２０２３年度

大学院文学研究科博士課程後期３年の課程入学試験

（　春期・一般選抜　）問題

筆記試験　　英　語　学　　専攻分野

試験開始の合図があるまで、この問題冊子を開いてはいけない。

受験記号番号	

成 績	

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次の英文を読んで設問に答えなさい。

The American structuralist consensus was absent in Cambridge (and from Ivy League universities generally). Roman Jakobson, also at Harvard, represented the European structuralist tradition. Beyond that, structural linguistics had little presence.

(1) 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.

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. **(2)**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.

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.

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 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. **(3)**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. 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, **(4)**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, **(5)**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.

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 **(6)**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.

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 **(7)**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 **(8)**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.

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

[Adapted from Noam Chomsky (2021) "Linguistics Then and Now: Some Personal Reflections," in *Annual Review of Linguistics*]

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問1　下線部 **(1)** についてその内容を本文に即して説明しなさい。

問2　下線部 **(2)** についてその内容を本文に即して説明しなさい。

問3　下線部 **(3)** について the former と the latter の内容を明らかにした上で、両者の関係について具体的に説明しなさい。

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問4　下線部 **(4)** についてその内容を具体的に説明しなさい。

問5　下線部 **(5)** についてその内容を具体的に説明しなさい。

問6　下線部 **(6)** についてその内容を具体的に説明しなさい。

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問7　下線部 **(7)** についてその内容を具体的に説明しなさい。

問8　下線部 **(8)** についてその内容を本文に即して説明しなさい。