" and "of no importance".[97] His opponents, he continued, "completely overlook [the] obvious facts", their remarks "are based in a simple confusion", and their arguments "have no force".[98] On the personal front, he describes Fred Householder's critique of Morris Halle as having "no relevance to any issue", and he concludes from R.M.W. Dixon's treatment of grammar size that "he must be using the term 'infinite' in some new and private sense".[99] With respect to Gilbert Harman's modified constituency grammar, Chomsky states that this work "is entirely irrelevant to the whole issue": "Harman's defense of phrase structure grammar is based on the claim that he has constructed a phrase structure grammar that generates exactly the set of sentences of a certain transformational grammar", he writes, and "[t]he first part of this claim [...] is based on nothing more than terminological equivocation. The second part is false. [...] Whether [these defects] can be overcome by more elaborate mechanisms I have no idea, but the point is hardly worth pursuing".[100] Despite the existence of a large body of work critical to transformational grammar, and a large body of work proposing alternative syntactic theories, Chomsky asserts that this work

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[95] Lakoff, *The Way We Were*, p 967–968.
[96] The reader is referred to Harris, *Generative Semantics* and Harris, *Linguistics Wars* for detailed studies of rhetoric in transformational grammar.
[97] Chomsky, *Topics*, p 24ff.
[98] *Idem.*
[99] *Idem.*
[100] *Idem*, p 41–46.

"do[es] not bear on [...] substantive proposals; rather, I have suggested that [it] amount[s] to no more than a proposal to limit 'linguistics' so as to exclude the mass of 'antediluvian' traditional questions, for example, questions of competence, semantic interpretation, 'creativity', the nature of grammatical rules, etc. But no reasons have been offered for abandoning these topics, and no alternatives have been suggested that might lead to more fruitful study. Consequently, I think that these criticisms have no force".[101] This type of polemics, Hockett recalls, "was unprecedented in the scholarly experience of most of us at the time".[102] "Our custom had been to disagree with one another [...] on many matters but still to be mutually respectful and polite", he continues, but the "hostile atmosphere" of transformational grammar did little to encourage open debate with proponents of rival syntactic theories.[103]

While Chomsky's words were harsh, it was Lees and Postal who delivered the brunt of the transformational polemics. "[T]he greatest contribution Lees has made to linguistics", Arnold Zwicky and Theodore Lightner wrote in a 1970 Festschrift for Lees, "lies in the quality of his criticism: clear-headed and succinct, often provocative, usually illuminating, upon occasion devastating".[104] In discussions, in print, and at conferences, Lees crafted his polemics to infuriate proponents of rival theories, challenging their legitimacy and scienticity. "The [Descriptivist] linguist has correctly accepted the two main tasks of linguistic research as 1) to give analyses of sentences, and 2) to give criteria for these analyses", wrote Lees, but "he has traditionally and naïvely interpreted the two key notions of 'analysis' and 'criterion of analysis' in a very primitive way. By 'analysis' he usually understands 'dissection into simple additive segments', and by 'criteria' he usually means 'recipes for segmentation'. Taking such unsophisticated conceptions as these to be the basis of scientific methodology is tantamount to viewing physiology as a branch of surgery, organic chemistry as a branch of petroleum engineering. [He does not] interpret these two tasks [...] in accord with the methodology of other sciences".[105] Most famous, perhaps, is

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[101]*Idem*, p 37–38.
[102]Hockett, *Approaches to Syntax*, p 161.
[103]*Idem*.
[104]Sadock and Vanek (eds.), *op. cit.*, p xii.
[105]Lees, *Nominalizations*, p xxv.

Lees' reaction to W. Nelson Francis' work on the Brown Corpus – a response which captures Lees' dismissal of text-based linguistics: "[t]hat is a complete waste of your time and the government's money", he said, since "[y]ou are a native speaker of English; in ten minutes you can produce more illustrations of any point in English grammar than you will find in many millions of words of random text". In an open letter to the Linguistic Society of America, he wrote that transformational views "enjoy widespread popularity because they are *inspiring*. They provide a frame-work of concepts in terms of which contemporary linguists can penetrate to deeper insights. What more should one demand of a view? [...] [T]hat its more vocal proponents be polite?".[106] Lees quickly gained a reputation for being anything but polite – he had, by the early 1960s, established a "national reputation of being a firebrand linguist", ensuring that transformational theory was given a voice at conferences and that criticisms of the theory did not go unanswered.[107] Much later, he himself described his tactics as "getting up at meetings and calling people *stupid*".[108]

"The polemical zeal of Chomsky, Halle, and Lees and their penchant for dismissing objections as 'uninteresting' were considerable", wrote sociologist Stephen Murray, "but Paul Postal's were greater still".[109] Spread over the oral and written fora, Postal's polemics were underwritten by his adeptness at debate and at conjuring counterexamples: at conferences, he would offer biting and detailed criticism immediately after hearing a paper for the first time. His performances became known for – and, by critics of transformational grammar, feared for – their intellectual richness, flamboyancy, and critical authority. "You can take [the Descriptivist] position that we are not interested in explaining anything, in which case, of course, there is nothing to talk about", Postal said at the 1962 Georgetown Round Table Meeting on Linguistics and Language Study: "[o]ne cannot argue with someone who wishes only to classify utterances. People have a right to do what they want. We can ask, however, whether this has the right to be called 'linguistics'; whether it has the right to

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[106]Maher, *op. cit.*, p 6, emphasis in original.
[107]Sadock and Vanek (eds.), *op. cit.*, p vii.
[108]Harris, *Linguistics Wars*, p 72, Robert Lees in personal communication with Randy Allen Harris, emphasis in original.
[109]Murray, *Theory Groups*, p 240.

claim to be a significant field of inquiry. I say 'no' because there is an infinite number of ways of classifying utterances and I see no rational basis for choosing between them [...]. Furthermore, it seems to be quite generally accepted that a science is supposed to explain something, that it is supposed to provide insight into the subject matter it deals with. How a classification alone provides this insight is not clear [since it is] simply imposed by the linguist on the data".[110] Conflating the constituency theory of the 1960s with an older philosophical position, Postal continued to assert that constituency grammars "are simply sets of utterance classifications [with] the explicitness of a library catalog", that they provide "no [...] explanatory insight", that they "fail to offer any rational justification for particular descriptions" and "yield[] results of little obvious value".[111] Postal often "begs the question by assuming that only a transformationalist approach is the right one", wrote F.R. Palmer in 1965, and he "refuses to look with any sympathy at his opponents' point of view".[112] It is, he continues, as if Postal "is almost deliberately trying not to understand" what his opponents are saying.[113] "There was a kind of holy war aspect" to these polemics, Arnold Zwicky recalls from his days as a graduate student at MIT in the 1960s: "a feeling that some people had that they had to turn people's minds around, and that it was *important*, and any device that did this, including ridicule, was legitimate".[114]

As a whole, the transformational polemics weighed heavily at conferences, taking center stage at, among others, the Third Texas Conference (Austin, 1958), the 1964 Linguistic Institute (Indiana University), and the Ninth International Congress of Linguists (Cambridge, Mass., 1962). "[A]nyone who presented a paper had to be prepared for an attack on some issue, either central to the topic or quite marginal", Gleason recalls: "no non-TG paper could be left unchallenged, and no attack on a TG paper could be left unanswered".[115] In the midst of this "intellectual bullying", key tenets of transformational theory doubled as polemical

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[110]Hamp et al., *Transformational Theory (Panel I)*, p 10–11.
[111]*Idem*, p 3–8.
[112]Palmer, *Review of Postal*, p 352.
[113]*Idem*.
[114]Harris, *Linguistics Wars*, p 173, Arnold Zwicky in personal communication with Randy Allen Harris, emphasis in original.
[115]Gleason Jr, *Theories in Conflict*, p 72–73.

weapons: the competence-performance distinction was invoked to reject counterexamples by labeling them as performance data; the separation of deep and surface structure was used to argue for the inadequacy of single-level explanations; and the TG dataset was called upon to demonstrate the explanatory weaknesses of rival theories.[116] Transformational grammarians were accused of imposing false debates on linguistics, of "get[ting] away with rhetorical tricks in place of logical argument", and of treating non-transformational work as second-class.[117] In the colorful and accurate words of educational linguist Matthew Bronson, it was an era of "testosterone linguistics" – an era in which theories battled publicly, and in which style and rhetoric could be as important as content.[118]

The public face of transformational grammar in the 1960s was that of a united front and a single mind. Transformational theory was constantly defended publicly, and rivals were not conceded any ground. "It was seen as very important not to let the bad guys see your weaknesses", remembers Robin Lakoff: "rather than be honest, acknowledge that TG had its flaws, things they couldn't do, the requirement of adversarial discourse was that one present a pose of perfect poise and complete certainty".[119] This attitude was self-perpetuating, and as the 1960s progressed polemics grew in quantity and in force. The public perception of confidence was reinforced by an internal confidence within the transformational community: transformationalists firmly believed that their theory was making rapid progress, and that transformational solutions would soon be found to all syntactic problems. The belief that no other theory came close to matching TG was all-pervasive, reaching even into the classroom. At the MIT linguistics department, for example, the course on non-transformational linguistics was popularly known as "the Bad Guys".[120] The single public face of transformational grammar did not crack until the late 1960s, when the theory split over the Semantics Wars. Through that decade, the transformational mentality attracted and retained students, bringing them in, as it were, to a tight knit family – a family

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[116]Yngve, *Early Research*, p 68.
[117]Geoffrey Sampson, "Review of E. F. K. Koerner's The Transformational-Generative Paradigm and Modern Linguistic Theory," *Language* 52/4 (1976), p 961.
[118]Falk, *Turn to the History of Linguistics*, p 156, note 54.
[119]Lakoff, *The Way We Were*, p 968.
[120]Koerner, *The 'Chomskyan Revolution'*, p 173, note 18.

which offered unwavering support in return for theoretical commitment.

To proponents of rival theories, the transformational polemic was, at best, irksome and predictable and, at worst, alienating and destructive. Robert Hall characterized the division between the two groups in doctrinal terms: "[h]e who does not accept [TG] is an infidel", he wrote – and "[h]e who does accept [TG] becomes a follower of the Chomskyan religion".[121] In an interview with Ved Mehta, Charles Hockett described American linguistics as being "aswarm with locusts – terrorized by transformationalists" – and without sound leadership.[122] The intensity of the transformational polemic, he continued, meant that linguistics students learned nothing outside that theory and, consequently, had no "basis for comparison".[123] In a 1997 autobiographical piece, Hockett identified the attitude of young transformationalists in the 1960s with the attitude of "party-line communists toward sympathizers with independent ideas": non-establishment ideas were not tolerated, and non-establishment figures villified.[124] In the late 1960s, the relationship between MIT and Yale – where the stratificational community was centered – was consumed by this polemic. Stratificational theory and its supporters were "the subject of abuse and ridicule at MIT", and no reasonable or rational dialogue between the two linguistics groups could take place.[125] "[B]ack in the sixties after I pointed out some mistakes in Chomsky's thinking", Lamb recalls, "I became a bad guy. Students of that period were discouraged from paying attention to my work [and] I was no longer receiving invitations to give lectures".[126] The transformational polemics went a long way towards making scholarly dialogue with rival theories impossible.

While younger supporters of rival theories continued to challenge transformational grammar through the 1960s, older linguists – especially those who had been trained in the constituency paradigm of the 1940s and 1950s – tired of the polemics and withdrew

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[121]Robert A. Hall Jr (ed.), "Review of Frederick J. Newmeyer's Linguistic Theory in America," in *Linguistics and Pseudo-linguistics: Selected essays, 1965–1985* (Amsterdam: John Benjamins Publishing Company, 1987), p 105.

[122]Mehta, *op. cit.*, p 218.

[123]*Idem.*

[124]Hockett, *Approaches to Syntax*, p 161.

[125]Makkai, *Idiom Structure*, p 60.

[126]Lamb, *Different Drummer*, p 100–101.

from potential confrontations. "We do not enjoy being told that we are fools", wrote Hockett in 1965 – and, a few years later, he stepped away from the theory debates: "I've done my bit for traditional linguistics", he said, and "I've got better things to do than spend the rest of my life fighting a rearguard action against the Philistines".[127] He was joined by, among others, Gleason and Archibald Hill. By the late 1960s, Gleason had grown "tired of the fight", and tired of working in a discipline in which public exposure "invariably meant attack".[128] For similar reasons, Hill chose to "keep out of the current disputes".[129] After a decade of polemics, transformational grammarians had managed to push aside critics and divide the American linguistics community.

The transformational attitude and polemics fostered and encouraged an underground culture. Outright rejection of past theories and current rivals provided little impetus to communicate research publicly, and ample motivation to restrict the circulation of knowledge. The 'us against the world' attitude rewarded group solidarity and commitment, and encouraged students to attach themselves to the underground literature scene. Together, the rejection of what had come before, Chomsky's personal influence, and the polemics of early transformational grammarians built private knowledge into a highly-valued commodity within the transformational community.

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The third impetus for the emergence of an underground culture in transformational grammar concerns the specific job market demands of the 1960s. In this decade, academic linguistics jobs were predicated not on publication in mainstream journals, but obtained through a combination of reputation, connections, and (usually unpublished) results. There was no requirement, no impetus to publish in journals – and no penalty for not doing so. For students of transformational grammar, underground literature, far from being an impediment to an academic job, in fact improved their chances of obtaining their job of choice.

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[127]Mehta, *op. cit.*, p 226, Hockett, *Sound Change*, p 187.
[128]Gleason Jr, *Theories in Conflict*, p 76.
[129]Hill, *Revolutions*, p 75.

As the number of linguistics departments at universities across America ballooned in the 1960s, there was a great need for faculty members. Impressed by the confidence, optimism, and early success of transformational grammar, many universities were intent on filling their newly-founded linguistics departments with transformationalists – a task made possible by the pace and volume of the MIT linguistics graduate program. In the early 1960s, MIT was the front runner in the intake and production of graduate students specializing in syntax. Riding on MIT's reputation and the cachet of transformational theory, graduates from MIT found jobs for the taking in Texas, Wisconsin, Ohio, Illinois, California, and elsewhere.[130] The pace of hiring in the 1960s meant that it was common for students to be offered jobs even before their dissertations were completed. McCawley, for example, took a job at Chicago in 1964 before completing his doctorate the following year, and Stanley Peters left MIT for a job in Austin, never completing his degree.[131] There were more jobs available than students to fill them – a market pressure which lent itself to the underground culture of transformational grammar.

Students and recent graduates were hired not on the basis of published papers, but of unpublished results, thesis drafts, and pedigree.[132] In the transformational community, reputations were built by contributions to the body of underground literature, and reputations played a significant role in securing a job. Not only did mainstream-published papers provide no advantage in the hiring process; in fact, they could be detrimental to a student's ability to obtain a job: the slow pace at which mainstream linguistics journals operated in the 1960s meant that students could have to wait up to a year, and in some cases even longer, for their papers to be published. Circulating their work on the underground scene – especially easy for graduate students at MIT – gave students a method of quickly distributing their results and, consequently, making a name for themselves in the transformational community. The underground culture of transformational grammar allowed students to avoid the delays common with traditional journals, bolster their reputation as researchers,

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[130] This point is also made in Newmeyer, *Linguistic Theory in America* and Newmeyer, *The Politics of Linguistics*.
[131] Bach, *Interview*.
[132] *Idem.*

and prove their establishment credentials. The job market demands of the 1960s rewarded underground publications as much as, and often more than, aboveground publications, providing a vital impetus for the perpetuation of underground culture through the decade.

The idea of private knowledge within a modern academic setting is an immediate source of curiosity and intrigue. For the transformational grammarians of the 1960s, however, private knowledge was both highly valued and highly rewarded. I have shown that this underground culture can be explained by three features of the era: the rapid pace at which transformational theory evolved in the 1960s, together with the insufficiency of mainstream journals; the intellectual atmosphere at MIT and the polemics encouraged by the transformational community; and job market demands. The underground culture which existed in transformational grammar between 1957 and 1968 was shaped by a combination of intellectual, socio-professional, and economic forces – a combination which defined both a generation of linguists and their theory.

5.3 Resolving the Paradox

The apparent paradox this chapter aims to resolve results from the rapid rise of transformational grammar in face of that theory's underground culture. Between 1957 and 1968, transformational grammar was not transmitted primarily through mainstream mechanisms: rather, transformationalists shunned traditional journals and developed a private network for the circulation of research results. Outsiders had little access to this underground literature. Nonetheless, transformational theory soon dominated academic linguistics in America. Two rival syntactic theories – constituency grammar and stratificational grammar – were reduced to secondary players in theoretical linguistics, used only by a minority of linguists. This section aims to understand how transformational theory was able to achieve a dominant position on a nationwide scale while also placing high value on underground culture and private knowledge. I argue that three factors are necessary to fully explain the rise of transformational grammar in the 1960s: discipline dispersion and informal networks, pedagogical appeal, and oral transmission. These are discussed in turn.

Despite the prevalance of an underground culture, transformational research dispersed across the United States following lines of discipline dispersion. As MIT-trained linguists took up positions at new university linguistics departments through the 1960s, they brought their theory and their MIT connections with them. The dispersion of people brought about a parallel dispersion of knowledge: young transformationalists, enthusiastic about their work, often set up informal networks at their new institutions for the circulation of underground work, providing faculty and students with access to this work. In the face of the pervasive underground nature of early transformational theory, the dispersion of people was vital to the dispersion of the theory itself.

By 1964, the MIT linguistics group was turning out graduate students – students trained in transformational theory and carrying with them the prestige of one of America's premier academic institutions. They were quickly hired by universities across the country, and they installed a transformational agenda at their new academic homes. "In the unprecedented expansion of the American university system during the economic boom of the middle and late 1960s [...] [j]obs were for the taking in the new departments then being organized at state universities in Illinois, California, Texas, Ohio, Massachusetts, Washington, and elsewhere", wrote Frederick Newmeyer in his 1986 *The Politics of Linguistics*: "practically every early generativist Ph.D. recipient obtained a position at a major university".[133] At UCLA, for example, Robert Stockwell – a proponent of transformational grammar from 1958 on – worked through the early 1960s to bring a "full-scale linguistics program" to the Los Angeles school, immediately hiring MIT graduate Barbara Hall Partee upon the completion of her degree in 1965.[134] Konrad Koerner – not one to mince words – described Stockwell's work in the early 1960s as "building a fledgling linguistics program into a strong TGG [transformational generative grammar] department with a clearly generativist agenda".[135]

In the 1960s, transformational theory was brought to the University of California at San Diego by Edward Klima, to the University of Chicago by James McCawley, to the Univer-

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[133]Newmeyer, *The Politics of Linguistics*, p 83.
[134]Stockwell, *To Linguistics and Back Again*, p 238.
[135]Koerner, *The 'Chomskyan Revolution'*, p 195.

sity of Illinois by Robert Lees, to Indiana University by Andreas Koutsoudas, to the University of Massachusetts by Robert Binnick, to Ohio State University by Arnold Zwicky, to the University of Texas at Austin by Stanley Peters, to the University of Washington by Frederick Newmeyer, and, of course, to UCLA by Robert Stockwell. Many of these young transformationalist-oriented programs quickly built productive graduate programs, training large numbers of students in transformational theory. Six years after Robert Lees had founded a linguistics department at the University of Illinois, for example, that department trained "over three dozen young, enthusiastic linguists" – linguists versed in transformational grammar and committed to transformational theory.[136]

The early dominance of transformational theory in linguistics departments across the country was due to, firstly, the size, productiveness, and head-start of the MIT linguistics group, which supplied the vast majority of young syntacticians in the 1960s; and, secondly, the reputation of that group, its intellectual leadership, and transformational theory itself. The perceived monopoly of transformational grammar on linguistics departments across the country, however, enraged supporters of rival syntactic theories and continued to be a point of contention for anti-transformationalists in the following decades. Stratificational grammarian Adam Makkai asserted in 1975 that transformational theory had led to "[i]ncredible and unprecedented inequality in hiring and publishing".[137] For Makkai and others outside of the transformational community, the dominance of MIT and MIT-trained linguists had led to a "rigid and impenetrable" academic system – one which was "alien in spirit to democracy and academic freedom".[138] Once a transformational grammarian was installed at a university, critics continued, they would more often than not actively work to prevent the hiring of linguists with other theoretical commitments. Robert Lees, for example, is accused of having "refused a job at Urbana to at least one who failed the test of TG faith".[139]

While transformational grammarians did, indeed, as these critics imply, dominate a sig-

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[136]Sadock and Vanek (eds.), *op. cit.*, p vii.
[137]Makkai, *Stratificational Solutions*, p 41.
[138]*Idem.*
[139]Maher, *op. cit.*, p 6, Bach 2008.

nificant number of linguistics departments in America, the transformational paradigm did not reign supreme at all universities in the 1960s. At Berkeley and Yale – strongholds of stratificational theory – transformational grammar took a back seat. Even after Sydney Lamb left Berkeley for Yale in 1964, Wallace Chafe provided an anti-TG personality at Berkeley. At Cornell, the linguistics group boasted two of the leading critics of transformational theory of the 1960s: Robert Hall and Charles Hockett. Other universities which were not dominated by transformational grammar in the 1960s include the University of Michigan (Kenneth Pike), the University of Florida (John Algeo), Georgetown University (Walter Cook, Paul Garvin and Robert Di Pietro, but also Francis Dinneen), Colombia (Robert Austerlitz and William Labov), and Canada's University of Toronto (H.A. Gleason and Peter Reich).

Importantly, as discussed earlier, the transformational grammarians who dispersed from MIT to universities across the country brought with them their connections to their home institution and the underground literature scene. James McCawley, Stanley Peters, Robert Stockwell, and others established informal distribution networks at their new academic homes, providing students and faculty with access to underground documents. In this way, underground literature escaped from the transformational inner circle and circulated among larger groups of linguists. The spread of linguists trained in transformational theory from MIT to all corners of the country was, simultaneously, a spread of people, their ideas, and their literature.

Informal distribution networks at universities with young linguistics programs were essential to bringing linguistics students into the transformational fold and, consequently, to the maintenance of transformational theory. With access to this literature, students were able to stay abreast of the latest developments in transformational research and follow the evolution of that theory through their undergraduate and graduate studies. By reading underground literature, students – regardless of their location in the country – were introduced to the polemic style and private world of transformational grammar – a world which was attractive and appealing for its cachet and aura as much as for the theory itself.

If the first generation of transformationalists was recruited and trained at MIT, the subsequent generations were recruited and trained at universities all across America. Access to the transformational underground literature was imperative to capturing these subsequent generations and to securing the perpetuation of transformational theory in America. Regardless of their flamboyance, success, and confidence, the first group of transformationalists – Lees, Postal, McCawley, and their colleagues – could not themselves guarantee the continuation of their theory: such continuation required the buy-in of students who would commit themselves to transformational research and to teaching the theory to subsequent generations of linguists. By providing students at universities across the country with access to underground literature through the 1960s, the transformational community took a necessary step towards establishing the dominance of their theory in American academic linguistics. At universities across the country, transformational grammarians successfully built "self-maintaining departments" – departments which secured the commitment of graduate students to transformational theory by giving them access to underground documents, and hence which secured continued teaching and research in that theory.[140]

It is necessary to draw a distinction between students at transformational-oriented universities who were offered access to underground literature, and linguists committed to rival syntactic theories (usually working at non-transformational-oriented universities) who had little or no access to this literature. As discussed earlier, proponents of rival theories were excluded from the underground transformational culture: they had great difficulty obtaining underground documents and were often unaware of what was circulating on the underground network at any given moment. The distinction between these two groups largely follows lines of departmental influence. At pro-transformational theory institutions such as Illinois and UCLA, students could easily access underground literature – but at non-establishment universities such as Yale and Berkeley, underground transformational literature was not available. The underground culture was hence selectively private: it was

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[140]Gleason Jr, *Theories in Conflict*, p 72.

open to students at universities with a transformational bent, but not to established rivals and universities dominated by other syntactic theories. In this way, the transformational community ensured that a new generation of American linguists would be immersed in transformational theory, while rivals would be denied entrance into that theory's culture. The dispersion of transformational grammarians to teaching positions at new linguistics departments across America in the 1960s thus played a dual role in the rise of transformational theory: first, it spread the theory far beyond MIT, where it had been incubated and developed and, second, it provided a mechanism to allow selective access to underground transformational literature. By and large, students given access to this literature themselves became committed to transformational theory, and were instrumental in guaranteeing the continuation of this theory in America.

<p style="text-align:center">* * *</p>

A second, and equally important, mechanism for the distribution of transformational ideas in the 1960s was through linguistics textbooks. Even as transformational grammarians valued private knowledge, the publication of aboveground transformational grammar textbooks through the middle of the decade (described in detail in Chapter 4) enabled the basics of the theory to disperse relatively freely. Published by mainstream companies – Holt, Rinehart and Winston, and McGraw-Hill – textbooks including Emmon Bach's *An Introduction to Transformational Grammars* (1964) and Andreas Koutsoudas' *Writing Transformational Grammars: An introduction* (1966) were common, public, and widely available. They promoted transformational grammar in the classroom and, with their wealth of exercises and practice problems, guided students to develop a transformational worldview. A generation of students was trained to use transformations as a fundamental tool for envisioning and manipulating language. In a decade when no university linguistics program could afford to ignore syntax, the theory which dominated the textbook market had a clear advantage. In this respect, transformational grammar succeeded where stratificational grammar failed: while the 1960s saw the publication of a handful of transformational theory textbooks, there was no stratificational grammar textbook until 1972. As a result,

many more students were taught transformational grammar as opposed to stratificational grammar through the 1960s.

Most importantly, however, the availability of transformational grammar textbooks meant that learning about transformational theory required no access to underground literature: the basics of this theory could be learned from mainstream textbooks. From 1964 on, textbooks played a vital role as a mechanism of knowledge dispersion in what was otherwise an underground culture. They taught students how to use transformations and the motivations behind the development of transformational theory, building in them a capacity for handling the transformation-as-tool and an appreciation of the inadequacies of constituency grammar. Textbooks, of course, did not include up-to-date research in transformational theory – research which, in the 1960s, was changing so rapidly so as to be incompatible with mainstream journal publications – but did, crucially, provide an entrance mechanism to the theory itself. Widely distributed and widely available, these textbooks provided an aboveground alternative to the culture of private knowledge and allowed open access to the basics of transformational theory regardless of connections and institutions. Acting as a counterweight, they ensured the dispersion of transformational grammar in face of a largely underground culture.

Finally, oral dispersion – including conferences, colloquia, and the Linguistic Society of America's Linguistic Institutes – also played a central role in the spread of transformational grammar throughout America. The 1960s saw an accelerating pace of linguistics meetings hosted by a variety of groups from university linguistics clubs to new linguistics societies to the American Mathematical Society. While their subject matters, intentions, and locations varied, these meetings had in common the bringing together of large numbers of linguists and linguistics students – and, consequently, the exchange and transmission of linguistics knowledge. For transformational grammarians, meetings provided an opportunity to step away from their culture of private knowledge and share selected portions of their theory publicly.

The use of the oral arena by transformationalists can be divided into three parts. First,

in the late 1950s and early 1960s – before transformational grammar had secured a dominant place in American academic linguistics – transformationalists regularly used meetings and conferences to criticize rival theories and argue for the superiority of transformational grammar. As described in the earlier discussion of polemics, transformational grammarians were adept at confronting rival theories – especially constituency grammar – from both the podium and the floor. At events from the 1958 Texas Conference to the 1964 Linguistic Institute, transformationalists actively promoted their theory as providing a necessary replacement for the flawed constituency theory of the 1950s. Second, later in the 1960s, transformational grammarians used meetings – especially Linguistic Institutes – to develop new ideas. "Ideas were worked out on the blackboard and theories modified in response to far-from-passive audiences", recalls Regina Darnell, and conferences were interactive and cutting-edge.[141] This use of the oral arena provided outsiders with glimpses into transformational research, which was otherwise restricted by the underground culture. Finally, Chomsky frequently used meetings and conferences to make sweeping pronouncements about theory change. The main ideas of *Aspects of the Theory of Syntax*, for example, were released in Chomsky's 1964 lecture to the Indiana University Linguistic Institute, a year before the manuscript was published. Similarly, his public lectures at Berkeley in January 1967 established his thinking about language acquisition and universal grammar, published the following year as *Language and Mind*.[142] Through the 1960s, Chomsky traveled widely, including a sabbatical year at Berkeley in 1966–1967, and spoke at institutions across the country. It was the second of these uses which was the most important in terms of the underground culture: by presenting transformational research at meetings and conferences, proponents of the theory were able to expose students to new research which was difficult to access through journals and textbooks.

With their roving location and emphasis on current research, the Linguistic Society of America Linguistic Institutes were particularly important for the transmission of transformational research. Through the 1960s, these institutes were hosted by the University of

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[141] Darnell, *op. cit.*, p 112.

[142] Noam Chomsky, *Language and Mind* (New York: Harcourt Brace Jovanovich, Inc., 1968).

Texas at Austin (1960–1961), the University of Washington (1962–1963), Indiana University (1964), the University of Michigan (1965, 1967), the University of California at Los Angeles (1966), and the University of Illinois at Urbana-Champaign (1968–1969). This geographic diversity meant that linguists and linguistics students from all over the country were able to attend at least some of the institutes. Further, since attendance at the institutes did not depend on theory affiliation, linguists from all schools attended lectures and courses on transformational theory. The Linguistic Institutes were vibrant and productive through the decade, and gave students a taste of transformational grammar in action. While students could not access up-to-date transformational research from textbooks, they could access some of this research at Linguistic Institutes. As such, oral dispersion was also vital to transmitting what was otherwise a theory immersed in an underground culture.

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Together, these three arguments – discipline dispersion and informal networks, the availability of mainstream textbooks, and oral transmission – explain how and why transformational grammar dispersed across America despite its pervasive underground culture. The paradox seemingly caused by the rise of a theory which placed high value on the private transmission of knowledge among a select group of insiders can be understood by considering the mechanisms through which this knowledge escaped the underground scene: the spread of people committed to the theory over a wide geographical range in the form of MIT-trained transformational grammarians taking positions at universities across America; the aboveground publication and distribution of transformational theory textbooks; and the person-to-person transmission of research at conferences and Linguistic Institutes. Given the pervasiveness of underground culture in the transformational community, and the importance of this culture to group cohesion and identification, the rise of transformational grammar cannot be explained without a consideration of these factors.

One key thread which runs through this investigation of the spread of transformational theory is the tension between private and public knowledge. On one hand, transformational grammarians valued the private transmission of knowledge through their under-

ground circulation networks and, on the other hand, they deliberately shared this theory with wide audiences by means of informal distribution networks at universities, by publishing mainstream textbooks, and by speaking at conferences. These attitudes and actions were not contradictory; rather, each played a specific role in transformational culture. The underground literature scene was highly valued for the speed at which it circulated results and for the sense of group cohesion, exclusivity, and prestige it provided. At the same time, the public presentation and dispersion of transformational theory – whether it be through informal networks, through textbooks, or in the oral arena – enabled transformational grammarians to defend and promote their theory, criticize rival approaches to syntax, and ensure that linguistics students would be cognizant of and trained in the transformational paradigm. The tension at play was that of balancing the desire for an underground culture with the need to ensure the perpetuation of the theory: even as transformationalists worked to restrict access to their underground documents, they were well aware that limited public exposure of the theory was necessary to ensure that the next generation of linguists would be committed to the transformational paradigm. As such, they deliberately opened the theory to the group most vital to theory perpetuation – linguistics students – while simultaneously restricting access to those committed to rival syntactic theories. The public face of transformational grammar in the 1960s was, as I have shown, designed to maximize exposure of the theory to students. The success of transformational grammar in the 1960s needs to be understood as the result of a carefully-balanced approach to private and public knowledge, designed to foster a community of transformationalists, a sense of identification and commitment, and to spread the theory across the country.

5.4 The Aftermath

Transformational grammar's underground culture thrived until 1968. In the following years, the nature of knowledge transmission in American linguistics drastically changed with the establishment of several new journals, which ensured rapid and accessible publication. Even as transformational grammarians began to shift their work from the private

sphere to public journals, some of the underground literature of the 1960s was published in anthologies, Festschrift, and individual monographs. Underground literature, however, did not disappear completely from American linguistics: through the late 1960s and early 1970s, the generative semantics movement – one of the two branches of transformational grammar which split off at the end of the decade – maintained and intensified a culture of private knowledge. This section discusses the first of these developments; the second is outside the scope of this thesis.[143]

By the mid to late 1960s, many in the American linguistics profession recognized that their discipline was suffering at the hands of an inadequate supply of mainstream journals. The number of article spots in the existing journals was far exceeded by the number of linguists interested in publishing; funding problems and publication lags meant that issues were often released months behind schedule; journal publication was so unreliable as to not be considered useful for securing academic jobs; and, within the transformational school, there was little incentive to use traditional publication outlets. "Not only are the problems of information flow reaching epidemic proportions", wrote Georgetown University's Robert Di Pietro, "but also the lag between writing and publication in linguistics worsens".[144] Publication lags of up to two years for mainstream journals were, Di Pietro emphasized, unacceptable in a discipline where "new ideas and insights are literally born overnight".[145] Between 1967 and 1975, seven new linguistics journals were founded in America, more than doubling the previous capacity. These journals explicitly aimed to "overcome the dearth of sufficient vehicles for publishing articles and reports" which had plagued the discipline for a decade – and, indeed, they were successful in ending the trans-

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[143]The generative semantics movement is one of the two branches of transformational grammar which split off in the late 1960s. The dispute between the two groups – known as the generative semanticists and interpretive semanticists – revolved around the relationship between semantics (the study of meaning) and syntax. During the approximately five-year duration of the Semantics Wars, as this period is known, underground literature played a significant role. The underground literature of the Semantics Wars has been widely discussed in the literature by both linguists and historians. The interested reader is directed to Harris, *Linguistics Wars*, Harris, *Generative Semantics*, Huck and Goldsmith, *Ideology and Linguistic Theory*, Huck and Goldsmith, *On Comparing Linguistic Theories*, Arnold M. Zwicky et al. (eds.), *Studies out in Left Field: Defamatory essays presented to James D. McCawley on the occasion of his 33rd or 34th birthday*, 2nd edition (Philadelphia: John Benjamins Publishing Company, 1992), and Matthews, *Grammatical Theory*.

[144]Di Pietro, *New Vistas*, p 16–17.

[145]*Idem.*

formational underground culture.[146]

The first of these new journals – *Glossa* and *Language Sciences* – were established, respectively, in 1967 and 1968. Designed for "rapid publication of relevant new articles, which formerly would have entered the underground literature", they immediately affected the distribution of knowledge among American linguists.[147] Published out of Indiana University, *Language Sciences* was considered a well-balanced journal, displaying little editorial bias towards any syntactic theory.[148] The same, however, cannot be said about *Papers in Linguistics* (founded in 1969), *Linguistic Inquiry* (1970), and *Linguistic Analysis* (1975), all of which clearly leaned towards transformational grammar. *Papers in Linguistics* was established by Anthony Vanek and published out of the transformational-oriented linguistics department at the University of Illinois. Trained in Slavic linguistics and transformational grammar, Vanek contributed greatly to linguistics publication in the 1970s and 1980s, responsible in part for, among others, *Papers in Linguistics*, *The International Review of Slavic Linguistics*, and the *Current Inquiry Into Language and Linguistics* monograph series. *Papers in Linguistics* in particular provided a key outlet for the quick publication of transformational articles.

The most contentious of these new journals was *Linguistic Inquiry*, which soon became "the unofficial organ of transformational generative grammar" – a reputation it has maintained for decades.[149] Published by MIT Press and edited by Samuel Jay Keyser, who worked at Brandeis and the University of Massachusetts before becoming the chairman of the MIT linguistics department, *Linguistic Inquiry* was explicitly designed to address the shortcomings of 1960s linguistics journals. "[N]ew theories are being advanced at an astonishing rate", asserted the journal's founding statement of purpose – and, consequently, the new journal "will devote a significant portion of its pages to a section entitled 'Squibs and Discussion' where short arguments are developed, where paradoxes and baffling facts with consequences for theory are brought to light, where reaction to issues raised in this journal

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[146]*Idem*, p 15.
[147]Fidelholtz, *op. cit.*, p 932.
[148]Bolinger, *First Person*, p 23.
[149]Newmeyer, *Linguistic Theory in America*, p 206.

and elsewhere will appear and where students of language and related fields will deposit and look for interesting areas of research".[150] This mission statement, which accurately reflected the nature of the journal, clearly shows the intentions of the journal as a replacement for the underground scene. Further, *Linguistic Inquiry* declared that it would "make every effort to match in speed of publication the speed with which new ideas are being formulated".[151] The journal provided an important alternative to private knowledge: the first volume, published in January 1970, included 19 articles and squibs on transformational theory, and by the end of the year *Linguistic Inquiry* had increased the number of journal spots for articles in the transformational paradigm by an order of magnitude.

While *Linguistic Inquiry* provided an outlet for transformational grammarians, it was viewed warily and critically by proponents of rival syntactic theories. "No journal but LI [Linguistic Inquiry] sparks such controversy", wrote Geoffrey Pullum in a 1991 chronicle of the journal's conduct: it exists, he continues, "solely to publicize MIT-specific, Chomskyan thinking", and is frequently "accused by furious authors of being biased".[152] Even Frederick Newmeyer concedes that non-transformationalists are rarely published in the journal, and that there has been much "grumbling that papers critical of Chomsky's work are judged excessively severely".[153] As the number of mainstream journals serving the American linguistics profession ballooned in the late 1960s and early 1970s, non-transformationalists grew frustrated with the bias many of these new journals showed towards transformational grammar. "There are people", Pullum wrote later, "who carry their LI [Linguistic Inquiry] refereeing horror stories around with them like albatrosses and will grasp you by the arm like the ancient mariner and force you to hear them out".[154] This discontent resulted in the formation of the Linguistic Association of Canada and the United States (LACUS) in 1974. The journal of this association, *Forum Linguisticum*, provided

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[150] Linguistic Inquiry, Vol. 1, no. 1, 1970.
[151] Linguistic Inquiry, Vol. 1, no. 1, 1970.
[152] Geoffrey K. Pullum, "The Conduct of Linguistic Inquiry," in Geoffrey K. Pullum (ed.), *The Great Eskimo Vocabulary Hoax and Other Irreverent Essays on the Study of Language* (Chicago: University of Chicago Press, 1991), p 30–31.
[153] Newmeyer, *Linguistic Theory in America*, p 206.
[154] Pullum, *op. cit.*, p 33.

an outlet for non-transformational work. While it was officially intended to "deal with all areas of linguistics", to provide a forum for the "free exchange of ideas", and to favor no theory "at the expense of others", *Forum Linguisticum* in practice favored stratificational grammar, and was the main publication outlet for SG through the 1970s.[155]

By the early 1970s, the American linguistics profession was thus served by over a dozen journals, approximately half of which were oriented towards transformational grammar, one of which favored stratificational grammar, and the remainder of which displayed no consistent theoretical bias. This explosion of new journals corrected the imbalance between the number of linguists desiring to publish and the number of article spots in journals and, as a result, underground transformational literature began to wane. By the mid-1970s, publication and circulation in American linguistics had normalized. "There are now too many outlets for publishing important papers, even preliminary ones, and people interested in such articles are spread too widely", wrote James Fidelholtz, "for the existence of such 'underground' literature to be defensible".[156] The disappearance of underground culture in transformational grammar (with the exception of generative semantics) was due to the establishment of a normalized publication system large enough to serve the American linguistics community and not plagued by publication lags, combined with the growth of transformational groups at universities across the country and the decentralization of MIT's influence.

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In the 1970s and 1980s, transformational grammarians looked back on their decade of underground culture with a mixture of pride and bemusement. As their discipline normalized its mechanisms for knowledge communication and transmission, transformationalists sought to bring some of the 1960s underground literature into the public eye. The following two decades saw the publication of a pair of anthologies of underground work, as well as the release of individual monographs of previously unpublished work.

The chief anthology of underground literature – *Notes from the Linguistic Underground* –

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[155]*Forum Linguisticum*, Vol 1, no. 1, 1975.
[156]Fidelholtz, *op. cit.*, p 932.

was published in 1976 as part of the *Syntax and Semantics* series, and edited by James Mc-Cawley.[157] A collection of papers which had circulated on the underground networks of transformational grammar and generative semantics, *Notes from the Linguistic Underground* is an essential source of previously private documents. McCawley's lengthy commentaries about each paper, as well as his introductory discussion of the underground culture in American linguistics, provide some of the only direct insights into the phenomenon. Containing, among other documents, Robert Lees' *What are Transformations?*, Paul Postal's *Linguistic Anarchy Notes*, and *Camelot 1968* (released under pseudonyms by Robert Binnick, Jerry Morgan, and Georgia Green), this anthology includes the 'classics' of underground literature – papers which stimulated discussion and research, and which influenced the development of transformational grammar, even as private knowledge. When he went to assemble papers for the anthology, McCawley recalls, he found a body of underground work "so large that it was easy for me to assemble a volume's worth of high-quality material without having to look very far" – a testament to the quantity of work which first circulated on the transformational grammar and generative semantics underground networks.[158] For many, this volume marked the end of an era in American linguistics: what had been private was now public, available for all to read. The anthology, Jerrold Sadock wrote, "can be looked upon as a sort of certificate of majority for the field".[159] The period of "unpublished mimeos, Xeroxes, and dittos" was put to rest, he continued, and the study of syntax had finally "come of age".[160] Fidelholtz concurred in his 1978 *Language* review of the anthology, writing that it "marks a decrease in elitism in the field, i.e. a more ready accessibility of important articles".[161]

A second anthology of underground literature deserves mentioning since it has gained notoriety outside of linguistics, but it is devoted to the literature of the generative semantics movement and hence is beyond the scope of this study. *Studies Out In Left Field: Defamatory*

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[157] McCawley (ed.), *Notes from the Linguistic Underground*.
[158] McCawley, *Linguistic Underground*, p 15.
[159] Sadock in McCawley (ed.), *Notes from the Linguistic Underground*, p xi.
[160] Sadock in McCawley (ed.), *Notes from the Linguistic Underground*, p xi.
[161] Fidelholtz, *op. cit.*, p 932.

essays presented to James D. McCawley on the occasion of his 33rd or 34th birthday was originally released in 1971, and reprinted in 1992. This Festschrift celebrates the epitome of the generative semantics movement: a witty, fast-thinking, and exuberant approach to linguistics; an approach which made as much room for commentary on sex, drugs, and politics (and especially opposition to the Vietnam war) as it did for linguistics work. Edited by Arnold Zwicky, Peter Salus, Robert Binnick, and Anthony Vanek, the anthology included underground generative semantics papers ranging from *English Sentences Without Overt Grammatical Subject* (Quang Phuc Dong, a pseudonym for James McCawley) to *Conjunctive Ordering* (E. Clifton Gamahuche, a pseudonym for John Robert Ross) to *On Abstract Drecative Nouns* (Noah A. Twadell and Coughlake Sweat, a pair of pseudonyms for Robert Binnick). In the words of the editors, the volume presents the early basis of generative semantics, as well as "'hard stuff', i.e., pornolinguistics and scatolinguistics; 'soft stuff', i.e., parody and burlesque; and whimsy, which is neither obscene nor very defamatory".[162] *Studies Out In Left Field* is a crucial source for the history of the generative semantics movement, as it contains a large number of otherwise inaccessible papers.

Finally, four giants of the 1960s underground literature scene were published as individual monographs between 1970 and 1986. George Lakoff's doctoral dissertation, *On the Nature of Syntactic Irregularity*, was published by Holt, Rinehart and Winston in 1970 under the title *Irregularity in Syntax*. "The appearance of this volume marks the end of the long period", James McCawley wrote in the foreword, "during which George Lakoff's *On the Nature of Syntactic Irregularity* (henceforth, ONSI) has been an underground classic, circulated in the form of a Harvard Computational Laboratory progress report and countless second and third order Xeroxes of that report, and quoted in scholarly journals more often than all but a handful of aboveground linguistics books have been".[163] In 1975, Chomsky's *The Logical Structure of Linguistic Theory* was published under the same name by Plenum Press, following two decades on the underground circuit. Four years later, Paul Postal's 1962 dissertation, *Some Syntactic Rules in Mohawk*, was released as part of the *Outstanding*

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[162]Zwicky et al. (eds.), *op. cit.*, p viii.
[163]Lakoff, *Irregularity in Syntax*, p v.

Dissertations in Linguistics series by Garland Publishing. Lastly, John Robert Ross' Ph.D.
thesis, *Constraints on Variables in Syntax*, was published in 1986 – just shy of two decades
after it was completed – under the title *Infinite Syntax!*. "The present work", wrote Postal
(who was then at the ıвм ʈj Watson Research Center) in the preface, "is among the most
frequently cited works in the transformational tradition", despite its origin as an under-
ground document.[164] Together, the publication of these volumes and the two anthologies
of underground work mark the opening of an era of private knowledge for public con-
sumption.

The impetus behind the publication of underground works in the 1970s and 1980s was
twofold. First, many underground documents had been seminal to early transformational
grammar and were imbued with the lore of a generation and of an era. These works were
cited frequently in both underground and aboveground literature in the 1960s and beyond
– and with citation came demand. In his introduction to the 1975 release of *The Logical
Structure of Linguistic Theory*, Chomsky noted that "[a] number of colleagues have informed
me that they find it [the manuscript] useful and have suggested publication" – a sugges-
tion which Chomsky says he only adopted reluctantly, due to the "unfinished character
of the manuscript and its date".[165] Like Chomsky, other authors were also influenced by
demand for their work, leading to the publication of individual volumes and the inclusion
of papers in anthologies. Second, the editors of the two anthologies – McCawley, Zwicky,
Salus, Binnick, and Vanek – had a personal interest in seeing underground works come
into the light. All had been central to the development and perpetuation of underground
culture – and when this era came to a close, they transferred the energy and enthusiasm
which theretofore had been devoted to the trappings of underground culture to the publi-
cation and celebration of this culture. *Studies Out In Left Field* proved immensely popular,
and copies were regularly stolen from libraries – an indication of the desire to retain a con-
nection to an older era.[166] By putting their stamp on these anthologies through editorial

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[164]Ross, *Infinite Syntax!*, p xvii.
[165]Chomsky, *Introduction*, p 1.
[166]Darnell, *op. cit.*

choices, introductions, and commentaries on individual papers, these five editors put their stamp on an exciting and colorful era – one which they were proud to have been a part of, and one which they could finally discuss publicly.

The establishment of new linguistics journals and the creation of a journal system large enough for and responsive to the needs of the American linguistics profession in the late 1960s marked the end of the underground culture in transformational grammar. The subsequent publication of underground works through the 1970s and 1980s shed light on what had been, for over a decade, a culture of private knowledge. For some, the establishment of a normalized publication culture in American syntax was an important step forwards – a clear rejection of the exclusive and proprietary attitudes of the 1960s, and a substantial effort to open transformational grammar to all interested. For James Fidelholtz, the end of the underground culture represented "a decrease in elitism" and "a more ready accessibility of important articles": a positive move towards abandoning the old restrictive practices and bringing the discipline into line with academic expectations.[167] For others – especially those who had been at the heart of the underground movement – it was the end of an exuberant and productive era, and the normalization of publication mechanisms was met with a mixture of relief and nostalgia. "Re-reading [*Studies Out In Left Field*] takes me back to another part of my own life", wrote Regina Darnell, to "a younger, freer, more innocent era, in which it was vitally important that intellectual work be *fun*".[168] Importantly for the historian, the publication of and commentary on previously private works provides important insight into a unique era.

<p style="text-align:center">* * *</p>

Through the 1960s, transformational grammarians valued the private circulation of underground knowledge more highly than publication in mainstream journals. They developed and maintained a tight network for the transmission of underground literature – and outsiders had little, if any, access to this literature. Within the transformational community, this underground culture provided a sense of group inclusion and status, as well as

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[167]Fidelholtz, *op. cit.*, p 932.
[168]Darnell, *op. cit.*, p 112, emphasis in original.

a mechanism for circumventing the slow and unsatisfactory mainstream publication process. To proponents of rival syntactic theories, transformationalists were seen to have an "in-group, rather elitist attitude", and frustrations with the inaccessibility of underground literature grew through the decade.[169] Despite this pervasive underground culture, transformational theory dispersed across America and, by the mid-1960s, achieved a dominant status in American academic linguistics. Even as transformational research results were circulated narrowly among a select group of insiders, the theory outperformed both constituency grammar, which had been dominant in the 1940s and 1950s, and stratificational grammar, the strongest challenger of the 1960s. This apparent paradox – the rise of a private theory – has not been addressed in other accounts of transformational grammar, but resolving it is imperative to our understanding of 1960s American syntax.

In this chapter, I have argued that three factors are needed to explain this seeming paradox: the dispersion of transformational grammarians from MIT and the subsequent formation of informal networks across the country; the access provided to transformational theory via aboveground textbooks; and the oral dispersion of the theory at conferences, colloquia, and Linguistic Institutes. These factors expose the mechanisms through which knowledge of transformational grammar escaped, and was allowed to escape from, the underground scene. These arguments also illuminate an important tension between private and public knowledge: by both valuing the private transmission of knowledge on underground networks and deliberately sharing portions of their theory with a public audience, transformational grammarians enjoyed the advantages of underground culture while also ensuring that students would have access to and be trained in transformational theory. The underground literature scene was highly valued for the speed at which it circulated results and for the sense of group cohesion and prestige it provided. At the same time, the public presentation and dispersion of transformational theory through informal networks, textbooks, and oral fora enabled transformational grammarians to defend and promote their theory, criticize rival approaches to syntax, and ensure that linguistics students would be

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[169]Fidelholtz, *op. cit.*, p 931.

trained in the transformational paradigm. The success of transformational grammar in the 1960s needs to be understood as the result of a carefully-balanced approach to private and public knowledge which enabled the transformational community to escape the mainstream publication circuit and build a sense of group identity, while also spreading their theory to the next generation of linguists.

Existing accounts of the rise of transformational grammar are flawed because they do not take into account the underground culture which pervaded the discipline from 1957 to 1968. As such, they cannot explain the paradox caused by the success of this theory in light of its private culture. As argued in Chapter 4, these accounts are also flawed by their omission of the role of pedagogy and training in 1960s syntax. This chapter shows that these two deficiencies are intimately linked: pedagogy and training are precisely the areas in which transformational grammarians made an exception to their preference for private knowledge, bringing underground literature and transformational theory to students by means of informal university networks, textbooks, and Linguistic Institute courses. While they valued the private circulation of knowledge highly, transformationalists realized that this culture would have to be tempered if their theory was to be continued by subsequent generations. Specifically, they would have to provide students with access to transformational ideas in order to obtain new commitment to the theory. They accomplished this by capturing the textbook and pedagogical markets early in the 1960s, and ensuring that a generation of linguistics students developed a transformational worldview. By investigating neither underground culture nor pedagogy and training, the existing literature misses a key connection between the two areas, and fails to adequately account for the rise of transformational theory. This chapter rectifies this lacuna by showing how transformational grammar dispersed across America and captured a generation of students despite its pervasive underground culture.

6 The Debate on Other Fronts

This study has thus far focused on manifestations of the syntactic theory-choice debates of the 1960s in university-level academic linguistics. In that decade, however, linguistics – the study of language – was far from constrained by academic or intellectual boundaries: it spilled out into a variety of fields including, most prominently, language teaching, field-work, missionary work, and machine translation. Language teachers at all levels, from elementary school to high school to college, espoused applications of theoretical linguistics for the teaching of speaking, reading, composition, English-as-a-second-language, and foreign languages. For American fieldworkers and missionaries – especially those affiliated with SIL International, the Wycliffe Bible Translators, and the Hartford Seminary Foundation – linguistic analysis was an essential element of their toolkit, used to describe rare languages in the field, to develop written forms for otherwise-oral languages, and for Bible translation and proselytization. Finally, machine translators brought theoretical linguistics to bear on the problems of developing automatic translation procedures to assist the American Cold War effort. Together, I will refer to this group of fields as *lay linguistics*. This chapter explores the relationship between academic and lay linguistics in the 1960s and, specifically, the influence of this relationship on the syntactic theory-choice debates.[1] I investigate the incorporation, use, and value of syntactic theories in lay linguistics; the efforts academic linguists took to extend their authority and influence in lay domains; and the role of academic linguists as public authorities on language. This line of inquiry pro-

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[1] Throughout this chapter, the term *academic linguistics* is used to refer to the theoretical linguistic work conducted on the American university scene between 1957 and 1970, and *academic linguists* to those who undertook this theoretical work. It is meant to contrast with the term *lay linguistics*, which is used to refer to applications of linguistic theory.

vides an important and different perspective on American syntax in the 1960s.

The picture obtained from investigating lay linguistics is starkly different than that obtained from the academic scene alone. In lay fields, the strong divisions which existed on the academic circuit between rival syntactic theories were blurred and inconsistent. While the three theories of interest to this study – constituency, stratificational, and transformational grammars – all had their proponents in lay linguistics, actual practice in lay fields frequently involved a mishmash of theories. Lay practitioners assembled what they saw as the advantageous tools of each theory into a toolbox designed for their work, while leaving aside aspects of the theories useless for or incompatible with their work. Often, lay linguists misunderstood or misrepresented linguistic theories, at times unintentionally and at times deliberately. Considered from the point of view of academic linguists, the results were heretical: syntactic theories which competed on the academic scene were taken apart piece-by-piece on the lay scene and scavenged for useful parts. I show that the divide between academic and lay linguistics is best understood as a distinction between *theories* and *tools*: lay practitioners rejected the academic view of linguistic theories as broad bodies of knowledge including technical tools, philosophies, and methodologies, and instead focused on descriptive and analytic tools themselves, which they evaluated according to their practical utility.

As a general rule, transformational theory enjoyed attention in language teaching, stratificational theory in machine translation, and constituency theory in fieldwork and missionary work. These identifications, however, only capture broad trends and are not representative of the intricacies of the era. By exploring these intricacies in detail, this chapter shows that the theory-choice debates which consumed academic syntacticians in the 1960s were neither extended to nor paralleled on the lay scene. The extent to which academic linguists cared about lay matters and worked to extend their authority in lay domains varied greatly between schools and within schools. There was little consensus within any school as to the importance or utility of lay matters: those academic linguists interested in lay contexts devoted time and energy to them, and those with no interest either ignored or scorned them.

Surprisingly, some linguists with strong theoretical commitments on the academic scene shed those commitments when discussing lay matters.

To develop these ideas, this chapter investigates language teaching (section 6.1), fieldwork and missionary work (section 6.2), and machine translation (section 6.3). For each of these areas of lay linguistics, we explore the use and status of syntactic theory, the interaction between lay and academic linguists, and manifestations of the theory-choice debates. There is a complex web of relationships between language teachers, educationalists, fieldworkers, missionaries, machine translators, and academic linguists – relationships ranging from intimate contact to outright rejection. I argue, first, that lay linguists were driven primarily by practicality and applicability, and were not concerned with theoretical completeness or commitment and, second, that academic linguists provided no uniform or consistent response to lay work, effectively drawing a boundary between the two contexts. Finally, I argue that this divide is best understood in terms of the conceptual divide between *linguistic theories* and *linguistic tools*. Together, these investigations show that lay linguistics had little influence in academic linguistic circles, little influence on the syntactic theory-choice debates of the 1960s, and little influence on the rise of transformational grammar. In particular, my arguments counter H.A. Gleason's claim that "[w]idespread recognition by other disciplines was, perhaps, the most potent force in the establishment of TG as dominant in North American academic linguistics".[2]

The thrust of this study is to understand the rise of transformational grammar and, concomitantly, the decline of rival syntactic theories between 1957 and 1970. In this context, it is insufficient to limit investigation to the academic world. Through the 1960s, syntactic theories were widely discussed, debated, and applied in a variety of lay disciplines. This work involved hundreds of teachers and thousands of schoolchildren, hundreds of fieldworkers and missionaries in communities around the world, and scores of machine translators. Supported by the National Science Foundation, the National Council of Teachers of English, the American military and Christian charities, among other funding bodies,

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[2]Gleason Jr, *Theories in Conflict*, p 71.

lay linguistics constituted a dynamic and broad body of work – one which was hailed as a solution for America's knowledge gap, a strategic Cold War weapon, and a tool for saving endangered languages. In the end, however, I show that lay contexts had little influence on the success transformational grammar enjoyed in American academic linguistics. While this conclusion may disappoint, it teaches us three important lessons: first, it emphasizes areas in which constituency and stratificational grammars excelled over transformational theory (namely, fieldwork, missionary work, and machine translation). Secondly, it deconstructs the singular theoretical commitments of the academic context and illuminates the assembly of practically-oriented linguistic toolkits from a variety of competing theories. Lastly, it demonstrates that dominance in the academic and lay contexts do not necessarily go hand-in-hand and, in doing so, counters existing accounts of the rise of transformational grammar.

6.1 Language Teaching

[L]inguistics is increasingly affecting what is done to children in school [and] is here to stay.

Neil Postman and Charles Weingartner, in *Linguistics: A revolution in teaching* (1966)[3]

Whether teaching Johnny the complex business of creating relative clauses or the processes of writing sophisticated sentences, the transformational model of syntax holds immense value.

Curtis Hayes, in *Syntax: Some present-day concepts* (1967)[4]

[T]he study of transformation grammar as an aid in teaching composition could not justify itself in terms of the time expended on the grammar.

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[3]Neil Postman and Charles Weingartner, *Linguistics: A revolution in teaching* (New York: Delacorte Press, 1966), p ix.
[4]Hayes, *op. cit.*, p 96.

Mark Lester, in *The Value of Transformational Grammar in Teaching Composition* (1967)[5]

The launch of Sputnik in 1957 stimulated massive government investment in the American education system – and, specifically, in language teaching. In the years after the launch, educationalists and politicians alike highlighted the need to teach American children to wield the English language properly and to increase foreign language capacity among young adults. They quickly turned to the academic linguistics field, asking what theoretical linguistics could offer to the classroom. By the early 1960s, transformational grammar was being promoted for use in teaching reading, composition, English-as-a-second-language, and foreign languages. Despite intense efforts by a pair of English professors, however, transformational theory was not universally adopted by language teachers: others argued loudly that transformational grammar had no place in the classroom, and there was little consensus in the profession. The actual *use* of linguistic theory by teachers shows that they valued practical applicability ahead of theoretical commitment, often mixing-and-matching syntactic tools to suit their needs. This section explores the relationship between academic linguistics and America's language teaching profession in the 1950s and 1960s.

Even before the Soviet Union took America by surprise on October 4, 1957, American teachers and educationalists had a particular interest in bringing linguistics to the classroom. Through the 1950s, they debated and discussed the need for syntactic tools to improve the reading and writing capabilities of America's elementary school students, and the composition abilities of high school students. In this decade, language teaching methodology was based on what were commonly called *traditional grammar* and *structural grammar*. Traditional grammar refers to school book sentence parsing using building block, or parts-of-speech, concepts such as *subject*, *predicate*, and *verb*. It includes the classification of words and groups of words into parts-of-speech categories, conjugation tables for verbs, and an emphasis on prescription, or the 'correct' use of language. Structural grammar was the label used to refer to what in this study is called Descriptivist linguistics, or, in the case

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[5] Mark Lester, "The Value of Transformational Grammar in Teaching Composition," in Mark Lester (ed.), *Readings in Applied Transformational Grammar* (New York: Holt, Rinehart and Winston, Inc., 1970), p 197.

of syntax, immediate constituency grammar. In the classroom, the teaching of structural grammar was strongly influenced by Charles Carpenter Fries' *The Structure of English: An introduction to the construction of English sentences* (1952), which "both lucidly reveals the general method and aim of the [...] structuralists, and shows how this method is applicable to grammatical study on the high school and college levels".[6] Known for its use of the labels *Class I words, Class II words*, etc. for sets of words which correspond approximately to the traditional categories nouns, verbs, etc., Fries' book formed the basis for numerous elementary and high school language-teaching textbooks and workplans. However, as we will see, by the late 1950s teachers and educationalists were becoming increasingly discontent with the pedagogical tools offered by traditional and structural grammars.

At the 1956 Georgetown Round Table Meeting on Linguistics and Language Study, Brown University's W. Nelson Francis argued strongly for the use of academic syntax in the classroom: while the study of syntax was in its infancy, he argued, still it offered the only way forward for language teachers. "[W]e cannot wait [...] for further theoretical breakthrough before we make practical and pedagogical use of syntax", he told his audience: "[t]hose of us who are faced with the daily necessity of teaching something about the structure of language, not only to prospective linguists and teachers, but also to freshman writers of themes and sophomore students of poetry, must have some kind of system to work with".[7] An interim syntax was required for immediate application in the classroom, he emphasized, regardless of whether academic linguists had achieved consensus on theoretical matters. By the early 1960s, Francis' call had been taken up by teachers and educationalists across America. At professional association meetings, in journals, and in teaching newsletters, language teachers debated the merits of applying theoretical linguistics in their classrooms. Leading the movement were the National Council of Teachers of English and its journal, the *English Journal*. Through the 1960s, the *English Journal* regularly

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[6]Charles Carpenter Fries, *The Structure of English: An introduction to the construction of English sentences* (Harcourt, Brace and Co., 1952), Norman Eliason, "Review of Charles Fries' The Structure of English," *Modern Language Notes* 69/1 (1954), p 66–67.

[7]W. Nelson Francis, "An Interim Syntax for the Classroom," in Paul L. Garvin (ed.), *Report of the Seventh Annual Round Table Meeting on Linguistics and Language Study, 1956* (Washington DC: Georgetown University Press, 1957), p 39.

devoted space to weighing the pros and cons of syntactic theories for language teaching, publishing over 30 articles on the subject in the decade and devoting its May 1963 issue to 'Linguistics in the Classroom'.

This rising interest in linguistics was underpinned by high stakes in the classroom. In the 1960s, American teachers were facing "the impact of two explosions", wrote Albert Marckwardt, a professor of English at Princeton and the 1962 President of the Linguistic Society of America: "an explosion of population and an explosion of knowledge".[8] The first of these meant that class sizes were pushing ever upwards, causing teachers to continually seek new resources to deal with the increased number of students. Improvements in teaching methodology and applications of academic ideas were eagerly looked to to ease and simplify teaching in the face of the new stresses. The second – stimulated in large part by the concerted efforts of American scientists and engineers to rectify the knowledge gap with the Soviet Union – meant that teachers felt public and political pressure to introduce new knowledge in the classroom and improve the capacities of their students in areas of strategic importance (namely, mathematics, sciences, and languages). High school language courses, previously restricted to English, began to experiment with a multitude of languages from Arabic to Chinese to Swahili.[9] At the university level, too, the 1950s and early 1960s saw a dramatic increase in the teaching of foreign languages. "Universities which once were satisfied with a half dozen may now be teaching thirty or more", wrote H.A. Gleason in 1965 and, by his count, over 100 languages were regularly available at universities across the country in that year.[10] In this new era, Marckwardt emphasized, linguistic theory had a central role to play in the classroom. Teachers faced two key questions: first, "the question of how much of what is new [in linguistic theory] should be put into the subject as it is presented in secondary schools" and, second, the question of how much teachers themselves should learn of new linguistic theories, even if they did not teach

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[8]Albert H. Marckwardt, *Linguistics and the Teaching of English* (Bloomington: Indiana University Press, 1968), p 3.

[9]Gleason Jr, *Linguistics and English Grammar*, p 483.

[10]*Idem.*

them in the classroom.[11] By the mid-1960s, the majority of language teachers agreed that linguistics could "contribute importantly to [...] improvements in the ways in which students *use* language (i.e., in reading, writing, speaking, listening), along with a knowledge of how to go about learning that which is yet to be learned" – but, beyond this general statement, there was little agreement over *how* linguistic theory should be applied in the classroom, and which theories offered the most useful tools.[12]

Linguistic Theory in the Classroom

Transformational grammar appeared on the language teaching profession's radar screen in the early 1960s – but, as the opening quotes for this section demonstrate, there was no unanimity of views concerning the suitability of transformational theory for the classroom. One pole of opinion, represented by the University of Nebraska's Curtis Hayes, believed that transformational grammar held "immense value" for the teaching of language comprehension and composition.[13] At the opposite pole, represented by the University of Hawaii's Mark Lester, transformational theory was seen as too complex and too far removed from the practicalities of language teaching to "justify [...] the time expended on the grammar".[14]

In the years immediately after the publication of *Syntactic Structures*, transformational grammar was hardly known among teachers and educationalists. Directed specifically to linguists and mathematicians via publications including *Language* and *The IRE Transactions on Information Theory*, Chomsky's early ideas were neither intended for nor presented in a format amenable to language teachers. Soon, however, the language teaching profession began to take note of developments in academic linguistics. In 1961, the Commission on English (under the auspices of the National Council of Teachers of English) debated the applicability of transformational grammar to the classroom in its summer meeting. At that time, the general feeling among English teachers was that transformational theory may be " 'correct' in some mathematical sense [but is] pedagogically unacceptable to the needs

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[11] Marckwardt, *op. cit.*, p 4.
[12] Postman and Weingartner, *op. cit.*, p 29, emphasis in original.
[13] Hayes, *op. cit.*, p 96.
[14] Lester, *The Value of Transformational Grammar*, p 197.

of the secondary school curriculum".[15] This malaise would soon turn into a full-fledged debate centering on the proper role of linguistic theory in the classroom, the suitability of theoretical linguistics tools for pedagogical purposes, and the elucidation of language-learning patterns via linguistic philosophy.

In 1962, two English professors – Indiana University's Owen Thomas and Cornell University's Paul Roberts – began a campaign to introduce transformational theory to elementary and high school classrooms across the country. Thomas' endorsement of transformational grammar stemmed from a course entitled *English Grammar for Teachers* which he taught at Indiana University in the summer of 1961. The course invited 30 students – all of them teachers or teachers-in-training – to evaluate the efficacity of transformational theory as a classroom tool for improving the ability of students to read, write, and manage the English language. "Without exception", Thomas wrote in the course report, "the students were convinced that certain deductions from the theories of Chomsky could be applied systematically to the teaching of grammar, not only in the secondary school but with equal effectiveness in the elementary school".[16] The advantages of transformational grammar in the classroom, he argued, were manifold. Transformations "indicate the exact nature of the relationship between a kernel sentence and the associated passive, negative, and interrogative sentences", thus providing a systematic pattern-based structure with which students can learn to form complex sentences from simpler ones.[17] Teachers can use the division between kernel and transformed sentences to "select and arrange grammatical elements in the most logical order and to build effectively upon preceding material", Thomas continued, and thus gain a theoretically-motivated structure for language-teaching methodology.[18] "As teachers", he concluded, "we can hardly ask more of any theory".[19] In the following years, Thomas' views would be adopted with enthusiasm by some within the American teaching profession, and vigorously rejected by others.

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[15]Owen Thomas, "Generative Grammar: Toward unification and simplification," *The English Journal* 51/2 (1962), p 94.

[16]*Idem*, p 95.

[17]*Idem*, p 98, note 9.

[18]*Idem*, p 113.

[19]*Idem*.

In the *English Journal,* Thomas highlighted the advantages of transformational theory over the grammatical theories used by teachers in the 1950s – that is, traditional and structural grammars. The chief problem with structural grammar, he argued in 1963, was that it "cannot explain relationships between, for example, the active and passive voice; it can simply *describe* such differences as may exist in construction".[20] It also, he continued, cannot explain ambiguous sentences such as constructional homonymities – something only possible with the "rigorous" approach of transformational grammar.[21] As such, teachers looking to instruct their students in syntactic relationships and ambiguities would be negligent to use structural grammar. The teachers who attended Thomas' 1961 *English Grammar for Teachers* course concurred: structural grammar, they concluded, could not be "readily adapted to the needs of secondary school pupils", and traditional grammar was riddled with inconsistencies and provided an outlook "too widespread and too basic" for pedagogical needs.[22] While immediate constituency theory enjoyed success on the academic scene through the 1950s, language teachers argued that it put too much emphasis on suprasegmentals and lacked the systematicity, consistency, and relevance required for the classroom. Instead of providing students with logical tools for language analysis, they concluded, it merely taught students to split sentences into "a hodge of podges".[23] Many of these arguments against constituency grammar were copied point-for-point from pro-TG academic literature and reworked to fit the language teaching scene.

With support from, among others, William Moulton, a Cornell- and Princeton-based linguist well-known for his work on German during the Second World War, Thomas' views gained credibility and began to spread. Speaking at the Ninth International Congress of Linguists in 1962, Moulton argued for the immediate application of transformational theory in the classroom: "[t]hough transformational grammar is too new to permit predictions", he said, "it seems likely that it can have far-reaching effects in improving both the presentation of grammatical structure in textbooks and the learning of grammatical struc-

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[20] Owen Thomas, "Grammatici Certant," *The English Journal* 52/5 (1963), p 325, my emphasis.

[21] *Idem,* p 325.

[22] Thomas, *Toward Unification and Simplification,* p 96.

[23] *Idem.*

ture through classroom drill".[24] It was Thomas' insistence that traditional and structural grammars were inadequate and unsuitable for the classroom which garnered him the most attention. The wide-spread use of these grammars in language-teaching, the University of Illinois at Chicago Circle's Falk Johnson wrote in the *English Journal*, "may explain at least partly why students have such tremendous difficulty in learning grammar – why year after year, in grade after grade, they grapple with grammar and gripe about it, but only rarely succeed in grasping it".[25] As a result of this teaching methodology, others agreed, students had difficulty determining "the relationship of the component or constituent parts [of a sentence] to each other" – and thus a difficult time learning to distinguish grammatical from ungrammatical structures.[26] The application of transformational grammar in the classroom, Johnson and others concluded, "correlated with the psychological development of the student, may enable him, not merely to grapple with grammar, but, triumphantly, to grasp it".[27]

Four years after conducting his initial course on transformational grammar in the classroom, Thomas released a book on transformational theory directed to English teachers, providing one of the first comprehensive introductions to transformational grammar intended for a non-academic audience. Released by Holt, Rinehart and Winston in 1965, *Transformational Grammar and the Teacher of English* received a great deal of interest from the teaching community but, as we will see, was heavily criticized by academic linguists. "Transformational grammar has significant application to the teaching of all languages, including English, at all grade levels and to both the native and non-native speakers", wrote Thomas in the opening pages, "[b]ut both teachers and prospective teachers of English generally feel that transformational grammar is both esoteric and forbidding".[28] With this in mind, his book was designed to provide a simplified introduction to the aspects of transformational theory deemed immediately applicable to classroom teaching. English teachers

[24]Moulton, *op. cit.*, p 108.

[25]Falk S. Johnson, "On Identifying the Parts of Speech," *The English Journal* 55/6 (1966), p 748.

[26]Marckwardt, *op. cit.*, p 23.

[27]Johnson, *op. cit.*, p 751.

[28]Owen Thomas, *Transformational Grammar and the Teacher of English* (New York: Holt, Rinehart and Winston, Inc., 1965), p 1.

have an "obligation", he argued, to learn and apply transformational theory: doing so is essential for "taking the student from one level of linguistic sophistication to a higher and more efficient level".[29] The most significant claim made in the book concerns language acquisition, innateness, and creativity. Thomas argues that transformational theory – with its commitment to a mentalist interpretation of the language acquisition process – can be used to identify typical grammatical mistakes and to provide methods for helping students overcome these mistakes. "If it proves true – as it almost certainly will – that a pupil acquiring a new derived pattern makes a few stumbling grammatical errors because he has not yet mastered the pattern", Thomas wrote, transformational theory can be used to "construct exercises that will help him overcome these errors in the shortest possible time".[30] This strong interpretation of transformational theory assumes that transformations themselves are psychologically valid, and that they can be reinforced through training. Both elements of Thomas' argument – the necessity for transformational grammar in the classroom and the psychological validity of transformations – would, as we will see, be challenged by others in the educational community.

Even more influential than Thomas' book was the work of Cornell University's Paul Roberts, who wrote a series of articles and books in the mid-1960s promoting the use of transformational grammar in the classroom. "Only today, thanks to the developments in linguistics", he wrote in his 1963 *Linguistics and the Teaching of Composition*, "we can do quite a lot better [at teaching language] than we could twenty or thirty years ago" – an improvement due entirely to the introduction of transformational theory.[31] With the development of Chomsky's linguistics program, he argued, "we are now in the position where grammar can be *taught*, and not just endlessly reviewed, as has been the practice heretofore".[32] Roberts reached out to both students and teachers, publishing *English Syntax: A book of programmed lessons (an introduction to transformational grammar)* (Harcourt, Brace and World, 1964), an exercise book designed to be "of practical use" to senior high school students,

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[29] *Idem*, p 17, 219–220.
[30] *Idem*, p 219–220.
[31] Paul Roberts, "Linguistics and the Teaching of Composition," *The English Journal* 52/5 (1963), p 334.
[32] *Idem*, p 335, my emphasis.

and *English Sentences: Teacher's manual* (Harcourt, Brace and World, 1962), which encouraged teachers to apply transformational grammar in the classroom.[33] Like Thomas, Roberts saw the distinction between kernel sentences and transformed sentences as essential to improving students' grasp of language. It is transformed sentences – a category which corresponds to relatively complex sentences – that cause "all the complications of English", he argued in *English Syntax*, and hence must be at the heart of language teaching strategies. Students make grammatical errors, he noted, "not in kernel sentences but in transformations of the kernel, in which they lose sight of and confuse the kernel relationships".[34] With this identification of the problem, Roberts provided a plethora of exercises to train students in correctly producing transformed sentences. Given a set of kernel sentences and transformational rules, he asserted, students "are able to generate a countless number of many correct English sentences" while also "keeping relationships clear through complicated sentences".[35]

In the wake of Thomas' and Roberts' work, transformational grammar was put into practice in classrooms across the country. At Sedgwick Junior High School in West Hartford, Connecticut, for example, English teacher Eileen McGuire praised transformational theory for offering the student "a grammar that moves with his thought, that in some way parallels the process going on in his mind".[36] She designed a work plan for the ninth grade intended to teach students to build complex sentences from basic ones by means of transformations – a work plan, she wrote in the *English Journal*, which brought "the practical possibilities of transformational grammar to the student's service as he learns to write good sentences".[37] At this point in a student's development – around age 14 – McGuire argues, language teachers can successfully apply transformational theory to help students who are "'grappling'

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[33]Paul Roberts, *English Syntax: A book of programmed lessons (An introduction to transformational grammar)* (New York: Harcourt, Brace & World, Inc., 1964), p 386, Paul Roberts, *English Sentences: Teacher's manual* (Harcourt, Brace and World, 1962).

[34]Roberts, *Programmed Lessons*, p 386.

[35]*Idem*, p 385–386.

[36]Eileen J. McGuire, "Sentence Building and Transformational Grammar," *The English Journal* 56/5 (1967), p 748.

[37]*Idem.*

with forms and ideas" to master the correct construction of complex sentences.[38] In the mid to late 1960s, the *English Journal* was full of similar plans for implementing transformational theory in the classroom. Peter Youmans (Pascack Valley High School, Hillsdale, New Jersey) advocates the use of transformational grammar in teaching English composition; Philip Cook (Department of English, San Jose State College) weds Chomsky's ideas with rhetoric and composition; Lawrence Ianni (Professor of English, Pennsylvania State College) provides exercises to teach students to combine sentences via transformational rules; and Verna Newsome (Professor of English, University of Wisconsin-Milwaukee) applies transformational analysis to writing style.[39] Transformations, these authors agreed, "present the student with a clear concept of the nature of grammar and a more methodical, more easily apprehensible means of putting grammatical knowledge to work in composition".[40] They championed transformational rules as a pedagogical method for fostering in students an understanding of sophisticated sentences: these rules, Newsome asserted, "reveal[] the processes of coordination and subordination more clearly and bring[] alternative grammatical structures into sharper contrast".[41] For this group of teachers and educationalists, transformational grammar was "more practical", easier to use, and pedagogically superior to traditional and structural grammars.[42]

Transformational theory also impacted on the teaching of English-as-a-second-language and foreign languages. In his 1963 *Grammatical Theory and The Teaching of English as a Foreign Language*, the University of California at San Diego's Leonard Newmark asserts that English-as-a-second-language teachers see four advantages in transformational grammar: first, it is "the most promising response we have to our desire for descriptions that *explain* rather than merely display language data"; second, it has "the advantage of showing not

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[38]*Idem.*

[39]Peter N. Youmans, "Practicing Linguistics," *The English Journal* 54/4 (1965), Philip H. Cook, "Putting Grammar to Work: The generative grammar in the generative rhetoric," *The English Journal* 57/8 (1968), Lawrence Ianni, "An Answer to Doubts about the Usefulness of the New Grammar," *The English Journal* 53/8 (1964), Verna L. Newsome, "Expansions and Transformations to Improve Sentences," *The English Journal* 53/5 (1964).

[40]Ianni, *op. cit.*, p 597.

[41]Newsome, *op. cit.*, p 335.

[42]Hayes, *op. cit.*, p 96.

only the direct and superficial, physically manifest similarities and differences between two languages, but also the more profound differences and similarities between languages that appear when the rules of sentence formation are required to be explicitly formulated"; third, it provides a systematic method for teaching second languages; and, fourth, transformational drills are "easy to construct and easy to operate in class".[43] Following the belief that language teaching should proceed "from grammatically simpler to more complex [ideas]", teachers restricted beginning courses to kernel sentences and only introduced transformed sentences in intermediate and advanced courses.[44] For the first time, Newmark concludes, English-as-a-second-language teachers felt that they had "a grammatically motivated principle for ordering the presentation of sentences [to students]".[45]

Even as transformational grammar was increasingly lauded in educational journals and applied in classrooms, the Ohio State University College of Education's Frank Zidonis conducted a two-year study to experimentally test the efficacity of transformational theory as a classroom tool. Working with the ninth and tenth grades, Zidonis aimed to answer a number of questions: "Can high school pupils learn to apply the transformational rules of generative grammar in their writing?", "To what extent will the proportion of well-formed sentences increase in pupil writing over the two-year period?", "Can students' repertoire of grammatical structures be increased by a study of generative grammar?", and "What kinds of transformational and co-occurrence errors will occur in pupil writing, and what extent will such errors increase or diminish over the two-year period?".[46] Conducted between 1963 and 1965, Zidonis' study was supported by a grant from the United States Office of Education and published in the *English Journal* and the report of the National Council of Teachers of English. His results provided strong support for the application of transformational grammar in the classroom. Knowledge of transformational principles, Zidonis

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[43]Leonard Newmark, "Grammatical Theory and the Teaching of English as a Foreign Language," in Mark Lester (ed.), *Readings in Applied Transformational Grammar* (New York: Holt, Rinehart and Winston, Inc., 1970), emphasis in original. Newmark himself disagrees with the application of transformational grammar in the second-language teaching classroom, but describes the situation as he sees it.

[44]Newmark, *op. cit.*, p 205–208.

[45]*Idem.*

[46]Frank J. Zidonis, "Generative Grammar: A report on research," *The English Journal* 54/5 (1965), p 405.

concluded, "enables pupils to increase significantly the proportion of well-formed sentences they write", and his statistical analysis suggests that there is "a relation between a knowledge of generative grammar and an ability to produce well-formed sentences of greater structural complexity".[47] Together, textbooks, journal articles, work plans, and experimental studies advocating the use of transformational grammar in the classroom gained a following among both language teachers and educationalists – but support for transformational theory was by no means ubiquitous in the language teaching profession.

Through the 1960s, a second group of language teachers and educationalists spoke out against the use of transformational grammar in the classroom. In publications from *Readings in Applied Transformational Grammar* to the *English Journal*, they argued that "[s]chools are making a serious mistake" by using transformational grammar as teaching methodology.[48] The University of Hawaii's Mark Lester asserted that the entry price of transformational grammar was too high to justify bringing it into classrooms: so much time and effort would be required to teach students and teachers about the theory, he argued, that any benefits to composition or language comprehension would be undermined.[49] Drawing on the competence-performance distinction, Lester also painted transformational grammar as inapplicable to teaching situations. "[P]erformance is the proper subject matter of the composition class", he wrote in *The Value of Transformational Grammar in Teaching Composition*, but since transformational theory explicitly rejects the study of linguistic performance, it cannot in principle assist students to improve their performance.[50] Lester's opposition to the application of transformational theory in the classroom was supported by, among others, R. Donald Cain (the assistant director of editing and textbooks for the Philadelphia public school system) and Harvard University's John Mellon, whose study investigating the effect of transformational theory on the "rate of student growth in syntactic ability" concluded with mixed-to-negative results.[51]

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[47]*Idem*, p 408.

[48]George Beissel, "Letter from George Beissel," *The English Journal* 56/8 (1967), p 1197.

[49]Lester, *The Value of Transformational Grammar*, p 197.

[50]*Idem*, p 194.

[51]Beissel, *op. cit.*, R. Donald Cain, "What Do We Mean by 'Linguistics'?," *The English Journal* 54/5 (1965), William G. Stryker, "Review of John C. Mellon's Transformational Sentence-Combining: A method for enhanc-

Proponents of transformational grammar in the classroom also faced criticism from academic linguists who felt that their theories were being misrepresented and misused on the lay circuit. Owen Thomas and Paul Roberts both came under fire for technically-deficient presentations of transformational theory. In a highly critical review of Thomas' *Transformational Grammar and the Teacher of English*, James McCawley describes the book as full of technical shortcomings and errors, "most of which could have been avoided if the author had explored the available literature a little more thoroughly and had reread his manuscript reasonably carefully and if the publisher had had the manuscript read and criticized by someone who is well up on current work on English syntax".[52] The chief technical flaw in Thomas' work concerns his treatment of deep structure: he assigns deep structures with no explanation or justification, and his readers are not provided with any rationale for choosing between various possible deep structures. "Thomas is a talented popularizer", concluded McCawley disparagingly, a writer who "puts across ideas lucidly and readably to the extent that he has understood them in the first place".[53] Likewise, Roberts faced criticism from Robert Hall, who asserted that Roberts' workbooks were based on "partially misunderstood transformational grammar".[54] Even Marckwardt, who advocated the use of linguistic theory in the classroom, remarked that teachers and educationalists "betray[ed] a certain lack of understanding" of transformational theory.[55]

Between these two poles – that which advocated for the use of transformational theory in the classroom and that which argued against it – fell a third position, one of particular interest to this study. In practice, the application of syntactic theories in the classroom in the 1960s often did not correspond to a single theory on the academic scene, but rather morphed and combined theories into a form suitable for the classroom setting. Teachers selected pedagogically-advantageous tools from a number of syntactic theories to de-

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ing the development of syntactic fluency in English composition," *The English Journal* 59/6 (1970), p 862.

[52] James D. McCawley (ed.), "Review of Owen Thomas' Transformational Grammar and the Teacher of English," in *Grammar and Meaning: Papers on syntactic and semantic topics* (New York: Academic Press, 1976), p 15.

[53] *Idem*, p 33.

[54] Hall Jr, *Recent Developments*, p 94.

[55] Marckwardt, *op. cit.*, p 25.

sign language-teaching toolboxes. The most common combination featured constituency and transformational grammars. "The point is that neither system, descriptive or transformational, is necessarily better than the other", wrote the San Jose State College's Michael Grady in the *English Journal*: each has pros and cons for classroom use.[56] "If one wishes to know the order of words of the favorite sentence types of English, the simplest way of gaining the information is to learn its descriptive notation", he continued, but "[i]f one wishes to learn more about the genesis of the particular pattern, one must study its generative-transformational aspects".[57] For language teaching purposes, he identifies constituency theory as best for "the purposes of most instruction in *writing*", and transformational theory as best for explaining sentence patterns such as active-passive pairs.[58] In the classroom, he concludes, "an amalgamation of the two is better than exclusive reliance on each individually".[59] Grady worried that teachers and educationalists had been blinded by the success of transformational grammar on the academic scene and were "tak[ing] unto their bosoms transformational linguistics in its entirety simply because it is 'newer' ".[60] He insisted that transformational grammar should be seen not as "a more up to date *replacive* for descriptive linguistics for the purposes of teaching writing", but instead as an additional tool to be selectively added to the language teacher's toolbox.[61]

Grady's assessment was implemented in several areas of language teaching. In her English-as-a-second-language college-level textbook *English Syntax: Advanced composition for non-native speakers* (Holt, Rinehart and Winston, 1965), Ann Nichols argues that non-native English speakers learn best through a combination of constituency and transformational grammars. While a "large percentage of foreign students in the United States are graduate students who must prepare seminar papers and theses in intelligible English", she wrote, few composition textbooks on the market address their needs since "the foreign student's problems, his questions, his language habits are totally dissimilar from those of the

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[56] Michael Grady, "The Uses of Linguistics in the Schools," *The English Journal* 57/6 (1968), p 872.
[57] *Idem.*
[58] *Idem*, p 872, emphasis in original.
[59] *Idem*, p 872.
[60] *Idem*, p 872–873.
[61] *Idem*, p 872–873, emphasis in original.

typical student in a freshman composition class".[62] With this in mind, her text is designed to teach English composition to non-native speakers using a synthesis of constituency and transformational theories. Constituency grammar, she argues, is necessary for teaching students to divide constructions into logical parts (i.e., subject and predicate), while transformational grammar is valuable for distinguishing basic from complex constructions and for identifying widely-used syntactic patterns. On the high school level, George Beissel's *A Program in Modern English* (Beissel English Services, 1967) attempts to "synthesize[] the best of three grammars – structural, traditional, and transformational" in order to create an effective language-learning program.[63] He, too, uses traditional and structural grammars to acquaint the student with basic sentence structures, and relies on transformational grammar to teach sophisticated sentence constructions. The combination of transformational grammar with other syntactic theories was also supported by the University of Nevada's Charlton Laird, who argued that "[t]he concept of the transform may simplify the grammatical statement [...] but it may or may not provide the clearest explanation of the way the language is working or the handiest means of teaching the use of the language".[64] "The best curriculums of the future", he continued, "will make more use of other linguistic and rhetorical devices": they will rely not on any one syntactic theory, but rather select syntactic tools for their ability to contribute to successful teaching.

The rivalry and sharply separate communities of transformational and constituency grammarians on the academic scene did not faze this third group: they saw linguistic theories not as mutually exclusive entities, but as sets of tools to be broken down, separated apart, and recombined as required for pedagogical needs. Whereas academic linguists saw their theories as tripartite bodies of knowledge, including technical linguistic tools, methodological assumptions, and philosophical commitments, language teachers rejected this broad conception and focused specifically on syntactic tools. This narrow approach

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[62] Ann Eljenholm Nichols, *English Syntax: Advanced composition for non-native speakers* (New York: Holt, Rinehart and Winston, 1965), p v.

[63] Shelby Johnson, "Review of George Beissel's A Program in Modern English," *The English Journal* 56/6 (1967), p 907.

[64] Charlton Laird, "Language: What to do about a drop-in," *The English Journal* 58/8 (1969), p 1202.

was enabled by the professional identity of language teachers: far removed from the academic linguistics scene, language teachers had no stake in the syntactic theory-choice debates. Commitment to a single linguistic theory offered no advantage to most teachers and educationalists and, consequently, the only criteria they deemed relevant to tool choice were applicability and suitability for classroom use. "Each system is correct", wrote Grady, "depending on the purposes one has" – a statement heretical in the academic context, but widespread on the lay scene. Language teachers explicitly recognized their willingness to overrule theory boundaries for pedagogical advantage: "I have at times adapted linguistic theory to my own purposes", Nichols wrote in her 1965 textbook as she explained that her application of transformational grammar was not faithful to Chomsky's ideas.[65] This adaptation was justified, she argued, because she had as her primary interest "the foreigner learning English, rather than [...] the linguist describing English".[66] Driven by practicality and applicability, this third group of language teachers valued a selection of tools from a variety of theories for their abilities to effectively teach and engage students.

The willingness of teachers and educationalists to mix-and-match theories was due in part to their wariness of the academic world. Of key concern to the educational community in the 1960s was the perceived instability of academic linguistic theories: in their view, transformational theory had burst onto the scene very quickly – too quickly, some said – driving fear that, in another few years, it would be replaced by yet another new theory. Faced with "a multitude of recent developments in the systematic study of language", teachers felt increasingly pressured to "come to grips with [...] new concepts and new approaches" – an investment they chose to make carefully.[67] Committing their energy and time entirely to transformational theory, they worried, would be a waste of resources since they might well have to adapt to another new theory in a short while. Unlike other subjects, linguistics provided teachers with no sense of stability or completeness. Academic linguists "are only just beginning to study and argue about syntax", wrote Nichols, and the

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[65]Nichols, *op. cit.*, p vii.
[66]*Idem.*
[67]Marckwardt, *op. cit.*, p 4.

resulting "lack of knowledge, or at least lack of any very sure conclusions, severely limits the practical linguists".[68] In light of this instability, many in the teaching profession "prudently rejected the opportunity to be burned" and divested their energies by focusing on syntactic tools, not theories.[69]

Reaction From Academia

The reaction of academic linguists to the use of their theories in language-teaching classrooms was, like that use itself, wide-ranging. Most critical of pedagogical applications of syntactic theory was Noam Chomsky. Addressing the 1966 Northeast Conference on the Teaching of Foreign Language, he asserted that "I am, frankly, skeptical about the significance, for the teaching of languages, of such insights and understanding as has been attained in linguistics and psychology".[70] Language teaching and theoretical linguistics, he continued to the chagrin of the conference organizers, were separate fields requiring separate expertise and had little, if any, meaningful overlap. No school of linguistics, he argued, "has achieved a level of theoretical understanding that might enable it to support a 'technology' of language teaching" – not even transformational grammar.[71] It is not, he concluded, the place, role, or mandate of academic linguists to comment on or advise the language teaching profession. Clearly, Chomsky saw no advantage for himself or for his theory by promoting its merits to language teachers. This mirrors his treatment of other lay disciplines including machine translation and computer applications, and emphasizes the low importance of lay linguistics as an explanatory criterion for the transformational community (cf. Chapter 3). It also highlights the limits of Chomsky's desire to be a public authority on language: through the 1960s, he promoted his linguistic theory to linguists and psychologists, but chose not to take a wider role in the public sphere.[72]

Even Robert Lees – on most fronts, an outspoken and assertive proponent of transfor-

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[68]Nichols, *op. cit.*, p vi.

[69]Thomas, *Toward Unification and Simplification*, p 94.

[70]Noam Chomsky, "Linguistic Theory," in Mark Lester (ed.), *Readings in Applied Transformational Grammar* (New York: Holt, Rinehart and Winston, Inc., 1970), p 37.

[71]*Idem.*

[72]Of course, Chomsky also promoted his political views widely and publicly in that decade.

mational grammar – did not see many opportunities for the theory in the classroom. His 1962 presentation to the National Council of Teachers of English convention, held at Miami Beach, downplayed the utility of syntactic theories for pedagogical purposes. The "promise of transformational grammar", as his paper was titled, lay not in teaching students to read and write, but in *explaining* the structure and nature of natural language.[73] Repeating his arguments from the academic circuit, Lees asserted that only transformational theory can explain language creativity and meaningfully account for differences between grammatical and ungrammatical sentences. Only transformational grammar, he told his audience, can explain the capacity with which every "normal, non-deaf, human child [is endowed] that enables him to learn any natural language so quickly and effortlessly if he is permitted to hear enough of it in a normal environment"; and only transformational theory can explain the ability of native speakers to "understand immediately any utterances in his language which he hears even though they are entirely new to him, or to construct quickly during conversation a never-ending succession of novel sentences each of which conforms perfectly to the requirements of well-formedness of his language".[74] But, despite his strong commitment to the theory, he did not promote transformational grammar in the classroom. "[I]t is obvious that every well-informed teacher of our schoolchildren ought at best to be familiar with the latest results in this area of linguistic science", he asserted, but "it is not at all clear [...] that the study or teaching of English grammar is very helpful in training children to write better".[75] Efforts to apply transformational grammar to language teaching, he concluded, have "little if any justification".[76] If there is any place for transformational theory in the schools, he added, it is "in the area of science and general education along with psychology and anthropology" – but not in language, composition, or English-as-a-second-language classrooms.[77]

In contrast to Chomsky's and Lees' rejection of the utility of transformational grammar in

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[73]Lees, *The Promise of Transformational Grammar.*
[74]*Idem*, p 327.
[75]*Idem*, p 345.
[76]*Idem.*
[77]*Idem.*

language-teaching classrooms, three of the chief opponents of TG in the 1960s – H.A. Glea-son, Robert Hall, and Archibald Hill – spoke out in support of the theory in the classroom. Well-known for four decades of strong-headed opposition to transformational grammar, Robert Hall broke with his usual stance when it came to language learning. "For humans, transformation exercises are helpful", he wrote in 1964, "particularly at the elementary level, in foreign language learning, and also in clarifying certain relationships in our native language".[78] Later, in 1968, Hall noted that transformational grammar is "useful in con-tributing to the design of more conventional language-learning textbooks, especially in the preparation of pattern-drills".[79] "[T]he number of sentence types which can be built up in this way [using transformations] is astonishing", added Archibald Hill in his 1966 *The Promises and Limitations of the Newest Type of Grammatical Analysis*, and transformational ex-ercises excel in their ability to "teach a lot of English in a short time".[80] Looking ahead, he painted a picture of transformational theory as central to language education: "[i]n compo-sition classes, formulaic operations can be of use when Johnny wants to learn how to handle relative clauses. The whole difficult business of teaching conscious control of sentence ma-nipulation will profit immensely by carefully controlled transformational explanations and drills. Freshman handbooks of the future, I predict, will have a lot to say about NP [noun phrase] and VP [verb phrase], the current transformational jargon, and perhaps less about the old and underlying terminology, subject and predicate".[81]

For his part, Gleason argued that to prepare students for the new world of the 1960s – an ever-shrinking world defined by internationality, communication, and language – they must be taught some basic linguistics: "they must know something of the nature and func-tion of language, of the differences between languages, and of the process of translation".[82] Through the decade, Gleason was a firm supporter of stratificational grammar – but this theory had no place in his thoughts on language teaching. His 1965 book *Linguistics and En-*

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[78]Hall Jr, *Introductory Linguistics*, p 226–227.

[79]Robert A. Hall Jr, "Fact and Fiction in Grammatical Analysis," in *Linguistics and Pseudo-linguistics: Selected essays, 1965–1985* (Amsterdam: John Benjamins Publishing Company, 1987), p 6.

[80]Hill, *Promises and Limitations*, p 18.

[81]*Idem.*

[82]Gleason Jr, *Linguistics and English Grammar*, p 484.

glish Grammar, designed to bring linguistic theory to bear "on the questions met in the class-room", presents a variety of linguistic theories which, he wrote, "at the present time may be considered live options for the American high school or junior high school".[83] Notably, while constituency and transformational grammars are included, stratificational grammar is excluded. Despite his strong commitment to the theory on the academic scene, he argued that stratificational grammar "has not yet received the development which would make it a possible contender for use in the schools", and hence that it did not yet have a place in language-teaching discussions.[84]

The reactions of Chomsky, Lees, Hall, Hill and Gleason to the use of linguistic theory in language teaching provide important insight into the relationship between academic linguists and lay contexts. On one hand, strong supporters of transformational grammar on the academic scene saw no role for that theory in language teaching and, on the other hand, opponents of transformational grammar on the academic scene – linguists who otherwise have little positive to say about the theory – openly advocated the use of TG in language classrooms. These departures from normal rhetoric show that the language teaching profession was considered by academic linguists to be far removed from the syntactic theory-choice debates of the 1960s. Importantly, proponents of rival syntactic theories felt no threat from promoting transformational grammar in the educational context. While language teachers were certainly interested in academic linguistic theories, this interest was only partially reciprocated: leading representatives from the academic scene considered language teaching unimportant to their theory-building efforts, and chose not to extend their debates to the educational scene.

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The picture obtained from studying syntactic theories in the educational context is strikingly different from that obtained from the academic scene alone. Importantly, many language teachers approached syntactic theories not as broad inseparable bodies of knowledge, but as sets of tools for language analysis. In contrast to the strong rivalries between

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[83]*Idem*, p vi.
[84]*Idem*, p vii.

theories on the academic scene, language teachers mixed-and-matched these theories to build toolboxes amenable to their pedagogical needs. This tool-oriented approach results from, first, the professional distance between language teachers and the academic context, which meant that teachers had little to gain from theoretical commitment and little interest in the academic theory-choice debates and, second, the high value placed on applicability and pedagogical utility on the lay scene. Further, as academic linguists chose not to extend their theory-choice debates to the language-teaching context, a boundary was drawn between the academic and lay circuits. All told, the relationship between academic linguists and the language teaching profession in the 1960s was limited and unidirectional: while teachers and educationalists applied syntactic tools in the classroom, there was no substantial or sustained interaction between the two groups. In particular, the use of syntactic theories in the language-teaching context had little influence on the rise of transformational grammar in academic linguistics, or on the relative decline of constituency and stratificational grammars through the 1960s.

6.2 Fieldwork and Missionary Work

> About half of this [fieldwork] was spent in the chief settlement which is named Jeberos, and the other half at the jungle base of the Summer Institute of Linguistics where work continued with an informant. In the course of this stay in Jeberos it was possible to record just over a hundred texts, mostly stories, from some ten persons. The analysis of these texts and the subsequent discussion of points arising during the analysis, which was possible while living in the village, proved particularly helpful and made it possible for a reasonably full understanding of the language to be gained in what was a comparatively brief period.

> John Bendor-Samuel, in *The Structure and Function of the Verbal Piece in the Jebero Language* (1958)[85]

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[85] John Bendor-Samuel, *The Structure and Function of the Verbal Piece in the Jebero Language* (Ph. D. diss.), Uni-

From the early 1800s to the mid-1900s, fieldwork and missionary work – at home and abroad – were central to American linguistics. In the years following the War of 1812, as Americans headed west, their encounters and interactions with Amerindians increased significantly. Beginning with John Pickering's 1823 study of the Cherokee language, assisted by an informant known as David Brown, investigations of Amerindian languages became a source of curiosity and intellectual interest. Wrapped up in colonial, expansionist, and scientific baggage, these investigations lay the groundwork for decades of American-led fieldwork, building up to Franz Boas' famous 1899 work on linguistic perception and observer bias in Inuit languages.[86] Fieldwork on Amerindian languages maintained its importance through the first half of the 20th century, when it went hand-in-hand with apprenticeship into the linguistics profession. Further, the foundation of the Christian organization SIL International in 1934, followed in 1942 by the Wycliffe Bible Translators, marked the beginning of intensive and influential efforts to apply American linguistics to missionary, Bible translation, and proselytization pursuits. In its first quarter century, SIL International trained over 4500 missionaries and related workers in linguistic theory, and has consistently been the foremost producer of descriptive language studies in America through the 20th century.[87] In the years of interest to this study – 1957 to 1970 – theoretical linguistics supplanted fieldwork and missionary work as the mainstay of American linguistics. Still, these pursuits by no means disappeared, and they continued to influence training, publishing, and theory through this period. This section explores the interaction between the syntactic theory-choice debates of the 1960s and American fieldwork and missionary work. I show that, in these areas, constituency theory maintained a dominant presence, valued for its simplicity and ease-of-use under difficult conditions. I also argue that the field and missionary contexts provided little stimulation for the rise of transformational theory on the academic scene.

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versity of London, 1958.

[86]I do not purport to offer a full account of fieldwork in early American linguistics; the reader is referred to Andresen, *op. cit.* for detail.

[87]Moulton, *op. cit.*, p 107.

Fieldwork refers to the elicitation, recording, transcription, description, and initial analysis of a language undertaken among a community of native speakers. It is typically concerned with endangered, previously-unstudied, and structurally-exotic languages in geographically or culturally remote locations, but also includes the study of little-understood languages in less remote situations.[88] Examples of the first include Boas' late 19th century study of Inuit languages on Canada's Baffin Island and sıl linguist Kenneth Pike's 1930s work on the Mixtec language of southern Mexico; examples of the second include William Labov's 1960s study of African American Vernacular English in inner-city America. Missionary work refers to the use of linguistics methodology and theory for, among other pursuits, making contact and entering into communication with communities targeted for proselytization, the creation of alphabets and written forms for previously-oral languages, and Bible translation. Fieldwork is often the first step in missionary work and, indeed, sıl International has undertaken fieldwork for over 2000 languages in locations from Cameroon to Peru to Papua New Guinea. This work has resulted in over 20,000 publications and articles ranging from technical linguistic studies to literacy books to cultural investigations.[89]

Linguistic fieldworkers – whether they identify themselves as linguists, missionaries, anthropologists, or elsewise – require a theoretical and analytical basis for their work. This can be as simple as the adoption of the International Phonetic Alphabet (IPA) for transcribing instances of the language under study, or as complex as the full use of transformational or stratificational grammar for language description and analysis. "[F]aced with numerous languages of which they were not native speakers", the University of Wisconsin-Madison's John Street wrote in 1969, American fieldworkers of the early to mid 20th century were "forced to seek some sort of nonintuitive (or at least less than fully intuitive) principles

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[88] An *endangered language* is a language which has a high risk of losing all native speakers. The UNESCO (United Nations Educational, Scientific, and Cultural Organization) Interactive Atlas of the World's Languages in Danger provides formal criteria for assessing the vitality of languages, characterizing languages at risk in five categories: *unsafe*, *definitely endangered*, *severely endangered*, *critically endangered*, and *extinct*. As of early 2009, the first four of these categories encompassed over 1300 languages. Christopher Moseley, UNESCO Interactive Atlas of the World's Languages.

[89] The complete citations, and many links, for these publications and articles are available through Raymond G. Gordon, SIL Bibliography (Ethnologue: Languages of the world).

by which they could discover and describe at least the general outlines of syntactic structures".[90] The principles they chose were those of immediate constituency grammar. In the 1940s and 1950s, constituency analysis was a key tool for the fieldworker: it allowed for the relatively quick and easy basic analysis of sentences, and it was the expected method of presentation of fieldwork results through those decades. In the 1960s, as constituency theory was losing ground to transformational theory in academic linguistics, it retained its utility and value in the field.

The chief advantage of constituency analysis, proponents argued, was its utility in the development of initial analyses for unknown languages. It is, Street wrote, particularly valuable for "the student who is tackling his first syntactic analysis of a corpus of utterances in a little-known language".[91] Compared to its rivals of the 1960s – transformational and stratificational grammar – constituency theory had a simple and easy-to-use theoretical framework. With its focus on surface-level structure, fieldworkers could use constituency theory to table a preliminary analysis of a language without confronting the complexities demanded by a multilevel syntactic theory. Sentences could be broken into constituent parts, features such as *subject-object-verb* order and verb structure determined, and agreement and tense analyzed, all the while avoiding the need to posit transformational rules or stratificational networks. Advocates of constituency grammar in linguistic fieldwork identified three key benefits of the theory: it was *easy to use* in the field, requiring little theoretical manipulation and little guesswork; it made for *fast reporting* of results, since it required the analysis of only a single level of syntactic structure; and its simplicity meant that it was unlikely to cause the fieldworker to impose theory-laden assumptions on the language under study. Through the 1960s, constituency theory was used in descriptions and analyses of, among other languages, Vietnamese, Navajo, and Old English.[92] Most often, the use of constituency grammar was underpinned by what I termed in Chapter 2 as

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[90]Street, *op. cit.*, p 89.

[91]*Idem*, p 109.

[92]Laurence C. Thompson, "Nuclear Models in Vietnamese Immediate-Constituent Analysis," *Language* 41/4 (1965), Herbert Landar, "Syntactic Patterns in Navaho and Huichol," *International Journal of American Linguistics* 33/2 (1967), Robert Howren, "The Generation of Old English Weak Verbs," *Language* 43/3 (1967).

the *weak ICG argument*.

The weak ICG argument characterizes those linguists, fieldworkers, and missionaries who maintained that while constituency theory was not necessarily suitable as a broad syntactic theory, it had particular advantages which made it valuable and, indeed, irreplaceable, in certain restricted situations – namely, fieldwork and initial language analysis. In its specific form, this argument states that constituency grammar is inferior to transformational and stratificational grammars in the academic context (that is, in providing an explanatory account of natural language), but is superior to its rivals for fieldwork applications. Street's support for this argument was matched by Yale University's Herbert J. Landar, who chose to use a "traditional non-generative immediate constituent grammar" in his mid-1960s study of Navajo syntax, arguing that despite the technical weaknesses of constituency theory, there are "descriptive and typological reasons for [its] continued study".[93] While transformational grammar "perhaps yields a deeper understanding of the multifarious aspects of language behavior than any other theory", Street wrote, it "is not recommended as the basic frame of reference for the first grammar of a little-known language".[94] The theoretical complexity of transformational grammar, proponents of the weak ICG argument continued, is in fact detrimental in the fieldwork context: if transformational theory "is used from the very start, an analyst is liable to be constantly twisting the language's arm and forcing upon it the structure of his own native language".[95] Despite its inferiority as a broad syntactic theory, they concluded, constituency grammar offered fieldworkers a descriptive and analytic tool which was easy to use and unlikely to distort the language in the early stages of analysis. The weak ICG argument was not limited to fieldwork and missionary work, but also extended to other areas of lay linguistics, including machine translation (cf. section 6.3).

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[93] Kalon Kelley, "Review of Herbert Jay Landar's Navaho Syntax," *International Journal of American Linguistics* 32/3 (1966), p 291, Landar, *Navaho and Huichol*, p 127.
[94] Street, *op. cit.*, p 111.
[95] *Idem.*

That transformational grammar was inappropriate for, and even detrimental to, field-work was emphasized by Robert Hall in his 1964 *Introductory Linguistics*.[96] For Hall, the theory-ladenness of transformational grammar makes it unsuitable for initial language analysis: transformational theory is not, he argued, "a more complete or powerful technique for linguistic description than previous types of analysis [that is, than constituency theory]"; rather, he continued, it is most useful for post-descriptive, or secondary, language analysis.[97] He saw transformational grammar as valuable "for making the equivalent of a guidebook, with instructions as to which path to follow at each point. Such a set of instructions presumes, however, that the territory to be covered is already known and has already been mapped; it contributes the relationships that exist among the features of the territory".[98] The implied division of labor assigns initial language description and analysis to a theoretically-simple constituency-type grammar, and only allows transformational grammar to enter the picture in the advanced stages of analysis (that is, in the identification of sentence relationships and the specification of complex syntactic constructions). The overarching worry is that the premature use of transformations will result in the introduction of structure-altering rules with little or no empirical justification. "At its worst", Hall concluded, transformational grammar "can degenerate into an arid, artificial game of inventing rules for constructing series of abstract formulae, pure 'hocus-pocus', with little necessary relation to the facts of language as it is spoken and as a functioning aspect of the behavior of humans living and interacting in society".[99] Hall's concerns about the theory-ladenness of transformational grammar emphasize a key premise of the weak ICG argument: that the relative simplicity of constituency analysis allows little room for the fieldworker to impose theory-motivated constructions on a language in the initial stages of analysis. The weak ICG argument sustained constituency theory on the lay scene through the 1960s, even as it was rapidly losing ground on the academic scene.

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[96] Hall Jr, *Introductory Linguistics*.
[97] *Idem*, p 226–227.
[98] *Idem*.
[99] *Idem*.

The most prolific American fieldwork and missionary organization of the 1960s – and indeed of the 20th century – was SIL International, in conjunction with its sister organization, the Wycliffe Bible Translators.[100] In that decade, SIL trained hundreds of students in linguistics at summer programs hosted at universities around the United States. These students were, for the most part, furloughed missionaries or missionaries-in-training preparing to work with language communities with little-to-no literate capacity. Their need for linguistic tools and theory clearly shows the link between fieldwork and missionary work: SIL students needed to be able to record and analyze languages quickly and efficiently in the field, to foster literacy in communities, to apply syntactic methodology to translation tasks, and to develop written forms for otherwise-oral languages as a first step towards Bible translation. By looking at what these missionaries were taught, and how they applied their linguistic knowledge in the field, we can gain an important understanding of syntactic theory in the lay context.

SIL ran linguistics training programs at four main campuses in the 1960s: Washington, Oklahoma, Texas, and North Dakota. These summer schools varied from nine to sixteen weeks, and included both introductory programs (equivalent to upper undergraduate and beginning graduate work) and advanced programs (equivalent to graduate work) as well as specialized courses designed for specific student needs.[101] Through the courses, students could earn course-hour credits at a selection of participating universities. The introductory courses presented "the rudiments of descriptive linguistics and provide[d] extensive drill in analytical techniques" in areas ranging from phonetics to phonology to syntax, and from anthropology to ethics to translation techniques.[102] Instructors included university profes-

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[100] In this study, I consider SIL International and the Wycliffe Bible Translators only in their linguistic-related capacities. For broader discussion, the reader is directed to Jonathan Benthall, "Summer Institute of Linguistics (Interview with William R. Merrifield)," *RAIN – Royal Anthropological Institute of Great Britain and Ireland* 53 (1982), Søren Hvalkof and Peter Aaby (eds.), *Is God an American? An anthropological perspective on the missionary work of the Summer Institute of Linguistics* (Copenhagen: International Workgroup for Indigenous Affairs (A Survival International Document), 1981), and Kenneth Pike, "A Linguistic Pilgrimage," in Konrad Koerner (ed.), *First Person Singular III: Autobiographies by North American scholars in the language sciences* (Amsterdam: John Benjamins Publishing Company, 1998).

[101] Frank E. Robbins, "Training in Linguistics," in Ruth M. Brend and Kenneth L. Pike (eds.), *The Summer Institute of Linguistics: Its works and contributions* (The Hague: Mouton, 1977), p 62.

[102] *Idem.*

sors, sil missionaries on furlough, and new sil linguists who had completed their theoretical training but not yet been sent into the field. The introductory courses culminated with a hands-on practice session in which students elicited data from a native speaker of an unfamiliar language and applied their newly-learned linguistic techniques to describe and analyze the language. The advanced courses – for sil linguists, a prerequisite for working in the field – aimed to provide students with "greater depth in linguistic theory and greater independence in analytical ability".[103] These courses centered on developing strong syntactic analytical ability, and included in-depth studies of a variety of linguistic theories. All told, sil training emphasized linguistic theory and methodology, as well as fieldwork, anthropological, and missionary methods.[104]

The syntactic theories taught and the linguistics tools emphasized in the sil advanced courses varied from campus to campus, year to year, and instructor to instructor. At the broad end of the spectrum, the Washington school offered a course titled *A Survey of Linguistic Theories*, which introduced students to a selection of linguistic theories including, among others, constituency grammar, stratificational grammar, transformational grammar, and tagmemics. (Developed by sil and University of Michigan linguist Kenneth Pike in the 1950s and 1960s, tagmemics was designed to efficiently and accurately extract linguistic descriptions from fieldwork data.)[105] The Oklahoma school emphasized stratificational grammar and tagmemics, while the North Dakota school focused on transformational grammar. "The aim is to leave room for development from the perspective of any linguistic theory in the hope that cross-fertilization will ultimately enable capitalizing on the strength of all of them", wrote Cornell University and sil linguist Frank E. Robbins, and, to that end, "the specialization in a given school should not be so narrowly exclusive in theoretical orientation as to exclude students from other backgrounds".[106] Akin to

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[103]*Idem*, p 63.

[104]Today, sil International offers academic training programs in linguistic fieldwork, literacy, and translation in 17 countries from Brazil to New Zealand to the Central African Republic, most often in collaboration with a local host university or institute. Chip Sanders, SIL Academic Training.

[105]The interested reader is directed to Kenneth Pike, *Language in Relation to A Unified Theory of the Structure of Human Behavior* (The Hague: Mouton, 1967).

[106]Robbins, *op. cit.*, p 63.

the language teachers who mixed-and-matched syntactic theories to suit their pedagogical needs, sɪʟ training encouraged students to view linguistic theories not as individual and incompatible bodies of knowledge, but as sets of tools to be selected and combined as necessary for fieldwork and missionary purposes. While Pike, the developer of tagmemics, was president of sɪʟ International from 1942 to 1979 and exerted an enormous influence on the organization, it is telling that his linguistic theory received little preferential treatment in sɪʟ training courses. "[T]he concern [of sɪʟ]", wrote Allene Grognet and Judith Brown in their 1970 technical report on university resources for the study of linguistics in North America, "is primarily with immediate practical implications [...] rather than pure theory".[107] This distinction between linguistic theories and tools, and the emphasis on practical utility in the field, would be central to the divide between academic linguistics and fieldwork in the 1960s.

Through the 1960s, sɪʟ linguists published descriptions and analyses of scores of previously little-studied languages, from the Ato Manobo language of the Philippines to the Gahuku language of Papua New Guinea to the Bahnar language of Vietnam. These studies were published in mainstream American linguistics journals (including *Language*, *Word*, and *The International Journal of American Linguistics*), as well as in country- and culture-specific journals (for example, *Mon-Khmer Studies* and *Philippine Studies*) and in unpublished sɪʟ working papers, and were presented at linguistics conferences from the Georgetown Round Table Meetings to the Linguistic Society of America Linguistic Institutes. As expected from the varied training of sɪʟ linguists, these studies reveal no singular preference for any particular linguistic theory. The choice of theoretical apparatus and tools depends on the training and personal preference of the linguist, as well as his intended task. These studies can be classified into four types of interest. The first are studies which use immediate constituency principles to investigate syntax and related elements of the grammar. These are predominantly basic descriptive studies of languages which had been subject to little or no previous work, and analysis is generally limited to the identification of

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[107] Grognet and Brown, *University Resources for the Study of Linguistics*, p 191.

structures and patterns. A particularly interesting example is John Bendor-Samuel's study of Jebero, a South American Indian language spoken by approximately 1500 speakers concentrated around the Jeberos village in northeastern Peru.[108] The result of 11 months of fieldwork in Jeberos and at sɪʟ's "jungle base", Bendor-Samuel's study presents a comprehensive constituency-based analysis of the phonology, verbal structure, nominal structure, adverb structure, and basic sentence structure of Jebero.[109]

The second class of studies uses stratificational theory. In line with the intentions of that theory, these are primarily investigations of supra-sentence phenomena. Mildred Larson and Margaret Sheffler, for example, use network diagrams to investigate discourse structure, explicitly showing "the relationships which exist in a discourse between participants and actions, between actions, and between places" for, respectively, translation purposes and the South American Indian Munduruku language.[110] The use of stratificational theory was strongly supported by sɪʟ-affiliated linguists Leonard Newell and Ilah Fleming, who straddled the academic and lay scenes in the 1960s and worked to highlight the advantages of the theory for linguistic analysis above the level of the sentence.

Third are studies which use transformational grammar. Influenced by the success of the theory on the academic scene, sɪʟ linguists increasingly turned to transformational grammar in the mid to late 1960s, but it never dominated sɪʟ fieldwork or missionary work. In the late 1960s and early 1970s, sɪʟ linguists applied transformational theory to the Mixteco language of Mexico (John Daly), the Kunjen language of Australia (Bruce Sommer), and the Busa language of Nigeria (Klaus Wedekind), among others.[111]

The final, and largest, class of studies uses combinations of various linguistic theories. With the particular challenges of fieldwork in mind, these approaches were designed to select advantageous tools from a variety of linguistic theories and recombine them to create descriptive and analytic procedures amenable to the contexts faced by sɪʟ linguists. Virginia

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[108]Bendor-Samuel, *Jebero*.

[109]*Idem*, p 4.

[110]Rensch, *op. cit.*, p 95.

[111]*Idem*, p 96, John Daly, *Generative Syntax of Mixteco* (Ph. D. diss.), Indiana University, 1966, Bruce Sommer, *Kunjen Syntax: A generative view* (Ph. D. diss.), University of Hawaii, 1970.

Austin's study of Ata Manobo and Ellis Deibler's study of Gahuku, for example, combined principles of stratificational grammar with insights from tagmemics.[112] Combinations of transformational grammar and tagmemics were used by Robert Longacre, and other combinations by Elizabeth Banker (Bahnar), Thomas Bearth (the Toura language of the Ivory Coast), and David Thompson (English).[113] Akin to language teachers, these fieldworkers valued not any single syntactic theory, but rather a range of tools offered by a variety of theoretical approaches. This wide display of theory use, combined with the lack of debate over theory choice, shows that sil fieldworkers and missionaries drew a strong line between the academic theory-choice debates and their need for practical, applicable linguistic tools to use in the field. Although many were well aware of the theory-choice debates taking place on the academic scene and although they studied on American university campuses, the vast majority of sil linguists never entered these debates.

A second organization offering linguistics training to missionaries in the 1960s was the Hartford Seminary Foundation. Based in Hartford, Connecticut, the Seminary Foundation provided "pre-field preparation for missionaries who had completed their professional training, and post-graduate work for missionaries in culture, religion, and related subjects".[114] During H.A. Gleason's tenure at the Foundation, from 1949 to 1967, the chief "related subject" was linguistics, and students could undertake masters and doctoral programs in the discipline. With a student body consisting of approximately equal numbers of young missionary candidates preparing for their first field assignment and experienced missionaries on furlough, the Hartford Seminary Foundation faced a situation very similar to that of sil International. The Foundation taught a wide range of courses in linguistics and language studies, including applied phonetics, translation, second language learning, literacy education, and syntax. Compared to sil, however, it was a small operation, offering courses in one location and with limited faculty.

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[112]Rensch, *op. cit.*, p 96.

[113]Robert E. Longacre, "Transformational Parameters in Tagmemic Field Structures," in *Report of the Sixteenth Annual Round Table Meeting on Linguistics and Language Study, 1965*, edited by Charles W. Kreidler, Rensch, *op. cit.*, p 96–97.

[114]Gleason Jr, *Theories in Conflict*, p 128.

Through the 1960s, the Foundation's linguistics program was headed by Gleason, who was firmly committed to stratificational grammar. Rival syntactic theories – that is, constituency grammar and transformational grammar – were not suitable for missionary work, Gleason argued, because the scope of such work frequently superseded the level of the sentence. Bible translation and communication – chief activities of missionaries – require an approach to language which "deals with texts, not isolated sentences", he wrote, and which is interested primarily in "the structure of discourses, [and] only secondarily of sentences".[115] For Gleason, only stratificational grammar was capable of handling supra-sentence phenomena, and was hence the only syntactic theory of utility for missionaries. The version of stratificational theory developed at the Hartford Seminary Foundation through the 1960s was a variation on Sydney Lamb's work, designed specifically for missionary needs: it placed minimal emphasis on sentence structure, focusing on larger units of discourse and aiming to "encompass whole texts".[116]

As we have seen throughout this study, Gleason was a key figure in the academic theory-choice debates of the 1960s, both during his time at the Hartford Seminary Foundation and at the end of the decade, when he moved to the University of Toronto. His involvement in fieldwork and missionary linguistics as well as the academic debates makes him one of the few linguists to straddle this divide. The vast majority of transformational grammarians, in contrast, had no interest in fieldwork or in the description of little-known languages. While they certainly recognized the importance of this work, they restricted their focus to tasks immediately connected to the development of their theory – that is, designing transformational solutions for problematic syntactic constructions and improving the design of the theory as a whole (cf. Chapter 3). The transformational community, Emmon Bach wrote in 1964, had little interest in and did not partake in "the urgent task of setting down data on the vast number of undescribed languages (many fast disappearing)".[117] Decades later, in a 2008 interview, he looked back on this collective decision with some regret, noting that

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[115]*Idem*, p 129.
[116]*Idem*, p 132.
[117]Bach, *An Introduction to Transformational Grammars*, p 187.

the steep decline in fieldwork from the 1960s on had led to a loss of cultural capacity and knowledge.[118] The disregard of transformationalists for fieldwork in the 1960s meant that they paid little attention to the publications of SIL and Hartford Seminary Foundation linguists – and, in turn, that these publications had little influence on their view of syntactic theory.

While SIL trained hundreds of fieldworkers and missionaries in linguistic theories other than transformational grammar through the 1960s, and while the Hartford Seminary Foundation trained scores to use stratificational grammar in the same decade, the linguists who graduated from this training had little impact on the syntactic theory-choice debates or on the American academic linguistics scene more broadly. Many of them left the United States promptly after their training to do fieldwork in remote communities – work which frequently spanned several years and allowed little communication with practitioners in America. Speaking about tagmemics, but making an argument which could equally well be applied to constituency and stratificational grammars, Robert Di Pietro asserted that the theory "has not had the impact on the thinking of the profession that T grammar [transformational grammar] has had", but that it is "nonetheless used to great advantage by missionary linguists who would certainly defend the social relevance of what they are doing".[119] Indeed, while fieldworkers and missionaries saw deep social relevance in their linguistics work, this relevance was far removed from the context of academic linguistics. Even as constituency and stratificational grammars were valued by fieldworkers, this did not boost their image on the academic scene. The intense focus on theoretical apparatus which followed the introduction of transformational grammar in the late 1950s left little room for that other staple of linguistics work – fieldwork and the recording, description, and analysis of languages.

The divide between the academic context and the fieldwork and missionary context is best understood in terms of the distinction between linguistic *theories* and linguistic *tools*.

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[118]Bach, *Interview*. In recent years, Bach has begun conducting his own fieldwork with the Amerindian languages of British Columbia and advocating language rights. These works are mostly unpublished, but can be accessed on Bach's website: Emmon Bach, Recent and Not So Recent Papers and Other Documents.

[119]Di Pietro, *The Need to be Practical*, p 47.

As with language teaching, fieldworkers by and large rejected the clearly defined syntactic theories of the academic scene – theories which incorporated syntactic tools, methodologies, and philosophies, and which were considered distinct and incompatible – and instead looked at syntax as a set of tools for language description and analysis. Constituency grammar, stratificational grammar, and transformational grammar – rivals on the academic scene – were for SIL linguists different elements of "the linguist's toolbox".[120] From the point of view of linguistic theories, we can say that these linguists valued constituency theory for its ease-of-use and theoretical simplicity in the description and initial analysis of previously-unknown languages; stratificational grammar for its amenability to supra-sentence phenomena; and transformational grammar for its ability to identify relationships between sentence types. From the point of view of tools, however, it becomes clear that these linguists put together a collection of syntactic tools which enabled them to conduct basic structural analyses of sentences quickly (by dividing sentences into constituent parts), relate sentences to discourse and metaphor (by capitalizing on the diffuse structure of network diagrams), and capture complex broad syntactic patterns (by introducing deep and surface structures).

As exemplified by the willingness of fieldworkers to combine and mix the analytical tools provided by various syntactic theories, they saw broader manifestations of these theories (that is, philosophical and methodological commitments) as baggage surrounding a core set of tools. The intense debates on the academic scene about simplicity metrics and psychological validity, for example, held little interest for fieldworkers and, vice versa, most academic linguists had no time for the slow and painstaking elicitation and description of previously-unstudied languages. As such, the use of syntactic tools by fieldworkers and missionaries had little influence on the academic scene and, in particular, on the syntactic theory-choice debates: first, while constituency and stratificational grammars enjoyed widespread application in these lay contexts, this did not increase their support on the academic scene and, second, while transformational rules were most often combined with

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[120]Landar, *Navaho and Huichol*, p 121.

tools from other syntactic theories, this had no detrimental effect on the rise of transformational grammar in academic linguistics.

6.3   Machine Translation

> Most of the people in the field supposed that MT [machine translation] was either a field of engineering, thus calling for workers trained in engineering, perhaps electrical, or a branch of computer programming, thus calling for skilful programmers, or an application of mathematics, hence requiring mathematicians. [...] While I recognized the interdisciplinary nature of the field, my thinking was different. I tried to explain it to my colleagues at one conference by alluding to an old Chinese recipe for tiger stew, which begins, 'First, you must catch the tiger'. My point was that if you want to construct a recipe for translating from one language to another, first you must understand the structures of the languages, and to do that, you must first understand some basic principles of linguistic structure.
>
> Sydney Lamb, in *Translation and the Structure of Language* (2000)[121]

"There is perhaps no other scientific enterprise in which so much money has been spent for so little return", wrote W. John Hutchins in a 1979 retrospective on machine translation.[122] In the early years of the Cold War, the American government poured money into machine translation in an effort to boost America's strategic capacity through the development of automatic Russian-to-English translation procedures. By 1965, this support had reached to 20 million dollars – support which, on the basis of the perceived failure of early machine translation work, has since come to be seen as miscalculated and misspent. While the American machine translation efforts of the 1960s are widely considered to have failed, they were cutting-edge and exciting at the time. As a key application of linguistic theory in that decade, machine translation offers a third important facet of lay linguistics for this

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[121]Lamb, *Translation and the Structure of Language*, p 177.
[122]W. John Hutchins, "Linguistic Models in Machine Translation," *University of East Anglia Papers in Linguistics* 9 (1979), p 31.

study. This section explores the relationship between the academic syntactic theory-choice debate of the 1960s and machine translation.[123] I show that while transformational grammar initially seemed to hold great promise for machine translation, it was quickly deemed inadequate. Constituency and stratificational grammars enjoyed more success on the machine translation scene, but this success did not translate into the academic context.

The earliest approaches to machine translation in the post-World War II years involved no syntactic theory at all. Known as *direct translation*, these approaches used "*ad hoc* [methods with] no notion of grammatical rule or syntactic structure".[124] Explicitly designed for targeted language pairs (for example, Russian and English, or German and English), direct translation limited syntactic analysis to the identification of word classes (nouns, verbs, adjectives, etc.) and the preliminary specification of homographs (that is, words with multiple possible connotations, such as *content*, which can refer to *the content of the shipment* or *the feeling of being content*).[125] Through the 1960s, as syntactic theories rose in prominence on the academic linguistic scene, machine translators began to incorporate them into their work. "Almost from the very beginning of [the machine translation] effort", wrote MIT's Victor Yngve, "it was evident that the linguistic difficulties standing in the way of the development of translating machines would be more serious than the technical computer difficulties" – linguistic difficulties which necessitated a deeper understanding of syntactic structure. Speaking at the 1960 National Symposium on Machine Translation, Georgetown University's Paul Garvin concurred: "the major purpose of a syntax routine in machine translation", he told his audience, "is to recognize and appropriately record the boundaries and functions of the various components of the sentences. This syntactic information is not only essential for the efficient solution of the problem of word order for the output, but is equally indispensable for the proper recognition of the determiners for multiple-

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[123]I do not purport to provide a detailed history of machine translation in America. Such a history is outside of the scope of this study, and the interested reader is directed to the extensive work of W. John Hutchins, including W. John Hutchins, *Machine Translation: Past, present and future* (Ellis Horwood, 1986), Hutchins, *Linguistic Models*, and W. John Hutchins (ed.), "The First Decades of Machine Translation: Overview, chronology, sources," in W. John Hutchins (ed.), *Early Years in Machine Translation: Memories and biographies of pioneers* (Amsterdam: John Benjamins Publishing Company, 2000).

[124]Hutchins, *Linguistic Models*, p 32.

[125]For details of direct translation methods, see *Idem*, p 31.

meaning choices. It is further becoming increasingly apparent in the work in which I am participating [that is, machine translation] that it is the design of the syntax routine which governs the over-all layout of a good machine translation program and lends it the unity without which it would remain a patchwork of individual subroutines and piecemeal instructions".[126] Soon, machine translation groups at universities and private companies across the United States were asking what the three main syntactic theories of the 1960s – constituency grammar, transformational grammar, and stratificational grammar – could offer them.

Even before the publication of Chomsky's *Syntactic Structures* in 1957, transformations had been identified as potentially valuable for translation. "[S]ince languages are much simpler in their kernel sentences than in their full sentences", wrote Zellig Harris in 1956, "translation may be procedurally simplified if the material to be translated is first reduced to kernel sentences, and then the kernel sentences are translated".[127] This endorsement captures the key idea behind transformation-based machine translation: since kernel sentences (later, deep structures) were seen to be structurally simpler and semantically similar counterparts of full sentences (later, surface structures), there was great hope that transformational grammar would allow the problem of machine translation to be reduced to that of the translation of kernel sentences. The translation procedure envisioned involved three steps: first, sentences from the input text would be reduced to their kernel forms; second, these kernel forms would be translated into kernel forms for the output language; and, third, the kernel forms for the output language would be expanded into full sentences. Importantly, this procedure offered a structure-based alternative to direct translation approaches, which relied on word-for-word procedures.

Initially, excitement over the use of transformational theory for machine translation ran high. "Chomsky's conception of grammar may prove to be of the utmost importance [in] the field of machine translation", wrote Robert Lees in his 1957 review of *Syntactic Struc-*

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[126]Paul L. Garvin (ed.), "Syntactic Retrieval: A first approximation to operational machine translation," in Paul L. Garvin (ed.), *On Machine Translation (Selected Papers)* (The Hague: Mouton, 1972), p 83.

[127]Harris, *Introduction to Transformations*, p 388.

tures.[128] The reduction of full sentences to kernel sentences, he continued, "may circumvent the discouraging problems involved in any scheme which attempts merely to render more sophisticated an essentially word-for-word type of translation system".[129] Working at the ᴍɪᴛ machine translation group in the early 1960s, Yehoshua Bar-Hillel described transformational theory as "an important new, not to say revolutionary, view of the structure of language [which could perhaps] be turned into a new method of machine translation, which would be more complex than known ones but also more effective".[130] Transformations, he continued, are "incomparably more efficient" than approaches which did not allow for significant simplification of full sentences. Chomsky's early work, including *The Logical Structure of Linguistic Theory*, struck the ᴍɪᴛ machine translators as full of "notations [which] seemed to resemble a computer program", generating anticipation that the theory would be easily computerizable.[131] Transformational grammar provided an "obvious model" for machine translation researchers and, as early as 1962, several machine translation groups were working with "detailed elaboration[s]" of the theory.[132]

Situated in the Research Laboratory of Electronics and headed by former physicist Victor Yngve, the ᴍɪᴛ machine translation group spent "considerable time" on transformational grammar in the late 1950s and early 1960s.[133] "We were [...] intrigued by the power of transformations and the promise they held out for an eventual approach to semantics", wrote Yngve in 1974 – but this initial enthusiasm soon turned to disappointment. "The problem in this instance was that the notation mirrored a theory of grammar in which the sentences of the language were generated all at once, as it were, whereas our recognition routine and construction routine would need to deal with sentences one at a time", Yn-

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[128]Lees, *Review of Chomsky*, p 406.

[129]*Idem.*

[130]Yehoshua Bar-Hillel, "Some Linguistic Obstacles to Machine Translation," in *Language and Information: Selected essays on their theory and application* (Reading MA: Addison-Wesley Publishing Company, Inc., 1964), p 76.

[131]Yngve, *Early Research*, p 53.

[132]Hutchins, *Linguistic Models*, p 33, Martin Joos (ed.), "Linguistic Prospects in the United States," in Christine Mohrmann, Alf Sommerfelt, and Joshua Whatmough (eds.), *Trends in European and American Linguistics, 1930–1960 (Edited on the occasion of the Ninth International Congress of Linguists, Cambridge MA, 27 August – 1 September 1962)* (Utrecht: Spectrum Publishers, 1970), p 11.

[133]Yngve, *Early Research*, p 56.

gve continued: "[i]t seemed a small difference, and we spent considerable time trying to modify the notation and theory so that it would serve our application and still retain the transformations. We were unsuccessful in the effort and reluctantly came to the conclusion that some of the most basic tenets of transformational grammar were incompatible with the needs of a mechanizable grammar of the sort we needed. The constraint that our grammars must specify actual real-time processes of sentence construction was unreconcilable with the type of linguistic abstraction embedded in Chomsky's theory of generative grammar".[134] In a 2000 retrospective, Yngve – by then a strong critic of Chomsky's theory – became more defensive: "[t]he failure of Chomsky's theory in this first test of it was a disappointment to us and it was not for lack of trying or any ineptitude on our part. [...] Our early discovery of [flaws] in generative theory was due entirely to the heuristic advantage of trying to program an actual computer model".[135] "I was perhaps one of the first persons to become excited about transformational grammar", he wrote, "and one of the first to become disillusioned with it".[136] The negative results of the MIT group reflected the American machine translation scene more broadly: at the University of Texas at Austin, a machine translation team headed by Winfred Lehmann applied transformational grammar to German-English translation in the early 1970s, with "clearly unsatisfactory" results, and other groups also discovered that approaches requiring reverse transformations are "inordinately complex", in large part because transformations eliminate structurally-significant information upon application.[137] This disappointment with transformational grammar led to a resurgence of constituency grammar in machine translation.

Having rejected transformational grammar as unsuitable for machine translation, Yngve and his colleagues at the Research Laboratory of Electronics turned to immediate constituency theory – the type of grammar "that had been especially well explored during the preceding twenty-five years by such men as Bloomfield, Wells, Nida, Fries, and Harris".[138]

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[134]Yngve, *The Dilemma of Contemporary Linguistics*, p 5.
[135]Yngve, *Early Research*, p 56.
[136]Yngve, *The Dilemma of Contemporary Linguistics*, p 4.
[137]Hutchins, *Linguistic Models*, p 34–35.
[138]Yngve, *The Dilemma of Contemporary Linguistics*, p 5.

Building on this body of work, the MIT group developed a computer model for translation which "produced grammatical sentences a word at a time in their proper order" as well as constituency-based "computer programs for generating actual sentences" for a variety of languages from German to Finnish to Arabic.[139] The chief challenge facing the group was modifying constituency grammars so as to be able to handle problematic syntactic constructions such as discontinuities. This work led to Gilbert Harman's development of a modified constituency theory, described in Chapter 3. "[I]n spite of its shortcomings", wrote Yngve, constituency theory "has the advantage of a certain simplicity and elegance, and it provides a framework for the description of many of the significant features of the language structure".[140] "Although Chomsky had argued that phrase-structure alone was inadequate for a linguistic theory", he concluded, "we were able to modify the phrase-structure notation and theory for our purposes and we did not in fact find it inadequate".[141] Yngve's and Harman's work also motivated Wayne Tosh's machine translation group at the University of Texas at Austin's Linguistic Research Center to devise a translation system incorporating modified constituency theory and a limited notion of transformations.[142]

Importantly, even as transformational grammar became the theory of choice among MIT linguists, the MIT machine translation group rejected transformational theory and committed themselves to constituency grammar. By 1963, Yngve and Harman had moved beyond the weak ICG argument and asserted that a modified constituency theory could match transformational grammar on all levels. Harman's work, described in Chapter 3, aimed to develop a constituency-based grammar which could handle all problematic syntactic constructions. Referring to Harman's *Generative Grammars Without Transformation Rules: A defense of phrase structure* (*Language*, 1963), Yngve wrote that Harman had "demolish[ed] Chomsky's complex arguments that phrase-structure grammars were inadequate and therefore transformations were required" – and that "[h]is report in *Language* should be

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[139]*Idem.*

[140]Victor H. Yngve, "A Model and a Hypothesis for Language Structure," *Proceedings of the American Philosophical Society* 104/5 (1960), p 444.

[141]Yngve, *The Dilemma of Contemporary Linguistics*, p 5.

[142]Smith, *Review of Tosh*, Tosh, *Syntactic Translation.*

read by everyone interested in formal grammatical theory and argumentation".[143] While
their constituency-based machine translation work was ultimately unsuccessful, it is telling
that even within the pro-TG MIT environment, constituency theory was still valued for its
particular amenability to computer applications.

The MIT and University of Texas at Austin machine translators were not the only groups
to work with modified constituency grammars in the 1960s. At Georgetown University – a
machine translation hub which in 1954 made headlines for a public demonstration of the
translation of Russian sentences into English – Paul Garvin's team used an " 'immediate-
constituent model', but with significant differences".[144] Born in Karlovy Vary, Czechoslo-
vakia, Garvin arrived in the United States in 1941 via Sweden. Immediately after the war, he
began doctoral studies at Indiana University under anthropological linguist C.F. Voegelin,
and was awarded the first Ph.D. from Indiana's new linguistics program. He soon joined
the Institute of Languages and Linguistics at Georgetown University's School of Foreign
Service and immersed himself in machine translation.[145] Trained primarily in European
linguistic thought, Garvin "could not stomach" the Chomskyan program and, correspond-
ingly, his machine translation work valued 'reverse' constituency grammars over transfor-
mational theory.[146] "Where the immediate-constituent approach takes the maximum unit
– the sentence – as its point of departure and considers its step-by-step breakdown into
components of an increasingly lower order of complexity", he said of his translation work,
"I prefer to start out with the minimum unit – the morpheme in straight linguistic analy-
sis, the typographical word in language-data processing – and consider its gradual fusion
into units of increasingly higher orders of complexity, which I call fused units".[147] This
reverse immediate constituent process was designed to identify the boundaries and func-

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[143]Yngve, *Early Research*, p 66.

[144]Garvin, *Syntactic Retrieval*, p 83, Press Release IBM, 701 Translator, IBM Archives (IBM 701), 1954, Earl
Ubell, "It's All Done by Machine: Words go in in Russian, English sentences come out," *New York Herald Tribune*
8 January (1954).

[145]Paul L. Garvin (ed.), "Audience with Five Decades of Linguistics: A conversation with Paul L. Garvin," in
Konrad Koerner (ed.), *First Person Singular II: Autobiographies by North American scholars in the language sciences*
(Amsterdam: John Benjamins Publishing Company, 1991), p 127.

[146]*Idem*, p 129.

[147]Garvin, *Syntactic Retrieval*, p 83–84.

tions of fused units of the input text. "Rather than attempting a consecutive left-to-right solution [to the problem of machine translation], the syntax routines conceived in terms of the [fused unit] approach have attacked it by a consecutive series of passes at the sentence, each pass designed to identify fused units of a particular order and type", Garvin told the 1960 National Symposium on Machine Translation.[148] "The advantage of this pass method over a single consecutive left-to-right search", he continued, "is that, instead of having to account for each of the many possibilities at each step of the left-to-right progression, every pass is limited to a particular syntactic retrieval operation and only information relevant to it has to be carried along during that particular search".[149] The first pass, for example, aims to identify the main clause, or predicate, of each sentence, and does so without also trying to capture subclauses and other grammatical information. Garvin's approach capitalizes on the hierarchical nature of constituency analysis, rebuilding sentences by means of their nested constituent structures and partitioning labor on the basis of constituent level.

Historian of machine translation W. John Hutchins has identified Georgetown University as one of the highlights in an otherwise bleak decade of effort. From Georgetown emerged the SYSTRAN translation system, first designed for Russian-to-English translation and later adapted for English-to-French and French-to-English translation. Adopted for this second language pair in the 1970s by the Commission of the European Communities, then intent on implementing official language policy, SYSTRAN was "the only MT [machine translation] system at present in full operation".[150] Akin to Garvin's fused unit system, SYSTRAN implemented constituency analysis by means of four consecutive passes for each sentence: the first resolved homographs; the second established primary phrase constituents (verb phrases, noun phrases, adjective phrases, etc.); the third identified objects and complements; and the fourth determined clause types, ranges, and final constituent structures.[151] The chief difference between SYSTRAN and Garvin's system was the addition of modularity in the computer program itself, which improved efficiency and debugging by separating

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[148] *Idem*, p 87.
[149] *Idem*.
[150] Hutchins, *Linguistic Models*, p 32.
[151] *Idem*.

the translation procedure into semi-autonomous modules.[152] Garvin's machine translation ideas were also adopted by the Bunker-Ramo Corporation, a computer and communications technology spinoff from the American defense and automotive giant TRW.[153]

Finally, a third stream of American machine translation work in the 1960s was based on Sydney Lamb's stratificational grammar. The development of stratificational theory, as described in Chapters 2 and 3, was intimately linked with machine translation from the outset. "Most of the people in the field supposed that MT [machine translation] was either a field of engineering, thus calling for workers trained in engineering, perhaps electrical, or branch of computer programming, thus calling for skillful programmers, or an application of mathematics, hence requiring mathematicians", wrote Lamb, but "while I recognized the interdisciplinary nature of the field, my thinking was different".[154] Indeed, of the three main syntactic theories of the 1960s, only stratificational grammar was developed hand-in-hand with machine translation work, and only in the stratificational school did the main contributors to academic work also contribute significantly to translation efforts. Along with Lamb, stratificationalists Peter Reich, Alexander Borgida, Adam Makkai, and Ilah Fleming all were involved with machine translation projects at Berkeley and/or at the Yale University Linguistic Automation Project. Importantly, stratificational grammarians saw machine translation and computerizability as explanatory criteria – a commitment which distinguishes them from the leading rival theory of the day. The implementation of translation procedures in stratificational grammar is described in detail in Chapter 3.

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The hierarchy of the three main syntactic theories of the 1960s was starkly different on the academic and machine translation scenes. Whereas transformational grammar dominated American academic linguistics, constituency and stratificational grammars were preferred by machine translators. Indeed, transformational grammar was categorically rejected by several prominent machine translation groups and deemed to be fundamentally unsuitable

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[152]*Idem.*
[153]Garvin, *Syntactic Retrieval*, p 89, note *.
[154]Lamb, *Translation and the Structure of Language*, p 177.

for translation procedures. Even as constituency theory was faring poorly in the academic context, it was picked up and reignited by machine translators. Critically, it offered an analytic basis capable of parsing syntactic structure and relatively easily implementable on computers. Stratificational grammar was also valued for its machine translation capabilities, and enjoyed National Science Foundation funding to pursue computer-based linguistic projects. The success of these two theories on the machine translation scene, however, did not translate to the academic scene. As described in detail in Chapter 3, transformational grammarians were dismissive of computer applications, and their emphasis on data as an explanatory criterion fostered in students a disregard for practical applications of linguistic theory.

The intimate relationship between machine translation and stratificational grammarians in the 1960s distinguishes machine translation from other areas of lay linguistics, where academic linguists had relatively little involvement. By straddling these two contexts, however, stratificationalists lost some of their ability to influence academic work. In particular, stratificational research published in machine translation journals was rarely circulated or read in the academic linguistics community. Indeed, work on machine translation – including work focused on the linguistic aspects of translation – was "seldom readily obtainable in linguistic journals".[155] By choosing a subject matter and a medium on the boundary of theoretical linguistics, stratificational grammarians effectively restricted their audience and, consequently, diminished their voice in academic linguistics.

Finally, with release of the Automatic Language Processing Advisory Committee (ALPAC) report in 1966, machine translation came to be regarded as "an expensive failure" and linguists who opted to continue pursuing translation research were "looked upon as eccentric and misguided (if not worse)" (cf. Chapter 3).[156] While some groups persevered with their work – chief among them, the Georgetown University team, the University of Texas at Austin team, and the stratificational grammarians – the optimism and excitement which

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[155]Mary Lu Joynes and W.P. Lehmann, "Linguistic Theories Underlying the Work of Various MT Groups," *Linguistics* 8 (1964), p 62.

[156]Hutchins, *Linguistic Models*, p 31.

had fueled and funded machine translation efforts through the 1950s and early 1960s were severely damaged. In the new environment, the decision of transformational grammarians to disregard machine translation as an explanatory criterion looked wise. The collapse of funding following the ALPAC report did not impact on transformationalists, and their reputation was bolstered by their distance from the report's repercussions. Together, the inability of stratificational grammarians to make machine translation matter on the explanatory arena, their position on the boundary of the academic scene, and the fallout from the ALPAC report meant that their machine translation efforts did not translate into success for the theory in academic linguistics.

<div align="center">* * *</div>

In his 1988 memoir, H.A. Gleason asserts that the interaction of transformational grammar with lay disciplines in the 1960s was responsible for "a considerable strengthening of the TG movement" and that "[w]idespread recognition by other disciplines was, perhaps, the most potent force in the establishment of TG as dominant in North American academic linguistics".[157] In this chapter, however, I have shown that lay linguistics had little influence on the syntactic theory-choice debates of the 1960s, and little influence on the rise of transformational grammar. In lay fields, the strong divisions which characterized rival syntactic theories on the academic circuit were blurred and inconsistent. Rather than committing themselves to a single syntactic theory, lay practitioners often assembled what they saw as the advantageous tools of several theories into a toolkit designed for their work. Language teachers frequently combined constituency and transformational tools, fieldworkers and missionaries mixed-and-matched tools from all three rival syntactic theories, and machine translators wedded constituency theory with transformational principles. Further, the lack of a uniform and consistent response to lay work on the academic circuit effectively drew a boundary between the two contexts. Importantly, transformational grammarians saw no advantage to promoting their theory on the lay scene, and ardent anti-transformationalists including Robert Hall and Archibald Hill saw no threat to dropping their rhetoric when

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[157]Gleason Jr, *Theories in Conflict*, p 71–72.

discussing language teaching. These departures from normal positions highlight the divide between the academic and lay fields. Together, the investigations of lay linguistics presented in this chapter show that lay contexts had little effect on the success of transformational grammar, and little effect on the relative decline of constituency and stratificational grammars, in American academic linguistics.

Driven by practicality and applicability, lay linguists were for the most part not interested in syntactic theories conceived broadly as tripartite bodies of knowledge including technical tools, philosophies, and methodologies, but rather focused on the tools *per se*. This distinction between linguistic theories and linguistic tools enables us to get at the core of the divide between the lay and academic linguistics scenes, and it strengthens our understanding of the weak ICG argument. By choosing to use constituency grammar for applications from fieldwork to machine translation while simultaneously maintaining that constituency grammar did not constitute a successful broad linguistic theory, proponents of the weak ICG argument were in fact selecting for the tools offered by constituency theory while rejecting the philosophical and methodological basis of this theory. More broadly, commitment to a single syntactic theory offered no advantage to most language teachers, fieldworkers, missionaries, and machine translators: rather, their interests were best served by adopting and adapting syntactic tools from a number of theories and honing those tools to meet their specific needs. The tool-based approach of lay linguists was enabled by their professional identity: in all but a few cases, lay linguists were far removed from the academic linguistics scene and had no stake in the syntactic theory-choice debates. They evaluated syntactic tools on the basis of their ability to effectively contribute to lay pursuits. In contrast to the academic context, where strong rivalries between theories underpinned divisions between communities, on the lay scene these rivalries were largely superseded by the need for effective and practical tools.

In the end, this chapter has shown that the lay scene had little influence on the rise of transformational grammar in American academic linguistics. This conclusion teaches us three lessons. First, it highlights areas in which constituency and stratificational grammars

maintained and even increased their importance through the 1960s. Even as they were losing ground on the academic scene, these two syntactic theories excelled over transformational theory in machine translation and fieldwork. By providing a different perspective on the relative value of these three theories, this investigation of lay linguistics counters the widely-held view that Chomsky's syntactic program established dominance on all fronts. Second, it deconstructs the theory commitments of the academic context and emphasizes the role of syntactic tools *per se*. While in the academic linguistics of the 1960s tools were rarely separated from their philosophical and methodological environments, on the lay scene these environments were stripped away and syntactic tools evaluated for their practicality and applicability. By assembling syntactic toolboxes from a variety of competing theories, lay linguists deliberately overruled theory boundaries and strove for advantages in language teaching, fieldwork, missionary work, and machine translation. Finally, this investigation of lay linguistics demonstrates that dominance in the academic and lay contexts do not necessarily go hand-in-hand. In doing so, it counters existing accounts of the success of transformational grammar and calls attention to the need for historians of science to step outside academic boundaries and view scientific work as diffuse and far-reaching.

7   Turtles All the Way Down

> As linguists looked closer and closer at syntax, they have found more and more
> and bigger and bigger turtles.
>
> Robert I. Binnick, Jerry Morgan, and Georgia Green in *Notes from the Linguis-*
> *tic Underground* (1968)[1]

It was supposed to have been easy: driven by the optimism and confidence built up from their work on phonology and morphology in the 1930s and 1940s, Descriptivist linguists began the 1950s with the belief that, by the end of the decade, they would have a firm grasp on the next level of linguistic structure: syntax. Bolstered by the success of American linguistics during World War II, they launched an intensive effort to apply immediate constituency theory to sentence-level phenomena. Just as quickly, however, these plans were dashed. Syntax proved far more difficult than anticipated: not only did each turtle lead to another, but the turtles kept growing increasingly complicated.[2] Faced with a multitude of problematic constructions, constituency grammar no longer looked able to provide a satisfactory understanding of syntax. By the end of the 1950s, two rival syntactic theories – stratificational grammar and transformational grammar – had staked out competing claims. From 1957 to 1970, these three syntactic theories competed for the attention of American linguists. While they all aimed to resolve fundamental questions about natural

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[1] Sir Lancelot of Benwick, Morgan le Fay and The Green Knight, "Camelot, 1968," in James D. McCawley (ed.), *Notes from the Linguistic Underground* (New York: Academic Press, 1976), p 259.

[2] In addition to Robert Binnick, Jerry Morgan, and Georgia Green's invocation of turtles, quoted at the beginning of this chapter, turtles were immortalized in American syntax in John Robert Ross' 1967 MIT Ph.D. thesis, *Constraints on Variables in Syntax*. In an introductory *fragestellung*, Ross recounts the story of William James, the little old lady, and the stack of turtles supporting the Earth, remarking that the turtle metaphor had "bull's-eye relevance to the study of syntax". Ross, *Constraints on Variables*, p iv.

language syntax, they developed drastically different technical tools, supported incompatible philosophical positions, and nurtured diverse socio-professional environments. In the end, transformational grammar gained the commitment of the majority of the American academic linguistics community. Constituency theory and stratificational theory were left with small followings, diminished influence, and few resources. This study has investigated the emergence, dynamics, competition, and eventual fates of these three syntactic theories.

One central puzzle runs through this study: how did a theory which operated in an underground culture – a theory in which research was deliberately kept out of mainstream journals and work narrowly circulated among a select group of insiders – spread across America and gain a majority of supporters? Through the 1960s, transformational grammarians valued private knowledge highly and outsiders found it difficult – sometimes impossible – to access transformational research. I have argued that this puzzle can only be understood by situating American linguistics in a confluence of four themes: explanation, pedagogy, knowledge transmission, and lay linguistics. Together, these themes allow us to understand how linguists selected and evaluated theories, how students were trained to think about and use syntax, how ideas and people spread across the country, and how academic ideas were interpreted on the lay scene. They also maximize our understanding of the syntactic theory-choice debates as dynamic and interactive events. Importantly, the three main syntactic theories of the 1960s did not exist in isolation: they interacted on a nearly daily basis, influencing and challenging each other through the decade. Finally, these arguments challenge the existing literature on the rise of transformational grammar. By not exploring the rival theories which competed with transformational grammar, by largely ignoring the technical linguistics debates of the decade, by failing to discuss pedagogy and training, and by not accounting for the prevalence and perpetuation of underground knowledge, the existing literature cannot solve the puzzle at the heart of this study. By paying particular attention to the ideas and problems which mattered to linguists of the era, I have presented a critical and novel account of American linguistics in the 1960s.

I began this study by posing four questions – questions which aimed to improve not only our understanding of American linguistics, but of theory-choice and theory-transmission in the history of science more broadly: How are explanatory criteria established and promoted within disciplinary communities? How can pedagogical and institutional needs influence theory diffusion and student training? How do cultures of private knowledge interact with the academic norms of open publishing and knowledge sharing? and, How does the relationship between a core discipline and peripheral manifestations affect theory choice? I now consider these questions in turn.

7.1 How are explanatory criteria established and promoted within disciplinary communities?

When it comes to understanding, one of the most basic questions underpinning scientific inquiry is *why?* When we ask *why?*, we are searching for an explanation. And, indeed, the concept of explanation played a central role in 1960s American linguistics. At stake are two questions: what counts as an explanation in linguistics?, and, how is this decided? As defined in this study, explanatory criteria are the conditions which, according to a given school of thought, any syntactic theory must meet in order to be capable of explaining sentence structure. The criteria proposed by proponents of the three rival syntactic theories of the 1960s ranged from accounting for the patterning of active-passive pairs to illuminating communicative intent to applicability to machine translation. It is on the strength and allure of these criteria that the theory-choice debates hinged: to convince an established linguist to change his theoretical commitment, or to convince a linguistics student to adopt a particular syntactic theory, that linguist first and foremost has to be persuaded by the value of the new theory's explanatory criteria and by the ability of the theory to fulfill those criteria. The decision to commit to a syntactic theory comes largely from the belief that the theory will eventually lead to satisfying and revealing explanations of syntactic structure, language, and related elements of the world at large – or, that the theory will be

able to answer our *why?* questions about language. The explanatory arena clearly plays a vital role in gaining and retaining supporters, and in recruiting linguistics students.

I have argued that transformational grammarians were successful at setting priorities for explanation in American syntax through the 1960s. Beginning with the publication of Noam Chomsky's *Syntactic Structures* in 1957, transformational theory raised compelling questions about language and set the stakes for syntactic explanation high. With their emphasis on the TG dataset, formalization, power, and psychological validity, Chomsky and his colleagues fundamentally altered the conception of explanation in linguistics. They did so by consistently keeping these ideas at the forefront of discussion and constantly challenging rivals to meet the TG standards. As this new conception of explanation gained momentum, rivals were forced to respond. Soon, stratificational and constituency grammarians were devoting as much time and effort, if not more, to showing that their theories fit the transformational explanatory criteria as they were to enunciating and advancing their own explanatory priorities. While stratificational grammarians worked to make computerizability and the explanation of supra-sentence phenomena mainstream concerns, they failed to raise interest outside of their own community. Further, while opponents of transformational grammar made inroads in the late 1960s in areas of simplicity metrics and syntactic power, and while they even forced transformational grammarians to reevaluate their basic assumptions, this was too late to block the tide. By delivering on their explanatory goals through the decade, transformationalists created a momentum of optimism and progress – a momentum which provided their own supporters with highly significant questions to pursue, which attracted the interest and attention of linguistics students, and which drained energy and resources away from rival theories. By successfully naming the conditions for explanation in 1960s syntax, transformational grammar took a large step towards establishing its dominance in the American academic linguistics community.

I have emphasized throughout this study that explanatory criteria are not the afterproducts of historians, philosophers, or other outside commentators, but the time-of-inquiry results of discipline-internal and community-internal priority setting. Explanatory

criteria are not set years or decades after the fact; they are set by the scientists of a discipline at the time of their work. These criteria may be – and frequently are – affected and shaped by outside forces from funding bodies to ethical norms to philosophical considerations, but these too are an element of the times. Too often in discussions of science we impose our own explanatory priorities – molded by hindsight and by our own interests and preferences – on the work of others. In doing so, we misrepresent the day-to-day concerns of working scientists and distort the priorities of an era. This has been especially prevalent in commentaries on and histories of 20th-century American linguistics, largely because those writing these works were and continue to be involved in the linguistics they write about – factors which make them prone to imposing current concerns on historical questions. This study has been careful to avoid imposing explanatory criteria on the syntactic theories of the 1960s. It is only, I have argued, by looking at what linguists themselves saw as important in terms of explanation that we can fully understand the role of explanatory criteria in theory-choice. In particular, I have not compared syntactic theories on empirical grounds; instead, I have investigated the relative ability of theories and theory communities to build support bases, recruit students, and ultimately secure a portion of the American linguistics market. By doing so, I have provided an historically-informed understanding of syntactic explanation in the 1960s which goes a long way towards characterizing the theory-choice debates of that decade.

7.2   How can pedagogical and institutional needs influence theory diffusion and student training?

In the late 1950s and early 1960s, American linguistics faced an immense challenge: stimulated by the success of linguists in the Second World War effort, linguistics departments sprang up at universities across the country and students flocked to study the discipline. Between 1960 and 1970, the number of American universities offering degrees or concentrations in linguistics increased more than fourfold; the number of linguistics Ph.D.s con-

ferred by American institutions more than tripled; and enrollment in doctoral programs grew by fivefold. Along with ballooning classrooms came a host of pedagogical problems: there were no set or standard courses in syntax, no syntax-specific textbooks, and few well-stocked library collections. The pedagogical and textbook markets were wide open – and the syntactic theory which capitalized on these markets would gain a significant advantage in the theory-choice debates. As linguists built a pedagogical framework for their discipline, they shaped the training and commitment of the next generation: what linguistics students were taught in the classroom was crucial to determining their theoretical commitments.

I have shown that transformational grammar emerged as the approach to syntax which was, from the pedagogical perspective, easiest to *teach*, *learn*, and *use*. By capturing the textbook market in the early 1960s, transformational grammarians captured a generation of young linguists, instilling in them a transformational worldview. Stratificational grammar, in contrast, did not produce a dedicated textbook until 1972 – too late to have a meaningful impact on training and theory commitment. Because of this asymmetrical presence on the textbook scene, linguistics students had far more access to introductory material on transformational theory, and faculty were far more likely to base a course on a theory with extensive and supportive learning materials. Transformational grammarians also excelled in the use of canonical examples. By providing a foothold to enter what was otherwise considered a difficult and complex theory, these examples enabled students to learn how to manipulate transformational rules without necessarily learning about the formal underpinnings of theory. For a generation of students, transformational rules became the normal way of approaching data and mediating between linguistic theory and syntactic phenomena. Finally, the notational techniques of transformational grammar made that theory particularly effective for both teaching and research. In contrast to the complex and distracting network diagrams at the heart of stratificational theory, transformational notation provided simple ways to display and manipulate syntactic patterns. Together, transformational grammar's dominance of the textbook market, use of canonical examples, and visually appealing no-

tation meant that the theory filled the needs of both students and teachers. Importantly, transformational theory provided a better pedagogical tool than its rivals at a time when linguistic pedagogy was of real concern. As a result, transformational grammar emerged as the most efficient mechanism for handling the rapid growth in enrollment in university linguistics programs of the 1960s.

The role of pedagogy, training, and institutional needs in theory transmission is fast becoming a significant topic of interest in the history of science. From David Kaiser's and Andrew Warwick's work on theoretical physics to the European Science Foundation's work on chemistry in the long 19th-century to Henderikus Stam and colleagues' work on social psychology, historians of science are finding immense value in studying the influence of textbooks and pedagogy on scientific knowledge.[3] This study adds to this body of work by exploring the relationship between pedagogy and theory transmission in a particularly unusual situation: one in which an underground culture flourished and the distribution of knowledge was highly restricted. In this environment, pedagogy – and especially textbook distribution – played a vital role in the spread of ideas. The prevalence of transformational grammar textbooks on the pedagogical scene allowed that theory to disperse across America despite the high value placed by transformationalists on private knowledge. I have shown, first, that institutional needs influenced theory diffusion and student training in 1960s linguistics by creating an open market for textbooks and pedagogical tools and, second, that the pedagogical scene was imperative to theory transmission in an otherwise underground culture.

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[3]Kaiser, op. cit., Warwick, op. cit., Lundgren and Bensaude-Vincent (eds.), op. cit., Henderikus Stam, Lorraine Radtke and Ian Lubek, "Strains in Experimental Social Psychology: A textual analysis of the development of experimentation in social psychology," *Journal for the History of the Behavioral Sciences* 36 (2000).

7.3 How do cultures of private knowledge interact with the academic norms of open publishing and knowledge-sharing?

Today, the idea of private knowledge in the academic world is highly unusual, curious, and even suspect.[4] For the transformational grammarians of the 1960s, however, private knowledge was valued and celebrated. It provided a sense of belonging and shared purpose; it acted as a mechanism to prevent and deflect criticism from outside; and it circumvented the unacceptable delays of traditional publication mechanisms. By deliberately maintaining a tight network of communicants and restricting access to new research, the transformational community fostered an insular culture which rejected what had come before and provided new support structures for students and researchers. The resultant underground culture defined a generation of linguists and their theory and was, undisputedly, one of the most colorful features of 1960s syntax. A central aim of this study has been to resolve the apparent paradox generated by the success of a private theory: in light of its underground culture, how did transformational grammar spread across the country and gain a majority of supporters in under a decade?

The solution to this paradox comes from a careful consideration of the intersection between private and public manifestations of knowledge. I have shown that transformational theory spread through the dispersion of transformational grammarians from MIT to newly-founded linguistics departments across the country and the subsequent formation of informal networks for knowledge transmission; the widespread distribution of aboveground textbooks; and oral transmission at conferences, colloquia, and Linguistic Society of America Linguistic Institutes. Together, these factors illuminate the mechanisms by which transformational knowledge escaped from, and was allowed to escape from, the underground scene. They also expose an important tension between private and public knowledge: while transformational grammarians valued the underground scene for the sense of group identity it provided and for the speed at which it circulated results, they also real-

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[4]A potential counterexample is knowledge produced in the academic context subject to publication restrictions from defense, government, and corporate funding sources. While this type of private knowledge was prevalent at MIT throughout the Cold War, it is separate from the linguistics work discussed in this study.

ized that the deliberate sharing of their theory with a public audience would be necessary for ensuring the perpetuation of that theory by the next generation of linguists. Indeed, the public face of transformational grammar in the 1960s was one specifically designed to maximize exposure of the theory to students. By opening their theory to the group most vital to idea perpetuation – linguistics students – and simultaneously restricting access to those committed to rival syntactic theories, transformationalists maintained a careful balance of private and public knowledge. The success of transformational theory cannot be understood without recognizing this balance and unraveling the specific role of each type of knowledge in the spread of syntactic ideas in the 1960s.

Open publishing and knowledge-sharing – enshrined in the thousands of peer-reviewed academic journals regularly published today – are the norm for 20th and 21st century science. The idea of private knowledge is most often associated with the alchemy of Newton's day, not modern science. As such, the underground culture of transformational grammar is particularly tricky for the historian to navigate. The defining characteristics of underground literature – unpublished, copied on thin paper, private, and circulated in small numbers – mean that this literature is now, nearly half a century later, difficult to locate and access. The historian's task is greatly eased by four factors: the publication of a selection of underground documents in the 1970s and 1980s, recent commentaries on underground culture by linguists active in the 1960s, the excellent archival collection at the University of Texas at Austin, and interviews with linguists focusing on private knowledge. However, it is particularly difficult to judge how much of this literature has not resurfaced, and hence how much material the historian is missing. This study has taken advantage of a multitude of information sources to build a comprehensive picture of the underground culture in American linguistics in the 1960s. Based on an in-depth examination of the period, I am confident that the documents I have been able to access, combined with commentaries, recollections, and interviews, provide rich and balanced insight into transformational grammar's underground culture.

7.4 How does the relationship between a core discipline and peripheral manifestations affect theory choice?

From the recent New York Times best-seller *Eats, Shoots & Leaves* to the controversy over the Nim Chimpsky experiments to the regular International Herald Tribune column on grammar, our society finds language fascinating and ever-debatable. Far from being restricted to the Ivory Tower, linguistics – the study of language – affects many aspects of our lives. In the 1960s, syntax spilled out into language teaching, fieldwork, missionary work, and machine translation. Elementary and high school pupils were taught to build complex sentences using transformational rules; fieldworkers and missionaries in locations from South America to Africa to Asia applied constituency and stratificational principles to the analysis of little-studied languages; and machine translators at university and corporate labs across America used all three rival syntactic theories in their efforts to develop automatic translation procedures. Given the widespread use of syntactic ideas in the lay context, no full investigation of the syntactic theory-choice debates of the 1960s can be limited to the academic world. Through that decade, hundreds of teachers and thousands of schoolchildren, hundreds of fieldworkers and missionaries in communities around the world, and scores of machine translators and their military patrons were affected by the tools and theories produced on the academic scene. A robust understanding of postwar syntax requires an understanding of both the effects of syntactic theories on the lay context and the implications of lay linguistics for the rise of transformational grammar.

The picture obtained from investigating lay linguistics is starkly different than that obtained from the academic scene alone. In lay fields, the strong divisions which existed between syntactic theories on the academic circuit were blurred and inconsistent. While the three rival syntactic theories of the 1960s all had their supporters in lay linguistics, actual practice in lay fields frequently involved the mixing-and-matching of ideas. I have shown that the divide between the lay and academic scenes is best understood in terms of the distinction between *linguistic theories* and *linguistic tools*. Driven by practicality and

applicability, lay linguists were not interested in syntactic theories conceived of broadly as bodies of knowledge including technical, philosophical, and methodological components, but rather focused on syntactic tools *per se*. Commitment to and defense of a single syntactic theory offered no advantage to most lay practitioners: their interests were best served by breaking theories down into useful parts and reassembling syntactic toolboxes designed to meet their specific needs. Further, the lack of a consistent response to lay work on the academic circuit – and, specifically, the abandonment of rhetoric by academic linguists when discussing the lay scene – drew a boundary between the two contexts. Finally, there was no correlation between the success of syntactic theories on the academic and lay scenes. Far from strengthening the position of transformational theory on the academic scene, lay linguistics had little effect on the success of that theory, and little effect on the relative decline of constituency and stratificational theories.

The thrust of this study has been to explain the rise of transformational grammar. The investigation of peripheral disciplines at first seemingly offers little insight into this phenomenon: lay linguistics, I have shown, had little influence on the outcome of the syntactic theory-choice debates in American academic linguistics. This conclusion, however, teaches us two important historiographic lessons. First, it shows that peripheral manifestations of academic work can highlight areas in which non-academically-dominant ideas flourish. Even as constituency and stratificational grammars were losing ground on the academic scene through the 1960s, they still excelled over transformational theory in various lay contexts. By providing an alternative perspective on the relative value of syntactic ideas, my investigation of lay linguistics counters the widely-held view that Chomsky's syntactic program established complete dominance and, in doing so, provides motivation for historians of science to break out of the academic sphere and view scientific work as diffuse and far-reaching. Second, by demonstrating that dominance in the academic and lay contexts do not necessarily go hand-in-hand, this conclusion challenges existing accounts of the success of transformational grammar. As such, it calls attention to the different priorities motivating theory choice and theory use in core disciplines and their peripheral

manifestations.

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Informed by history of science methodology, this study has challenged the existing literature on the history of linguistics. It has demonstrated the importance of a full investigation of competing syntactic theories; the explanatory contexts of technical apparatus and syntactic tools; the role of pedagogy and the training of linguistics students; the transmission of knowledge in public and private spheres; and the interaction between the lay and academic contexts. Together, my arguments explain *why* transformational grammar prevailed over alternative syntactic theories, and *how* transformational theory spread across America despite its pervasive underground culture. The story I have told answers fundamental questions about the rise of syntax in American linguistics and suggests powerful ways for investigating theory development, theory choice, and theory change. It also raises one final question concerning recent historiographic practices and the division between scientific theories and scientific tools.

7.5 Theories, Tools, and the History of Science

Recent work in the history of science has emphasized the investigation of *tools* and *calculations* in disciplines where theories are poorly defined or nonexistent. "[I]f we are to make sense of changes and developments in modern theoretical physics", writes David Kaiser in his 2005 *Drawing Theories Apart: The dispersion of Feynman diagrams in postwar physics*, we must "focus attention on how most theorists have spent most of their time – and at once we have been drawn into a world of *calculations*, rather than worldviews, paradigms, or theories".[5] Championed by Kaiser and Andrew Warwick, this approach has been chiefly applied to theoretical physics – a discipline in which, Kaiser argues, "[t]heories do not appear, nor is it clear where they might even be found".[6] This study of linguistics applies the lessons of this tool-oriented approach to a discipline in which theories *did* appear, and in

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[5] Kaiser, *op. cit.*, p 356, emphasis in original.
[6] *Idem*, p 377.

which it was very clear where they could be found.

"[W]hen we inspect the materials with which theoretical physicists have worked, night and day", Kaiser continues, "we see tinkering and appropriation of paper tools – tools fashioned, calculations made, approximations clarified, results compared with data, interpretations advanced, analogies extended to other types of calculations or phenomena, and so on" – but, importantly, these physicists spent "little effort trying to articulate specific theories".[7] Theories simply did not play a role in the day-to-day work of postwar theoretical physicists. The situation in American linguistics, this study has shown, was starkly different: when we look at how syntacticians spent their time in the 1960s, we see a relatively balanced division between, on one hand, technical work intended to develop syntactic analyses and tools and, on the other hand, promotional work designed to sell syntactic theories, broadly conceived, to the American linguistics community. Indeed, linguists spent much of their time promoting their *theories* as such. Chomsky's presentation to the 1964 Linguistic Institute, for example, included not only a set of technical tools, but also philosophical commitments and methodological assumptions – the work which was to become the *Aspects* program. This presentation, like many of the decade, was a broad-based effort to promote an inseparable combination of tools, philosophies, and methodologies. From Robert Lees' 1957 essay review of *Syntactic Structures* to Jerrold Katz's 1964 *Mentalism in Linguistics*, and from Sydney Lamb's presentation at the 1965 Linguistic Institute to David Lockwood's 1972 *Introduction to Stratificational Linguistics*, American linguists presented their theories as broadly conceived tripartite bodies of knowledge. The answer to the question *where can the historian of American syntax find theories?*, then, is *everywhere*. Theories were articulated in textbooks, in classroom courses, in journal articles, in underground literature, and at conferences. They were integral to how American syntacticians conceived of their work and their role.

It is clear that, for American linguists of the 1960s, *theories* – tripartite bodies of knowledge including technical, philosophical, and methodological components – were real and ever

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[7] *Idem.*

present. On the academic scene, these theories held a place of prime importance: transformationalists were as committed to transformational rules as they were to their mentalist and anti-behaviorist conception of language, their restriction of linguistics to competence data and sentence-level phenomena, and their introspective methodology; and stratificationalists were as committed to the stratified and relationship-based nature of language as they were to their full inclusion of supra-sentence phenomena, their emphasis on communicative intent as an explanatory criterion, and their continuum-based methodology. Far from only tinkering with technical tools, syntacticians of the 1960s actively worked to promote broad theories – theories they distinctly saw as being in competition with one another. The word *theory* itself – a word which, Kaiser implies, is absent from the theoretical physics discourse of the post-World War II years – was omnipresent in 1960s linguistics. From Noam Chomsky's *Some Empirical Issues in the Theory of Transformational Grammar* to Ilah Fleming's *Stratificational Theory: An annotated bibliography* to Philip Davis' *Modern Theories of Language*, theories were at the heart of linguistic discourse.[8]

Theories have also been central to the recent philosophy of science literature. This literature defines two chief ways of looking at scientific theories: as axiomatic systems of sentences in a formal language (enunciated by, among others, Rudolf Carnap and Carl Hempel) and as sets or collections of models (championed by, among others, Nancy Cartwright, Ian Hacking, and Margaret Morrison).[9] The syntactic theories of the 1960s, however, were neither axiomatic systems nor collections of models: they were what can be described informally as worldviews, or broad conceptions of the study of language, and formally as tripartite bodies of knowledge including technical, philosophical, and methodological components. To working linguists, they represented staked sets of claims – *this is our way of looking at language* – claims which went far beyond technical tools. Importantly, these theories defined communities of practitioners and provided the theoretical equivalent of a roof over heads. Given this reality, I have worked with constituency, strat-

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[8]In accordance with the specific linguistic environment, the terms *theory* and *grammar* were used interchangeably.

[9]Unhelpfully in a study of linguistics, these two views of theories are commonly known as the *syntactic view* and the *semantic view*, respectively.

ificational, and transformational grammars as *theories* throughout this study. I have also, however, integrated the important historiographic lessons of Kaiser's and Warwick's work in two ways.

First, it is instructive to bring ourselves back to the original motivation for the emphasis on tools and calculations in historical studies of theoretical physics. It began, in fact, not as a discussion of *theories*, but as a discussion of *phenomena*. In 1984, sociologist of science Andrew Pickering's work on weak neutral currents led to a seminal observation. "One can [...] sift through the documentary record of particle physics for as long as one likes without coming across a weak neutral current", he wrote: "[w]hat one will find is a record of practices, a record of what scientists were *doing*".[10] Shifting the focus phenomenon to quarks, Kaiser elaborated on this observation, writing that "no matter how hard historians, philosophers, and sociologists might look, they will never find a quark in the midst of the materials they study. They will find notes and calculations, diagrams and plots of data, memoranda, letters, articles, and textbooks – including materials that physicists considered significant for whether quarks exist – but not the phenomena themselves".[11] The historiographic lesson learned is that historians and other observers of science need to avoid *"putting the phenomena first"*, and focus instead on scientific practices, activities, and discourses.[12] Pickering's observation is an important one, highlighting the necessity to move beyond the depiction of science as discovery- and phenomena-centric, and to investigate what scientists actually *do*.

In line with this lesson, I have deliberately placed the phenomena of syntax – specific instances of sentence-level data pulled from textual or spoken manifestations of natural language – on a second tier. Instead, I have focused on the four themes of explanation, pedagogy, knowledge transmission, and lay linguistics. Each of these themes captures a different set of ways in which linguists practiced, acted, and formed their interpretations and understandings of syntax. Explanation illuminates *what* syntacticians were doing, and *why*

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[10] Pickering, *Against Putting the Phenomena First*, p 86–87, emphasis in original, Pickering, *op. cit.*.
[11] Kaiser, *op. cit.*, p 377.
[12] Pickering, *Against Putting the Phenomena First*, p 86, emphasis in original.

they were doing it: by looking at explanatory criteria, we learn what it is linguists wanted their work to explain, and what methods they developed to achieve their goals. Pedagogy uncovers how young linguists came to form their beliefs about their chosen field of study, and reveals the interaction between students and textbooks, courses, and university programs. Knowledge transmission explores the physical ways in which linguistic knowledge diffused and dispersed, linking ideas and research with the material of communication itself. Finally, lay linguistics shows what language teachers, fieldworkers, missionaries, and machine translators were doing with syntactic ideas and, as such, expands the investigation of activity and discourse from core to peripheral disciplines. By organizing my study not through syntactic theories or phenomena as individual threads, but rather on a thematic basis, I highlight the activities and practices of linguists, the interplay between research, training, and idea propagation, and the complexities of social control over knowledge dispersion. There is no single way for historians of science to avoid "putting the phenomena first": the implementation of this lesson depends on the nature of the science under study. In theoretical physics, the lack of theories *per se* has directed historians to tools and calculations. In postwar American linguistics, the omnipresence of theories leads to the study of the manifestations of theories in academic, pedagogical, socio-professional, and practical contexts – a set of themes which provide a powerful method of investigating what syntacticians were doing through the 1960s.

Second, I have shown that while linguistic *theories* were central to the academic scene, in the lay context linguistic *tools* took precedence. This distinction between theories and tools enables us to understand the core of the divide between the academic and lay linguistics scenes, and to understand why lay contexts had little influence on the outcome of the syntactic theory-choice debates. Driven by practicality and applicability, many lay linguists rejected broad tripartite conceptions of linguistic theories and focused instead on syntactic tools *per se*. Importantly, commitment to a single syntactic theory offered no advantage to most lay practitioners. Their interests were best served by assembling the advantageous tools from several theories into a toolkit designed for their specific needs.

Enabled by professional identity and affiliation, this emphasis on tools meant that the syntactic theory-choice debates of the academic context were not replicated in the lay context – and hence that lay work did not strongly affect the fate of any theory on the academic scene. From the opposite perspective, the tool-based focus of lay practitioners did not offer an environment in which academic linguists could easily extend their theory debates. Theory-based rhetoric – omnipresent on the academic scene – was often dropped when academic linguists addressed the lay context. This departure from normal discourse emphasizes the divide between theory-based and tool-based needs and motivations. While in studies of theoretical physics, tools supersede theories in the academic context, this study of American syntax shows that tools and theories can coexist and be valued for different reasons by different communities.

<p style="text-align:center">* * *</p>

With rising interest in the history of psychology, economics, and sociology, the history of science – for decades dominated by the traditional disciplines of physics, chemistry, and mathematics – is beginning to encompass the social sciences. Courses are being developed, books written, museum displays produced, and dissertations devoted. Still, it is a fledgling discipline, and many social sciences have been subject to relatively little historical work. Theoretical linguistics, in particular, has received little attention. As such, it presents great opportunities and great challenges. By combining contemporary history of science tools with linguistics-specific concepts, this study begins to provide an informed historical understanding of theoretical syntax in postwar America. In doing so, it has identified a set of concerns which will be of central importance as the history of linguistics is elaborated: the influence of pedagogical and institutional needs on what is still a young professional discipline; the interaction between academic and lay contexts in an area which is particularly appealing to the general public; and the varied roles played by linguistic theories and linguistic tools in the emergence of a discipline. Most importantly, it has provided a rich and novel account of a dynamic and exciting period in linguistic history.

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