Reflections
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It is, needless to say, very gratifying to see this comprehensive collection of thoughtful papers relating to the “generative enterprise,” as it was called by Henk van Riemsdijk and Riny Huijbregts 40 years ago.

In these remarks I won’t try to go into the many fascinating topics discussed in the papers, but rather will add some thoughts about questions that have persisted in the background from the days when the enterprise began to take shape 70 years ago, and that remain alive, even if in somewhat different forms.

One basic question is: “What is a theory of language?” And more fundamentally: “Is there a theory of language?” The latter question makes sense even assuming the existence of language (itself a vexed question and one rarely addressed in any useful form, the record indicates). The relation between “theory of language” and “language” is asymmetrical. There can be no theory of language without language, but it’s perfectly coherent to hold that language exists in some form but that it is idle, even seriously misguided, to seek a theory of language. In fact, this position is implicit in the procedural approaches to language that were the major component of the work in structural linguistics, reaching their apogee in Zellig Harris’s classic Methods in Structural Linguistics (Harris 1951),¹ which seemed to many to bring a kind of closure to the field, so understood.

The two most outstanding theoreticians of “post-Bloomfieldian” structural linguistics, Zellig Harris and Charles Hockett, adopted perspectives of this general nature in the mid-1960’s, in different and instructive ways.

Harris’s rejection of the search for a theory of language was explicit. He opens Methods by describing it 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.” (1951, 1)

Alternative procedures may be quite legitimate, yielding different analyses of phonemic or other structures. In 1965 Harris took a stronger position: 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.

From this point of view, language exists – or at least linguistic behavior does -- but there are no theories of language. Though it is not entirely clear what language is, we can distinguish
linguistic from other forms of human behavior, and we can exhibit its properties in different ways, none having a privileged position (Harris 1951, Harris 1965).²

At about the same time, in the late ‘60s, Charles Hockett, who devoted more careful and sophisticated attention to these issues than anyone else, held that it is “nonsense” to suppose that a speaker of a language has acquired anything like a grammar, coded in the brain, which determines, for example, that this sentence is well-formed with a specific range of interpretation, while the same string of words read backwards has a radically different status (Hockett 1968).³

Hockett adopts the general American structuralist consensus based on Leonard Bloomfield’s conception of language as “a matter of training and habit” (Bloomfield 1933).⁴ Hence he understands a language to be a “set of habits,” changing constantly. Speech is produced on the basis “of habits – that is, of analogies” (and “blending,” a marginal process). Sentences are produced and understood on the basis of “analogies” from a “stock of already-familiar forms.” It might be possible, he suggests, to entertain the hypothesis (untestable at present) that “the brains of speakers and hearers coin and understand on the basis of `abstract patterns’ of some sort, extracted over the months and years of language-acquisition and language-use from actual utterances of similar shapes.” But this remote possibility would not require “an additional independent mechanism for the generation of speech,” only further resort to analogy and “abstraction” (both notions left unanalyzed).⁵

The habits that constitute language are acquired by the child by “cultural transmission.” Beyond that, language acquisition relies only on “the basic ability to acquire a language,” apparently some general mode of habit formation that is applied in the special case of language. Analogy too is presumably some general cognitive capacity.

From this perspective, it is also nonsense, Hockett observes, to suppose that a language has an infinite number of sentences, since habits of speaking have temporal bounds (memory, death, etc.).

Notice that while Harris and Hockett reject the generative enterprise as nonsensical, they differ in how they regard language and theory of language. Hockett adopts a “realist” stance. Language is somehow coded “in the head” as a constantly changing set of habits, hence a biological trait of individuals. Theories of such biological traits can be compared for accuracy, an idea that Harris dismisses. The difference was described at the time as “hocus pocus” vs “God’s truth” linguistics, Fred Householder’s ungenerous terms. The distinction was often posed in terms of whether the structure of language is real or imposed by the analyst; sometimes explicitly in Hockett’s terms of neural representation though the realist assumption seems implicit in any interpretation of language as a system of habits. Whether there is a theory of language beyond the general theory of neurally-coded “sets of habits” (analogies) seems to be left open.
Hockett comments that “by 1950, West of the Atlantic, we had reached what seemed to be a reasonable working consensus on these matters,” with only some technical matters unresolved. Much the same conclusion is reached by another prominent theoretician, Martin Joos, who writes that descriptive linguistics in 1956 “seemed to be without a serious competitor” in the United States. It was solidly established for the first time as a true science, a “classificatory science” (in Hockett’s phrase), a “taxonomic” science (Joos 1995).

Hockett’s arguments are based on what he describes as a fundamental principle: no physical object, like the brain, can be well-defined. It will always be subject to “wear and tear,” to “thermodynamic [and] relativistic indeterminacy,” and other deviations from perfect functioning. Accordingly, he concludes, there can be no well-defined grammar formulating properties of language that are coded in the brain and that enter into production and interpretation of speech.

That is quite a far-reaching proposal. The same logic would appear to rule out the study of knowledge of arithmetic, for example a child’s knowledge that there is no largest number. Similarly, it rules out studies of the principles and mechanisms of insect navigation and communication or of visual perception (e.g., the rigidity principle, object constancy, the Marr program, etc.), all efforts to determine the actual computational procedures encoded in the brain while abstracting from many factors that enter into unanalyzed phenomena. It is not easy to see how Hockett’s “classificatory science” of linguistics escapes this critique; any two utterances have distinct neural codings, so even the core procedures of phonemic analysis are unavailable without the kind of abstraction that Hockett rejects under the recommended version of “empirical science.”

Hockett describes his core objection as a demand to keep to the norms of science, part of the consensus of the Bloomfield-based structural linguistics of which he was, as noted, the most sophisticated exponent. If so, it is of a piece with Bloomfield’s insistence on grounding the study of language in behaviorist doctrines, also a sharp departure from the approach of the sciences.

From this general perspective, even the most elementary results about language must be dismissed as nonsense since they are developed within a framework that is blocked in principle: the crucial competence-performance distinction – the distinction between what we know and what we do – which was made clear and explicit for the first time within the theory of computation along with the equally crucial distinction between generation and production. Its core contribution to the study of language and cognition generally was the concept of Turing architecture, a system with a stored program that is constrained by memory but proceeds with no change of program as these constraints are relaxed, as distinct from a system that is constantly revised as new data appears, such as the habit-based consensus that Hockett reviews.

Hockett was quite familiar with the theory of computation, and even has a tutorial chapter about it in his book. But within the habit-based framework of the structuralist consensus that
he reviews, there is no place for its most important contribution to the study of language. A standard illustration at the time Hockett wrote was depth of embedding. As George Miller and I had shown, embedding is intelligible without external memory up to the general limit of short-term memory, and indefinitely beyond as memory is increased—much like our ability to multiply (Miller and Chomsky 1963; Chomsky 1965). None of this shows up in a corpus or our “habits of speaking,” which naturally avoid complexity, a matter with no bearing on what we have internalized. All of this remains complete mystery within the habit-analogy framework. Interestingly, failure to grasp these simple points persists in much current work.

The consensus considered the basic notions of habit and analogy to be clear enough for the purposes of the linguist and scientists in related disciplines. There plainly are many obscurities. Thus consider the idea that speaking and understanding are based on a “stock of already-familiar forms” that supply “the bases for the various analogies, in the head of the speaker.” Or perhaps “abstract patterns” derived by analogy, in some way not yet understood. How is the stock of forms or patterns stored “in the head of the speaker”? It cannot be some astronomical list of sentences remembered from previous occasions, or of “patterns” (also an astronomical list). What are the “forms” and “patterns” that are the basis for the analogies/habits that are used? How do they yield the analogies that are required? How does the inductive leap take place in the specific way it does? More generally, what can we explain about the nature of language, or particular languages, or acquisition, or use in these terms?

The last concern came to prominence sharply with the first modern efforts to go beyond taxonomy to explanation of properties of language by means of a generative grammar, a theory of the language for which considerations of explanatory adequacy at once arise. It quickly became clear that the entities that enter into an explanatory theory of this kind could not in principle be discovered by the procedures of taxonomic linguistics. There was, it seemed, an unbridgeable gap between the procedural approaches of the consensus and the search for explanatory theory—not a problem of course if there are no theories of language apart from the general theory of neural coding of habits (or at all, in Harris’s conception).

Ideas similar to the structuralist consensus soon became familiar in philosophy of language. W.V.O. Quine, one of the most influential philosophers of language in the mid-twentieth century, and one of the few to take up these questions directly, adopted a stand not unlike Bloomfield’s. Rather like Bloomfield’s conception of language as “a matter of training and habit,” Quine took a language to be a “complex of dispositions to verbal behavior, in which speakers of the same language have perforce come to resemble one another” (Quine 1960, 27)—a complex established by Skinnerian operant conditioning, he elsewhere elaborates. And like Bloomfield (see note 4), he elsewhere takes a language to be a set of “significant sequences, [which,] being subject to no length limit, are infinite in variety” (Quine 1953, 53), raising the same questions of consistency as for Bloomfield. David Lewis later took the same view in an influential article: language is an infinite set of sentences used by a population. Both Quine and Lewis conclude that while it makes sense to say that a population uses this infinite set, it doesn’t make any sense to say that there’s a right or wrong way to characterize the set. To look for that, Quine said, would be “folly.” Lewis wrote that he could make no sense of the notion.
Quine and Lewis left unclear how these infinite sets are specified apart from “having meaning in the language” (Lewis 1975).^7

The American structuralist consensus, and the serious questions that it left unanswered, were part of the background for the generative enterprise, which began to take shape in Cambridge Mass. in the early 1950s, along with the “cognitive revolution.” The consensus was, in fact, virtually absent from Cambridge. It happened to be my personal background (I was then a graduate student at Harvard), but that was about all from West of the Atlantic. Roman Jakobson, also at Harvard, represented the European structuralist tradition along with his student and collaborator Morris Halle.

There were, however, other lively currents, with their own concepts of language and theory. One was behaviorism, in the radical behaviorist form developed by B.F. Skinner. His 1947 William James lectures, the source of his later publication Verbal Behavior, was widely circulated and highly influential, in large part thanks to its advocacy by W.V.O. Quine, who was then lecturing on what later appeared as Word and Object, adopting the Skinnerian paradigm.^8 Another current was based on engineering and mathematics: Norbert Wiener’s cybernetics and Claude Shannon’s information theory. This work was widely regarded as providing a plausible framework for integrating the social and emerging cognitive sciences more generally.

Roman Jakobson’s 1949 formulation of the European “structuralist or functional” tradition (in his words) found its place within the evolving Cambridge framework: “the common denominator of the various trends in the modern science of language... [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 is components serve” (Jakobson 1990, ch. 1.).

In general, there was a feeling of euphoria and anticipation of major achievements in what had been taken to be the study of mind. Prevailing attitudes are well captured in a retrospective discussion by Israeli logician Yehoshua bar-Hillel, who was a regular visitor at the Research Laboratory of Electronics (RLE) at MIT, the interdisciplinary research institution that was one of the major centers of these developments:

“There was a ubiquitous and overwhelming feeling around the Laboratory 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.” (Bar-Hillel 1965, reprinted as Bar-Hillel 1971).

The concept “communication field” was understood very broadly, as indicated by the range of professions mentioned.

There were a few who were highly skeptical, not of the achievements of information theory and cybernetics, but of the anticipated breakthrough based on these ideas. The skeptics included
three graduate students who met at Harvard in 1951, and quickly became close friends: Morris Halle, Eric Lenneberg, and me. A few years later, Eric went on to medical school and founded the modern field of biology of language (Lenneberg 1967). Morris and I worked closely together until his last days. Yehoshua also was then and remained a close personal friend. By the time his retrospective was written, he had come to share this skepticism to a fair degree.

The generative enterprise grew from these skeptical roots, including dissatisfaction with the taxonomic consensus in linguistics and in particular, its apparent inconsistency with the search for explanation. In some cases, proposals were clear enough so that they could be refuted outright; the prevailing Markovian conceptions of language use, for example. Others were undermined in ways I won’t review here (Chomsky 1964). By the mid-’50s, the enterprise was beginning to reach beyond RLE. A concept of language and theory of language was taking shape within what later was called the “biolinguistics framework,” the term proposed by Massimo Piattelli-Palmarini. It shared with the Bloomfield-Hockett tradition the realistic conception that the basis for language is coded in the brain, but followed normal scientific practice in abstracting from irrelevant factors to seek explanatory theories of language (in the form of generative grammars) and general principles of language.

Principles are sought at two levels: at one level for individual languages, and at a deeper level for the faculty of language itself -- for “universal grammar” (UG), adapting a traditional term to a new framework. The approach assumed general Turing architecture and distinguished competence from performance and generation from production, distinguishing the stored internal linguistic system from the ways it is accessed for production and perception, integrated with other factors.

From this perspective, both language and theories of language exist. Theories arise at two levels, for each language and for UG, the latter apparently a species property with no known group differences and isolated in essentials in the biological world. Language is understood to be an internal property of an individual, much like the visual system, the immune system, the enteric nervous system (“gut brain”), etc. Both language and theory of language were referred to by the systematically ambiguous term “grammar.” To overcome the confusion, I suggested later that the term “grammar” be reserved for the linguist’s theory, conforming to traditional usage, and that the language be called l-language, “I” to suggest internal, individual, intensional (in the sense of “function in intension”; we seek to discover the actual mode of computation, not just what is enumerated).

Furthermore, much as in the case of the visual system and others, what is coded in the brain of the individual results from several interacting factors: (1) innate structure, (2) external data, and (3) elements that lie outside of the system under discussion. For a computational system like language, the third factor includes principles of computational efficiency along with special properties of the brain and other cognitive processes. There is by now extensive evidence of the role of computational efficiency in the growth and function of l-language, and just the beginnings of evidence of independent cognitive processes and neural properties.9
Within this general framework, we then face the normal tasks of theory construction. Its goal is explanation, which relates closely to simplicity, matters examined in some depth in the ‘40s and ‘50s by Nelson Goodman and a core concern from the first attempt to construct generative grammars 70 years ago. This concern entails unearthing and addressing hidden assumptions, not a simple task as the record indicates, and one that remains challenging today.

There are also special concerns following from the biolinguistic framework, the “realist” conception of I-language and faculty of language. Adopting it, we seek a theory UG of the initial state of the faculty of language that meets the conditions of learnability and evolvability. It must provide the innate basis for acquisition of language within the empirical constraints revealed by study of the process, and a plausible picture of how UG might have evolved.

While these conditions were recognized from the start, attention was largely focused elsewhere, for good reasons. As soon as the first efforts were undertaken to provide some substantive alternative to such concepts as “habit” and “analogy,” it became very clear that the consensus was far off base in believing that basic questions were mostly answered and that what remained was to apply the procedures that had been developed to new materials and to fill in the gaps in the taxonomic science that was pretty much in place. It quickly turned out that very little was known and understood, that the procedures that had been developed were quite limited and had fundamental flaws, and could not even provide the entities that enter into explanatory theory.

The first efforts were therefore focused on developing concepts rich enough to least capture the phenomena of language, with longer term goals sidelined temporarily. It also became clear that the study of learnability faced an enormous problem of “poverty of stimulus” (POS); data available to the child do not come anywhere near accounting for what young children know and understand. No imaginable general mechanism of learning, conditioning, or habit formation could come close to handling the most elementary examples, from acquisition of words on to further properties of what the child quickly attains. Recent statistical investigation of actual materials available to children shows that data are far more limited than had been assumed (Yang 2013) and extensive studies of child language have revealed richness of knowledge and understanding at a very early age, thus radically extending the POS problems.

Recognition of these about acquisition seemed to require that UG be highly complex to account for the inductive leap, but that tends to undermine the goal of evolvability, increasingly so in recent years with new evidence appearing about evolution of language, apparently quite recent in evolutionary time, at about the time of appearance of Homo Sapiens and in place before separation of small groups not long after, so genomic evidence reveals (Berwick & Chomsky 2016, Huybregts 2017).

Pursuit of the enterprise therefore followed two paths: (1) expansion of coverage to a wide range of typologically varied languages, and (2) refinement of descriptive devices to satisfy the conflicting demands of learnability (“explanatory adequacy”) and evolvability, the latter in the background until recently.
Early work used the devices of phrase structure grammar (PSG) and transformations. It was understood that both were far too rich. PSG permits impossible rules and incorporates many hidden assumptions (why not a rule NP → P VP? How is VP → V NP different from X → V Z?). PSG was eliminated in the ‘60s, and efforts were underway from the early ‘60s to reduce the overly rich power of transformations. I won’t run through the work that followed.

By the 1990’s it seemed reasonable to me and some others to consider the thesis that the core part of language, the operations that generate expressions of thought (narrow syntax) might approach perfection from a computational perspective (the Minimalist Program). Pursuit of this program breaks new ground, I think, in reaching genuine explanations: that is, explanations that satisfy the twin conditions of learnability and evolvability, the “holy grail” from the earliest days. If correct, these conclusions constitute a new and exciting stage in the long and rich history of study of language.

The optimal result would be explanations that involve no learning at all and that make use only of operations that must have evolved if language is to exist at all. There is at least one such operation: the minimal computational operation, binary set formation (“Merge” in recent usage). Hence the conditions of learnability and evolvability would be met (vacuously) by an explanation that relies solely on Merge and third factor properties, restricting the second factor to providing data with the triggering effects that are normal for innate properties.

Remarkably, a good deal follows from just the assumption that I-languages are generated by Merge.

In the first place, that suffices to yield at least the skeletal structure for what has been called the Basic Property of Language: the fact that an I-language consists of an unbounded set of hierarchically structured expressions, each a linguistic articulation of thought, each potentially externalized to some sensorimotor system SM, which imposes arrangements required by SM (including linear order, so it appears). SM is typically the sound system, but it is now known that signed language is essentially the same as spoken language in all significant respects, and that even touch can be used (with some limits). I-language is rather like a stored program in a laptop that can be hooked up to different printers.

Second, we derive the ubiquity of displacement, always regarded (by me in particular) as a strange imperfection of language, but in fact produced by the most parsimonious subcase of Merge (Internal Merge, IM). The question is why there is ever resort to the second subcase of Merge (External Merge, EM), which yields compositionality. The answer follows from the external requirement that thoughts require argument structure, not provided by IM, and more generally, from the principle of duality of semantics (Chomsky 2007, 2016).

Third, displacement automatically provides the basis for “reconstruction” – the fact that displaced phrases are interpreted where they are not heard – without the need for special rules.
Fourth, we derive the curious property of structure dependence, known by infants without relevant experience, a property that entails that in applying syntactic or interpretive operations, the child ignores 100% of what it hears (linear order) and attends only to what it never hears (structures constructed by the mind). It follows also that linear order and other arrangements (like those used in sign) are not strictly speaking part of language proper, but are rather imposed by the SM systems used for externalization, systems that were in place long before language emerged and are unaffected by it.

A further consequence, supported by much other evidence, is that the modern doctrine that language is essentially a tool of communication is misguided. Language can of course be used for communication but its basic design ignores this function, and in keeping to the simplest design, nature appears to have endowed language with properties that impede communication.

A more technical conclusion is that the only admissible subcases of Merge are the earliest ones assumed, Internal and external Merge. Various extensions of Merge that have been investigated can be shown to be illegitimate. They yield valuable descriptions, but fall short of genuine explanation of the kind that can attained in some core cases.\textsuperscript{13}

More recent theoretical work, which I cannot review here, examines hidden assumptions that still have not been recognized and eliminated, and that show up even in the best contemporary analyses of such simple sentences as “what should we do?” Dismantling of these assumptions suggests the need to revise and deepen current conceptions in ways that may significantly alter our understanding language.

Increasingly, we are learning how to address a fundamental challenge posed early in the modern scientific revolution, when Galileo and the logician/linguists of the Port Royal monastery expressed their awe and amazement at the fact that language permits us to construct

“from 25 or 30 sounds an infinite variety of words, which although not having any resemblance in themselves to that which passes through our minds, nevertheless reveals all of the secrets of the mind, and makes intelligible to others who cannot penetrate into the mind all that we conceive and all of the diverse movements of our souls.”


Seventy years ago, the challenge (had it been known) would not have been taken seriously. There were answers: training, habit, conditioning, analogy. Prevailing assumptions were that the secrets of language – its nature, use and acquisition -- had been largely unveiled and that what remained was to clean up some loose ends and apply to new phenomena the methods of analysis and description that had been carefully crafted. The project was approaching a successful termination.
There are no such illusions today. To paraphrase the title of a famous history of science, as the generative enterprise began to take shape it soon became apparent that we had to leave behind a “closed world” in which everything was more less understood and enter an “infinite universe” that we were barely beginning to explore. By now we are beginning to understand some of the problems posed by the remarkable species property of human language. Investigations over a range never before contemplated have illuminated many aspects of language that were completely unknown not long ago. We can for the first time provide genuine explanations for some of the basic properties of language, also barely recognized until they were exposed in the course of the generative enterprise. New and exciting vistas are opening up, though some of the traditional concerns remain beyond our reach, perhaps, we may discover, for fundamental reasons grounded in our cognitive nature.

References

Chomsky, N. Forthcoming. UCLA lectures.

1 Completed in 1947, and as it happens, my own introduction to linguistics.
2 For careful review and analysis of the early period discussed briefly here, then on to the 1990’s, see Freidin (1994).
3 In fact, Hockett dismisses this question as irrelevant to linguistics: “a string assembled by consciously violating the patterns of a language, like Chomsky’s (1957) *colorless green ideas sleep furiously*, read forwards or backwards, is equally unimportant for linguistic purposes: it is fully explained when we understand how Chomsky produced it, and for what purposes” (p. 74).
4 Or elsewhere, as “the totality of utterances that can be made in a speech community,” ideas that do not seem compatible (Bloomfield 1926).
5 There had, by then, been extensive work in generative grammar with specific proposals about what Hockett describes as “analogy, “abstraction,” “pattern,” also subjecting them to verification over a wide
range of materials in many languages. Hockett does not allude to this work because it is all outside the range of science, according to his construal of the notion, to which we turn directly.

6 Elsewhere Hockett proposes a framework that seems to resemble a generative grammar (Hockett 1954). Each of the models “sets forth principles by which one can generate any number of utterances in the language” (hence an unbounded number – a conclusion forcefully rejected in his 1968 monograph, as just noted). It is unclear how this result is achieved. The models assume a set of “constructions” (or “processes”), and an account of what can appear (or which processes operate) in each position. But apart from few examples, nothing is said about these core notions, which apparently are intended to be narrowly refined (e.g., conjunction/disjunction are different constructions). It seems, then, that a generative grammar is needed to spell out each construction/process, furthermore independent grammars for each of the positions in these entities. Similar questions seem to me to arise for contemporary Construction Grammar.

7 Quine dismissed the generative enterprise as “folly” on assumptions about “indeterminacy of translation” that he seems to have abandoned in a 1969 article in which he welcomes “any innate mechanisms [...], however elaborate,” (1969b, 307) in particular the “innate mechanisms [...] that are needed in language learning” (1969a, 97) to yield generative grammars that “demarcate the right totality of well-formed English sentences.” (1970, 388) It seems that his basic conclusions on indeterminacy of translation, inscrutability of reference, and others, have to be abandoned with these 1969 conclusions. Quine never reverted to the matter, which is also ignored in the extensive literature on Quine’s thought, so it is not clear what to make of it. For further discussion, see Chomsky (1975). On Lewis, see Pietroski (2018).

8 Later however apparently abandoning it. See note 7.

9 On cognitive processes, see Roberts (2019). Of course, other cognitive processes play a major role in acquisition and use, separate matters. On neural factors, see the observations of Sandiway Fong cited in Chomsky (forthcoming), which relate to interesting developments that I cannot review here.

10 On these matters as they were conceived from the earliest days of the generative enterprise, see Chomsky (2021).

11 The topic reached the research agenda with Lenneberg (1967). See Berwick & Chomsky (2016) for further discussion.

12 Note that narrow syntax excludes externalization. It yields structured expressions that should suffice for articulation of thought, if all further operations fleshing out the articulated thought are universal (or if there are none; on this possibility see Chris Collins, https://ordinaryworkinggrammariant.blogspot.com/2019/10/thought-as-syntax.html). That they are universal is generally assumed. This domain is conventionally called “formal semantics,” but it should I think be regarded as part of syntax – symbolic manipulation – reserving “semantics” to domains that crucially involve reference/denotation. These properties do not appear to hold for human language (as distinct from the speech act of referring) in my opinion, for reasons extensively discussed elsewhere, a matter that would carry us too far afield.

13 For further discussion, see ch. 1 of Chomsky (2016), Berwick & Chomsky (2016, ch. 3) Chomsky (2019), Chomsky (forthcoming).