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Linguistics Then and Now: Some Personal Reflections

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Abstract

By mid-twentieth century, a working consensus had been reached in the linguistics community, based on the great achievements of preceding years. Synchronic linguistics had been established as a science, a “taxonomic” science, with sophisticated procedures of analysis of data. Taxonomic science has limits. It does not ask “why?” The time was ripe to seek explanatory theories, using insights provided by the theory of computation and studies of explanatory depth. That effort became the generative enterprise within the biolinguistics framework. Tensions quickly arose: The elements of explanatory theories (generative grammars) were far beyond the reach of taxonomic procedures. The structuralist principle that language is a matter of training and habit, extended by analogy, was unsustainable. More generally, the mood of “virtually everything is known” became “almost nothing is understood,” a familiar phenomenon in the history of science, opening a new and exciting era for a flourishing discipline.

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INTRODUCTION

I've been asked to reflect on my own personal experience with the field of linguistics, a long and complicated story.

I discovered the professional field 75 years ago. It soon became a central part of my life, and has remained so ever since. For some 70 years much of my own work has been devoted to "the generative enterprise," to borrow the term suggested by Henk van Riemsdijk and Riny Huybregts 40 years ago (Huybregts & van Riemsdijk 1982). To try to review that record would be a major undertaking, more than I can contemplate now. Among many current demands are those of ongoing work that I'll barely allude to at the end and that is quite challenging.

Instead of undertaking the comprehensive task, I'll try to give snapshots of "then" and "now," reconstructing "then" as best I can from documents and memories, adding a few thoughts about guiding themes, and only a brief comment on "now," which is before our eyes. And I will try to draw a few general lessons from this comparison.

WHAT HAS CHANGED?

Linguistics is hardly the same field now as it was then. The enormous worldwide expansion in departments, in participants, and most crucially in scope and depth of research is all familiar. What undergraduates study today in introductory courses could scarcely have been envisioned then, let alone questions at the borders of inquiry. Then, the few professional linguists formed a rather tightly knit group. Each typically had a fair degree of competence in just about every area of the field. The picture is radically different today.

What seems to me even more striking is a difference in prospects and aspirations. At that time the field seemed to be approaching an end. The status of linguistics as a science seemed well established for the first time. Careful and sophisticated methods of analysis of language had been developed. At most some tinkering might be needed. What remained within linguistics proper was primarily to apply the methods to new materials.

That much was largely accepted. We get a good deal of insight into the nature of the field—which, for what it's worth, accords with my memories—from an important retrospective study by Charles Hockett (1968), who was not only a central figure of American post-Bloomfieldian structural linguistics but also unique in the careful attention he devoted to the general character, assumptions, and goals of the discipline.

Hockett (1968) wrote that "by 1950, West of the Atlantic, we had reached what seemed to be a reasonable working consensus" on the major issues of linguistic science. Much the same conclusion was reached by another prominent theoretician, Martin Joos (1995), who summarized the collection of major works that he co-edited with the observation that descriptive linguistics in 1956 "seemed to be without a serious competitor." It was firmly established for the first time as a true science, a "taxonomic" science, a "classificatory science" (in Hockett's phrase).

The qualification "West of the Atlantic" was understandable. Little of the long and rich European tradition survived in Europe after the ravages of fascism, Stalinism, and wartime destruction.¹ And there were only fragments elsewhere.

¹One of the leading figures of European linguistics, Roman Jakobson, managed to escape to the United States in 1941, moving in 1949 to Harvard (where I later met him). He shared the sense of enormous advance in the post-Saussurean period and of establishment of linguistics on a scientific basis by mid-twentieth century. See his 1949 paper "Current Issues of General Linguistics" (Jakobson 1990).

The working consensus, as Hockett elaborated and the Joos *Readings* illustrate, was based on two leading principles, largely but not completely shared:

- (1) Linguistic description follows specified procedures of analysis.
- (2) Language is a matter of training and habit, extended by analogy.

The first theme (principle 1) encompassed the large bulk of theoretical work and was the basis for descriptive practice. The second (principle 2), which received only passing attention, generally adopted Leonard Bloomfield's conception of language as "a matter of training and habit," extended to "analogy" for innovation (Bloomfield 1933, Hockett 1968).

Philosophy of language was similar. One of the few who dealt with such matters, W.V.O. Quine (1960), described a language as "a complex of present dispositions to verbal behavior, in which speakers of the same language have perforce come to resemble one another,"² basing himself on Skinnerian radical behaviorism.

From this perspective it is "nonsense" (Hockett 1968) to suppose that there might be anything like a generative grammar, coded in the brain, that determines the structures, sound, and meaning of the sentences of a language, a system that determines, for example, that the sentences in this paragraph have a status different from their mirror images. Similarly, it is "folly" (Quine) to raise questions of right or wrong about choices as to how to describe the properties of language. An individual's language is a constantly changing array of habits. No particular account of this array of dispositions has a privileged status as compared to others. If, alternatively, we think of a language as an infinite set of grammatical/meaningful sentences, as Quine sometimes did, then it is "folly" to believe that one recursive procedure enumerating the set might be right while others are not. Furthermore, there is no "faculty of language." Acquisition of language is just a special case of the general process of habit formation (for Quine, conditioning). Like habit, the mechanisms of analogy were taken to be available without analysis, presumably extensions of general animal behavior.

The Bloomfieldian conception, as developed by Hockett, Quine, and others, was "realist" in the sense that it assumed that a person's language really exists as a (constantly changing) internal state. Though procedural studies typically respected this conception, it played no role in development of analytic procedures or general principles.

Sometimes, however, the Bloomfieldian conception was explicitly rejected. The clearest proponent of this stance was Zellig Harris. His classic work on procedures of analysis opened by describing the study as "a discussion of the operations which the linguist may carry out in the course of his investigations, rather than a theory of the structural analyses which result from these investigations. The research methods are arranged here in the form of the successive procedures of analysis imposed by the working linguist upon his data" (Harris 1951). Alternative procedures may be quite legitimate, yielding different analyses of the material subjected to analysis.

²A few years later Quine appears to have abandoned this view, expressing his willingness to accept whatever "innate mechanisms" are needed for language learning to overcome the "great hump" that lies beyond induction to the cognitive state attained, including whatever is needed to yield a generative grammar—a rule system that "demarcate[s] the right totality of well-formed English sentences"—what is called "weak generation" in the literature (Quine 1969). It is the cognitive state attained for which Quine introduced his "analytical hypotheses," which fall within natural science under this 1969 formulation. Quine never returned to this willingness to adopt the requisite innate mechanisms, which appears to be inconsistent with his major work, and the paper is ignored in the extensive commentary on Quine's views. For discussion of Quine's varying views on these matters, see Chomsky (1975b, chapter 4).

Retrospectively, Harris (1965) took a still stronger stand: There are no “competing theories”; “pitting of one linguistic tool against another” is senseless, an “aberration” with sociological roots. Alternative procedures of analysis can be applied “as a basis for a description of the whole language,” bringing out its various properties in different but not competing ways.

Harris’s (1951) *Methods in Structural Linguistics* (hereafter, *Methods*) was a true classic, the apogee of the procedural enterprise and its virtual culmination. Nothing comparable has been attempted since.

PERSONAL INTERLUDE

My own first awareness of the professional field of linguistics was in 1946. I actually had some prior familiarity with the study of language. My father was a distinguished Hebrew scholar. As a young teenager I read a draft of his doctoral dissertation, a scholarly edition of the main work of the medieval Hebrew grammarian David Kimhi (Chomsky 1952), and we discussed it extensively. A large part of the work was devoted to historical Semitic linguistics. What interested me particularly was the explanatory force of the historical process, a concern that became the primary focus of my own work on language a few years later and since.

In mid-1946, I completed my first year of college. Apart from studying Arabic with the great scholar Giorgio Levi Della Vida, college had proven to be a disappointment and I was seriously thinking about dropping out. I met Harris through independent connections, shared interests in sociopolitical issues. I was one of many young people whose thinking and life course were significantly affected by Harris’s intensive engagement in these domains.³ From our first meeting, I was extremely impressed. Harris had deep and penetrating understanding of topics that were my own special interests, and far beyond. He was very generous in responding to my own inquiries and suggesting new directions to follow. I mentioned that I was thinking about leaving college, and he suggested that I might be interested in taking his graduate seminar in linguistics—the first time I had heard of the field. That sounded exciting, and indeed proved to be. Since I had no undergraduate training, he suggested that I read the draft of his *Methods*, then in the final stages of preparation. I proofread it at the same time. In retrospect, I presume that Harris was indirectly suggesting that there would be much value in my continuing with college.

Reading *Methods* carefully turned out to be more than enough preparation for graduate work. I registered as a linguistics major and took a few undergraduate courses. I was meanwhile renewing my earlier interest in the explanatory power of historical linguistics, again Semitic, in personal discussions with Della Vida and from reading Harris’s (1939) monograph *Development of the Canaanite Dialects*. These interests led me to study some Ugaritic with another great scholar, E.A. Speiser. I was particularly struck by the work on the laryngeal hypothesis based on postulated entities that did not actually appear in existing languages but could explain their curious properties. I learned about this from a book by E.H. Sturtevant that I picked up in the library—I don’t recall which. I was toying with the idea of concentrating on Semitic languages. But Harris’s innovative thinking was much more compelling.

Harris’s courses were unique. They really were compelling. The courses were not about linguistics, which was regarded as largely a finished project apart from applying known ideas to new languages. Rather, Harris was exploring new directions, employment of linguistic methods in analysis of discourse, mostly in sociopolitical domains.⁴ Only two others were regularly involved in these discussions: Fred Lukoff and A.F. Brown, grad students in linguistics. “Class meetings”

³For some discussion, see Barsky (2011).

⁴Not much was published of the extensive work of these years—only two papers, mostly about abstracting (Harris 1952a,b).

were informal, off campus, often marathons, ranging broadly over many areas. And fascinating. It was in the course of these discussions that Harris developed his concept of transformation as a relation of co-occurrence between two structures. The purpose was to normalize texts by using this device to reduce texts to a simpler standard form to which structural methods could be applied.

Harris also suggested that I take graduate courses with some of the outstanding faculty at the university—at the time, not a particularly distinguished one in general but with a scattering of truly outstanding figures. I did, in mathematics and in philosophy, where Nelson Goodman permitted me to take his graduate seminars even though (as in math) I had almost no background apart from my own reading. That turned out to be another life-changing experience. Goodman's courses were mostly devoted to ongoing work of his that was published as *The Structure of Appearance* (Goodman 1951) and to explorations that appeared as *Fact, Fiction, and Forecast* (Goodman 1955), both classics.

In 1948, I started work on a senior thesis (Chomsky 1949). Harris suggested that I do a structural analysis of Modern Hebrew, a language in which I was fairly fluent from childhood.⁵ I found a native Israeli speaker and applied the standard field methods, but soon discovered that it made little sense. I didn't care much about the phonetics, and in the other areas—morphophonemics, syntax, semantics—I pretty much knew the answers, so there was no point eliciting them. I put it aside and turned to something else: what later came to be called a generative grammar, a system that provides "a recursive specification of a denumerable set of sentences," ultimately in phonetic form, assigning them their correct structures [Chomsky 1979 (1951)]. I submitted my thesis, *Morphophonemics of Modern Hebrew (MMH)*, for graduation in 1949; a revised 1951 version was later published [Chomsky 1979 (1951)].⁶ The revision was partially based on a suggestion by Israeli logician Yehoshua Bar-Hillel that the analysis might be simplified by taking forms closer to proto-Semitic as the base—which proved to be the case, something loosely analogous to deep conservation in evolution. That resonated with my earlier interest in the explanatory power of historical linguistics, and turned out to provide valuable suggestions for later work (e.g., Chomsky & Halle 1968).

The project was in part suggested by Goodman's (1951) *Structure of Appearance* and his work in the same years on simplicity and explanation—closely related concepts, as he discussed: The simplicity of a theory provides a measure of its explanatory success. In the close paraphrase in *MMH*, "the motives behind the demand for economy are in many ways the same as those behind the demand that there be a system at all" [Chomsky 1979 (1951)].

Goodman was working on constructional systems. His concern was simplicity of the base, determined by the number and complexity of primitives. I thought it might be interesting to look at a much more complex system from the same point of view. *MMH* opened with a general discussion of simplicity for systems of ordered rules, with notational devices designed to capture valid linguistic generalizations by mapping rule systems expressing these generalizations to a form in which the number of symbols evaluates the grammar—in effect, assigning to a series of rules a number that measures their simplicity/explanatory depth. That provided an evaluation measure for grammars based on testable empirical assumptions about what constitutes a valid generalization. The same approach was used in later work, notably *The Sound Pattern of English* (Chomsky & Halle 1968). And the notations are commonly used without such explicit concern or motivation.

⁵The term Modern Hebrew here is used in contrast to Biblical Hebrew. The term refers to the language that took shape in the revival of Hebrew as a spoken and literary language in the nineteenth-century Hebrew renaissance (Haskalah). It was rather similar to what was spoken in Palestine when I was a student, but different in many ways from today's language.

⁶On the general project, see Chomsky (2021); the title ("Simplicity and the Form of Grammars") is taken from an undergraduate (unpublished) paper, cited in *MMH*, and the first talk I gave as a graduate student. This project was my prime interest at the time, remaining so in many ways.

The grammar in *MMH* consisted of a rudimentary syntax (rewriting rules, involving Harrisian long components) and a detailed system of partially ordered rules for the fairly intricate morphophonemics, followed by a partial demonstration that the ordering was maximally simple. The demonstration could be only partial because in those precomputer days it was impossible to work through all possible reorderings; it was limited to showing that any inversion of adjacent rules increased complexity.

Though the thesis was submitted for graduation in linguistics, I did not regard it as really belonging there. It did not fall within linguistics as I and others understood the field, in accord with principles 1 and/or 2 above. I understood it as an exercise in the study of explanation and simplicity. I think that the faculty in linguistics and philosophy understood it the same way. That seems to have been a good part of Goodman's motive in recommending me for appointment at the Harvard Society of Fellows, where Quine was the relevant Senior Fellow, affording me four very valuable years of work and study, from 1951.

At the time, I was unaware of a linguistic tradition that pursued a similar project, tracing back to classical India and exemplified only a decade earlier in Bloomfield's "Menomini Morphophonemics" (MM) (Bloomfield 1939).⁷ No one pointed this out to me, though it was no secret as I discovered some years later. I suspect it was because this tradition was too remote from the consensus, either the procedural or the conceptual principle (i.e., principle 1 or 2). According to his student Charles Hockett, Bloomfield had studied Menomini intensively and was fluent in the language. His grammar, making crucial use of rule ordering, was sharply inconsistent with his book *Language* (Bloomfield 1933), the founding work of American structuralism. There Bloomfield dismissed rule ordering as a kind of mysticism that violated the behaviorist assumptions and the interpretation of logical positivism that provided the background framework for his work. Rejection of rule ordering was explicit in consensus principles 1 and 2. In particular, the procedural approaches required strict separation of levels, from narrow phonetics to sentences. There were further inconsistencies: The elements Bloomfield postulated violated the basic principles of structural linguistics.

This is sheer speculation, but I've often wondered whether the inconsistencies were factors in Bloomfield's decision to publish MM in Prague, unusual at the time.

Though unaware of MM or the earlier tradition, I was troubled by similar inconsistencies. The entities constructed in the generative grammar *MMH* could not possibly be established by procedures of analysis and radically violated the structural conditions of the consensus. Either the procedural approach or the quest for explanation was misconceived, so it appeared. It was also transparent that an internalized generative grammar could not be described in terms of habit and analogy, nor could it possibly be acquired by the means assumed. What is more, the same was clearly true more generally for the most rudimentary elements of normal language use. It seemed that explanation lay outside the framework of the consensus.

These problems induced a kind of schizophrenia: I took for granted that Harris's achievements in his procedural work were essentially correct, and indeed devoted some efforts to repairing gaps and problems. But what really interested me was what I regarded as a personal hobby: generative grammar and what it suggested about cognition generally. The conflicts became sharper in the early 1950s as I began to pursue the hobby more intensively and reached some limited level of explanation. It came to a head on a day I remember well in 1953 when I decided that the consensus was misguided and that I should devote myself to the hobby.

This transition was expedited by experiences when I entered Harvard in 1951. The American structuralist consensus was absent in Cambridge (and from Ivy League universities generally).

⁷For a careful critical analysis and reconstruction within generative phonology, see Thomas G. Bever (1961).

Roman Jakobson, also at Harvard, represented the European structuralist tradition. Beyond that, structural linguistics had little presence.

There were, however, other lively and influential currents, with their own concepts of language. Behaviorism was flourishing in the radical form developed by B.F. Skinner. His 1947 William James lectures, the source of his later publication *Verbal Behavior* (Skinner 1957), were widely circulated and highly influential, in large part thanks to the advocacy of Quine, who was then lecturing on what later appeared as *Word and Object* (Quine 1960), adopting the Skinnerian paradigm. Another current was based on engineering and mathematics: Norbert Wiener's cybernetics and Claude Shannon's information theory, widely regarded as providing a framework for integrating the social and behavioral sciences more generally.⁸

In general, there was a feeling of euphoria and anticipation of major achievements in what had previously been taken to be the study of mind. Prevailing attitudes are well captured in a critical retrospective discussion by Bar-Hillel (1971) from 1965: "There was a ubiquitous and overwhelming feeling. . . that with the new insights of cybernetics and the newly developed techniques of information theory the final breakthrough towards a full understanding of the complexities of communication 'in the animal and in the machine' had been achieved. Linguists and psychologists, philosophers and sociologists alike hailed the entrance of the electrical engineer and the probability mathematician into the communication field"—a field understood to have broad scope, as indicated by the range of professions mentioned.

There were a few skeptics, joined by Bar-Hillel by the time of his 1965 comments. They included three Harvard graduate students, who met in the fall of 1951 and quickly became close friends, later collaborators: Morris Halle, Eric Lenneberg, and me. It seemed to us that the euphoria was misplaced. We began to read work in comparative psychology and European ethology (Konrad Lorenz, Nikolaas Tinbergen, and others). The art historian (and polymath) Meyer Schapiro brought to my attention an important paper by the prominent neuroscientist Karl Lashley that completely undermined the dominant Cambridge doctrines: Skinnerian behaviorism and the Markovian and statistical-approximation approaches of the new communication science.⁹

By the mid-1950s, our work—joint and separate—had turned to directions that broke sharply from these currents as well as from the structuralist consensus, illustrated in publications of the time and later years [among them, Chomsky 1955, 1975a (1956); Halle 1971; Lenneberg 1967]. And by then it began to be joined by others, among them linguists G.H. Matthews, Robert Lees, Edward Klima, and Robert Stockwell; psychologist George Miller; and mathematician M.-P. (Marco) Schützenberger.

A NEW CHAPTER

At this point a new chapter opens, too complex to try to review here.¹⁰ With the behaviorist constraints on inquiry and theory-construction abandoned, the generative enterprise could adopt what Massimo Piattelli-Palmarini later termed "the biolinguistics program": the search for the internal mechanisms, coded in the brain, that constitute the faculty of language and that, given data

⁸Jakobson's 1949 formulation of the European "*structuralist or functional*" tradition (Jakobson 1990) found its place within the Cambridge framework: "[T]he common denominator of the various trends in the modern science of language," he held, is that "language is primarily interpreted as a *tool of communication*, and its structure is analyzed in the light of the purposes that it and its components serve" (italics in original).

⁹See Lashley (1951), discussed in my review of Skinner's *Verbal Behavior* (Chomsky 1959), a companion to my *Syntactic Structures* (Chomsky 1957). Both went to press in 1957. Rather surprisingly, Lashley's important paper seems to have been virtually unknown at the time outside the neurosciences.

¹⁰For careful review and analysis from the early days, see Freidin (1994).

of experience (which we now know from child language research and careful statistical investigations to be extremely limited), yield the internal language (I-language). The attained I-language generates an unbounded array of hierarchically structured expressions that constitute the linguistic formulation of thoughts and that can (but need not) be externalized in some sensory modality, commonly production and interpretation of sound.

The enterprise then proceeds on many fronts. Within linguistics proper, the goal is to find genuine explanations for the phenomena of language. Within the biolinguistic program, a genuine explanation must meet the conditions of learnability and evolvability. The former has been a driving concern from the earliest days. The latter was also a concern but was shelved as premature; mechanisms that came close to descriptive adequacy were too complex for any plausible evolutionary account. Lenneberg's innovative and comprehensive 1967 work (Lenneberg 1967) was a major breakthrough, but the task could not be seriously undertaken until the 1990s, in my opinion.¹¹ By then, progress in simplifying the theoretical apparatus (UG, in current terminology) had reached the point where genuine explanations could be offered for crucial properties of language within the "Minimalist Program" (MP) (Berwick & Chomsky 2016, chapter 3; Chomsky 2016, chapter 1; Chomsky 2019). How far this program can reach, we do not of course know, but the prospects seem to me far brighter than could have been imagined a few years ago.

Work in MP has also unearthed hidden assumptions in earlier work, which reveal that proposed accounts, while extremely valuable in advancing understanding of complex phenomena in a vast typological range of languages, fall short of genuine explanation—the primary goal of inquiry. That's true of even the simplest cases studied: analyses of such expressions as *what will John read*. Standard analyses make use of a rule of head movement, which violates fundamental principles of transformational grammar or of any system concerned with explanation in the sense discussed here. Furthermore, the substantive elements (*what, John, read*) are just the minimal limiting cases of unbounded unstructured sequences (e.g., *John, Mary, the man who lives on the first floor, . . .*). It has long been recognized that these structures, which abound, are beyond the reach not only of phrase structure grammar but also of any form of transformational grammar or their successors. These are topics now under investigation. There are plausible solutions, integrating the two dilemmas. They involve new concepts that may, as in the past, require rethinking of the mechanisms that have proven so valuable in unearthing the secrets of language, most of them unknown and hardly conceivable when the generative enterprise was in its infancy.

Returning to "then," it should be recognized that there was ample reason for the sense of accomplishment through the twentieth century. The Saussurean separation of synchronic and diachronic linguistics laid a firm basis for the achievements that followed. Anthropological linguistics opened up an enormous range of new material, very different from what had been familiar—in fact, most publications in American structural linguistics were in the *International Journal of American Linguistics*. While the phonemic principle, regarded as a prime discovery, could not be sustained, it nevertheless opened the way to a much improved understanding of the structure and principles of sound structures. Carefully constructed methods of analysis set new standards for inquiry and brought to light critical properties of language, hitherto unrecognized. Much of what was discovered found its place in later research: among many examples, Jakobsonian distinctive features and Harris's morpheme-to-utterance procedures, reconstructed in a different way in X-bar theory. While not part of the structuralist consensus discussed earlier, the symmetrical co-occurrence relations that Harris devised, and in later years studied intensively, found a place in a different form in the generative enterprise.

¹¹For discussion, see Berwick & Chomsky (2016).

In general, the mid-twentieth-century perception that a new stage in the science of language had been reached had a solid basis.

Something crucial was missing, however: puzzlement at what is right before our eyes, the willingness to ask “why” and “how could it be?” That’s not an uncommon occurrence in the history of science, particularly when there have been real accomplishments, and a consensus has been reached. By mid-twentieth century, the time was ripe for doubts and skepticism. How can we think of the field as approaching a terminal point when we have only a limited understanding of the basic mechanisms that determine how the sentences of normal use differ from random rearrangements of their words? Plainly habit, analogy, and dispositions to verbal behavior get us nowhere. Why should we be bound by taxonomy instead of seeking explanations? Or by the dogmas of behaviorism, which bar the normal methods of scientific inquiry involving conjectures about the internal structures that enter into determining phenomenal outcomes? When we allow ourselves to be puzzled about what is taken for granted without warrant, new vistas open.

We might recall that modern science emerged in a somewhat similar way: with puzzlement about the simplest phenomena of nature. Seventeenth-century scientists were no longer satisfied with neo-scholastic accounts that relied on “occult ideas” to account for attraction and repulsion, visual perception, and other ordinary events, when what is needed is clear and distinct concepts and authentic explanations.

Language did not escape their concerns. Galileo Galilei (1632) and the logician-linguists of the Port-Royal monastery expressed their awe and amazement at “one of the most significant proofs of reason: that is, the method by which we are able to express our thoughts, the marvelous invention by which using 25 or 30 sounds we can create the infinite variety of expressions, which having nothing themselves in common with what is passing in our minds nonetheless permit us to express all our secrets, and which allow us to understand what is not present to consciousness, in effect, everything that we can conceive and the most diverse movements of our soul” [Arnauld & Lancelot 1803 (1660)]—the “Galilean challenge” that captures eloquently the puzzle that the study of language seeks to explore and unravel.¹²

An era of “rational and universal grammar” ensued, rich in insight, “rational” in that the goal was explanation, “universal” in that it sought to discover the invariant elements of human thought and its linguistic expression. The tradition was swept aside by twentieth-century structuralist and behaviorist currents, and was almost entirely forgotten. It was revived without awareness in the generative enterprise, which was able to approach the challenge on the basis of the much greater understanding of language that had been attained and with new conceptual tools provided by the theory of computability. Today we can formulate this challenge more precisely, and can partially respond to it, but much remains as much of a mystery as it was 400 years ago, possibly beyond human understanding.

Since the 1950s we have learned to be puzzled by the “marvelous invention” that so amazed seventeenth-century thinkers and to join in their puzzlement about what seems obvious on the surface. Rather than coming close to knowing everything about fundamentals, it’s been clear since the earliest days of the generative enterprise that we know only the rudiments. Theories of language are constantly being revised as new insights emerge from new empirical discoveries or deeper conceptual analysis. There are no methods of inquiry beyond those of science generally. No one can imagine that any terminal point is in sight. The field is open-ended. Advances constantly open new questions, not recognized before. That’s continuing right now in ways that may, I think, significantly recast current understanding.

¹²For a discussion of the Galilean challenge and of the ensuing efforts to rise to this challenge, see Chomsky (1966, 2008); see also Chomsky (2017).

To me at least, that's what seems the greatest difference between "then" and "now."

DISCLOSURE STATEMENT

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Contents

Linguistics Then and Now: Some Personal Reflections <i>Noam Chomsky</i>	1
The Respiratory Foundations of Spoken Language <i>Susanne Fuchs and Amélie Rochet-Capellan</i>	13
Cracking Prosody in Articulatory Phonology <i>Dani Byrd and Jelena Krivokapić</i>	31
Prosody and Sociolinguistic Variation in American Englishes <i>Nicole Holliday</i>	55
The Motivation for Roots in Distributed Morphology <i>David Embick</i>	69
The Morphome <i>Martin Maiden</i>	89
Serial Verb Constructions <i>Joseph Lovestrand</i>	109
Logophoricity, Perspective, and Reflexives <i>Isabelle Charnavel</i>	131
Noncanonical Passives: A Typology of Voices in an Impoverished Universal Grammar <i>Julie Anne Legate</i>	157
Resumptive Pronouns in Language Comprehension and Production <i>Aya Meltzer-Asscher</i>	177
Syntactic Structure from Deep Learning <i>Tal Linzen and Marco Baroni</i>	195
Evidentiality, Modality, and Speech Acts <i>Sarah E. Murray</i>	213
Shifty Attitudes: Indexical Shift Versus Perspectival Anaphora <i>Sandhya Sundareshan</i>	235
Frames at the Interface of Language and Cognition <i>Sebastian Löhnner</i>	261

The Linguistics of the Voynich Manuscript <i>Claire L. Bower and Luke Lindemann</i>	285
Syntactic Change in Contact: Romance <i>Roberta D'Alessandro</i>	309
The Classification of South American Languages <i>Lev Michael</i>	329
The Origin and Dispersal of Uralic: Distributional Typological View <i>Johanna Nichols</i>	351
Cognacy Databases and Phylogenetic Research on Indo-European <i>Paul Heggarty</i>	371
Acquisition of Sign Languages <i>Diane Lillo-Martin and Jonathan Henner</i>	395
Language Socialization at the Intersection of the Local and the Global: The Contested Trajectories of Input and Communicative Competence <i>Lourdes de León and Inmaculada M. García-Sánchez</i>	421
Birdsong Learning and Culture: Analogies with Human Spoken Language <i>Julia Hyland Bruno, Erich D. Jarvis, Mark Liberman, and Ofer Tchernichovski</i>	449
The Use of Corpus Linguistics in Legal Interpretation <i>Neal Goldfarb</i>	473
Environmental and Linguistic Typology of Whistled Languages <i>Julien Meyer</i>	493

Errata

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