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Language and Other Cognitive Systems. What Is Special About Language?

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The traditional conception of language is that it is, in Aristotle's phrase, sound with meaning. The sound-meaning correlation is, furthermore, unbounded, an elementary fact that came to be understood as of great significance in the 17th century scientific revolution. In contemporary terms, the internal language (I-language) of an individual consists, at the very least, of a generative process that yields an infinite array of structured expressions, each interpreted at two *interfaces*, the sensory-motor interface (sound, sign, or some other sensory modality) for externalization and the conceptual-intentional interface for thought and planning of action. The earliest efforts to address this problem, in the 1950s, postulated rich descriptive apparatus—in different terms, rich assumptions about the genetic component of the language faculty, what has been called “universal grammar” (UG). That seemed necessary to provide for a modicum of descriptive adequacy. Also, many puzzles were discovered that had passed unnoticed, and in some cases still pose serious problems. A primary goal of linguistic theory since has been to try to reduce UG assumptions to a minimum, both for standard reasons of seeking deeper explanations, and also in the hope that a serious approach to language evolution, that is, evolution of UG, might someday be possible. There have been two approaches to this problem: one seeks to reduce or totally eliminate UG by reliance on other cognitive processes; the second has approached the same goal by invoking more general principles that may well fall within extra-biological natural law, particularly considerations of minimal computation, particularly natural for a computational system like language. The former approach is now prevalent if not dominant in cognitive science and was largely taken for granted 50 years ago at the origins of inquiry into generative grammar. It has achieved almost no results, though a weaker variant—the study of interactions between UG principles and statistical-based learning-theoretical approaches—has some achievements to its credit. The latter approach in contrast has made quite considerable progress. In recent years, the approach has come to be called “the minimalist program,” but it is simply a continuation of what has been undertaken from the earliest years, and while considered controversial, it seems to me no more than normal scientific rationality. One conclusion that appears to emerge with considerable force is that Aristotle's maxim should be inverted: language is meaning with sound, a rather different matter. The core of language appears to be a system of thought, with externalization a secondary process (including communication, a special case of externalization). If so, much of the speculation about the nature and origins of language is on the wrong track. The conclusion seems to accord well with the little that is understood about evolution of language, and with the highly productive studies of language acquisition of recent years.

The question “What is special about language?” covers vastly too much ground for me to try to address it more than very superficially, cutting a lot of interesting corners. I would like to concentrate on two approaches to the question, which differ radically in assumptions. Since space is brief, I will have to draw lines too sharply, but not too much to identify some issues worth thinking about, I hope.

The two approaches differ on whether the topic of this symposium, and my specific topic as well, makes any sense in the first place. Both the general symposium and my topic are based on a presupposition, that is, that language exists—by which I mean that language is a coherent system that can be described by principles of Universal Grammar (UG). But a major tendency in cognitive science, possibly the dominant one by now, holds that it does not, and sometimes states that forcefully and explicitly in a form that I will discuss below. It should be recalled that there is nothing new in this stance. Fifty years ago, it was widely held by the most prominent philosophers and psychologists that language is just a matter of conditioning and some obscure general notion of “induction” or “analogy.” A widely held view in professional linguistics was that languages can differ arbitrarily (within very restricted constraints, like choice of phonetic features, perhaps just properties of the articulatory apparatus), and that the subject consists of nothing more than an array of procedures to reduce a corpus to an organized form in one or another way, selected on the basis of the specific goals of the inquiry, with no other criterion of right or wrong.

Later versions of the “nonexistence” conception were that rules of language can be justifiably postulated only if they are “in principle” accessible to introspection, a dogma—largely incoherent in my opinion—that excludes almost everything. There are other variants, among them the insistence, again by prominent philosophers and others, that language must be regarded as a socio-political entity of some kind, hence dependent on continuity of empires and literary cultures, national myths, military forces, and so on.

The symposium topic—Language and Other Cognitive Systems—not only presupposes the existence of language but also a modular approach to the mind, taking it to be much like the rest of the organism, a complex of subsystems, often informally called “organs,” with enough internal integrity so that it makes good sense to study each in abstraction from the others with which it is integrated in the life of the organism; for example, the visual, immune, digestive, and other organs “below the neck” metaphorically speaking, and the various mental organs: language, planning, the various structures of memory, organization of action, and so on—whatever the right analysis turns out to be. Randy Gallistel (2000) has observed that the biological norm is modular systems with special growth/learning mechanisms in different domains and in different species. There is every reason to expect human language to keep to the biological norm in this respect. There are in fact crucial features of human language that appear to have no significant analogue in the biological world. They also seem to have emerged very recently in evolutionary time, many millions of years after the separation of modern humans from any other surviving species.

My own assumption is that language does exist as a module of the mind/body, mostly the brain, but that the nonexistence approach in its contemporary form in the cognitive sciences is actually raising the right questions—though pursuing them in a way that is very likely to fail, at least as failure and success have been understood for centuries in the sciences.

The study of evolution of language is a hot topic these days, judging by the number of publications that pour out with such titles. That is rather odd in many respects. Much simpler questions

are scarcely investigated: the evolution of communication of the hundreds of species of bees, for example, plainly a far simpler question, but recognized to be too hard to say much about. Very little is known about evolution of cognition generally. Furthermore, it is quite possible that nothing much can be learned by currently available methods, as the prominent evolutionary biologist Richard Lewontin (1998) has argued in unfortunately neglected essays. A look at the literature on evolution of language reveals that most of it scarcely even addresses the topic. Instead, it largely offers speculations about the evolution of *communication*, a very different matter. It is also often based on very strange beliefs about evolution, to some of which I will briefly return.

Let me illustrate with a recent essay that encapsulates clearly many of the assumptions of the nonexistence approach to language and its evolution. In a recent issue of *Science* magazine, there is a review-article discussing books on evolution of language by N.J. Enfield (2010) of the Max Planck Institute. He finds essentially nothing of value in the books reviewed, apart from some discoveries about the lowered larynx in mammals, which have at best a remote relation to language and its evolution. Most of the rest of the contents of the books, Enfield argues, is lethally tainted by the *existence assumption*, the belief that there are rule systems that determine form-meaning relations and conditions of language use; that includes phonology, formal semantics and pragmatics, and narrow syntax—all of which falls within what he calls “syntax,” plausibly because it all involves internal mental computations, and hence technically is syntax in the broad sense.

To illustrate the fallacy of the existence approach, the article is accompanied by a photograph of three infants, suitably interracial, apparently noticing one another. The caption reads: “communication without syntax.” The point apparently is to show that rule systems of the kind studied under the existence assumption are not necessary for communication. The photo could have been replaced by a picture of three bacteria, making the same point.

The title of Enfield’s (2010) article captures another of the major criticisms of the existence approach: it ignores social context when it seeks to determine the properties of language.

To make the matter concrete, take the sentence (1) and two corresponding interrogative forms, (2) and (3):

- (1) He wondered whether the mechanics fixed the cars
- (2) How many cars did he wonder whether the mechanics fixed (answer, “3 cars”)
- (3) How many mechanics did he wonder whether fixed the cars (answer, “3 mechanics”)

Sentences (2) and (3) clearly differ in status: unlike (2), (3) is severely deviant, a violation of the Empty Category Principle (ECP), in technical terms.¹ To investigate such questions as these, according to Enfield (2010) and apparently the editors of *Science*, we have to consider the social context of actual normal use of these expressions (there is effectively none). It is a mistake to raise the question anyway because the sentences are constructed as an experiment and not drawn from a massive corpus.

¹The ECP, which states that an overt subject is required in this position, is a descriptive principle holding under various conditions that have been extensively studied. There have been efforts to explain it, some I think promising, but that would carry us too far afield here. It suffices here to recognize that the phenomenon illustrated falls under a descriptive principle of broad scope, and when properly formulated, should turn out to be universal for human language.

The observation about social context is uncontroversial with regard to communication. It is true, a virtual tautology, that the study of communication takes into account social context.² It is also uncontroversial that the study of the mechanisms that we put to use typically ignores social context, and quite rightly so: for example, the classic work of David Hubel and Torsten Wiesel on the neurophysiology of vision (Hubel & Wiesel, 1959), or of Elizabeth Spelke, Renée Baillargeon, and others on object recognition and constancy (e.g., Spelke, 1985, 1990; Baillargeon, Spelke, & Wasserman, 1985), or Shimon Ullman's Rigidity Principle (Ullman, 1979a, b), or in fact virtually all of the fundamental work that aims to determine the properties of the modules of cognition, at whatever level of inquiry it is conducted.

But, we are instructed, the study of the mechanisms involved in (1)–(3), or vowel harmony, or relative scope of operators, or in fact everything else about language must depart from the scientific norm.

A related criticism, also widespread, is that linguistic research resorts to idealization and abstraction, relying on invented examples as in the cases I mentioned, not keeping to unanalyzed data but rather creating evidence by design. In other words, linguistic research is like the sciences generally. The sciences typically rely on experiments, highly idealized and abstract, and theory-internal—and even on thought experiments, including classic discoveries.

The observation about (1)–(3) is an experiment, much like the study of perceptual illusions, the foundation of much perceptual psychology. One might argue that better experimentation is required in this and other cases—though in reality the facts are so clear in this case that an experiment would be a test of the experiment, not an investigation of the facts: as any scientist knows, it is easy to design experiments that yield noise and hard to design ones that yield meaningful results, a task that often requires determining whether the experimental method proposed gives the right results in clear cases.

The kind of critique just outlined, which is quite widespread, is generally accompanied by a novel concept of science that has emerged in the computational cognitive sciences and related areas of linguistics, with a new notion of “success”: an account of some phenomena is taken to be *successful* to the extent that it approximates unanalyzed data.

Take the study of bee communication. According to this conception, the way it is generally conducted is seriously flawed. Instead of difficult experiments devising circumstances that never occur in nature—say, having bees fly to flowers on an island (see Gallistel, this issue)—bee scientists should be carrying out statistical analysis of massive collections of videotapes of bees swarming, achieving greater and greater success in approximating the videotapes, and getting a tolerably good prediction of what is likely to happen next, doubtless better than bee scientists could give (or would care about). Perhaps physics should be revised the same way. No balls rolling down frictionless planes and other such abstractions and idealizations that have virtually defined the subject for centuries: rather, extensive statistical analysis of videotapes of leaves blowing in the wind and other natural events, which will surely give more successful predictions of what will happen outside the window than what the physics department can provide.

In the major cognitive science journals, we find review articles triumphantly listing dramatic failures, called successes, because they accord with this new concept, unique in history of science. The idea has been radically restricted to language because it is so obviously absurd when

²Statistically speaking, language use is overwhelmingly internal—“speaking to oneself.” If one chooses to call this “communication,” thus depriving the term of much significance, then imagined social context is relevant.

we move beyond language that no one would dare to propose it, even for systems as closely related to language as arithmetic. We do not study arithmetical capacity by constructing models based on statistical analysis of masses of observations of what happens when people try to multiply numbers in their heads, without external memory. At least I hope no one does.

Enfield (2010) also puts forth a far-reaching thesis, quite standard today in the cognitive sciences, and a clear expression of the nonexistence thesis: “language is entirely grounded in a constellation of cognitive capacities that each—taken separately—has other functions as well.” Which means that language exists only in the sense that there is such a thing as today’s weather, a constellation of many factors that operate independently. Take the phenomena (1)–(3), for example. Enfield cites a source to justify his conclusion, but it has little relation to the thesis, not because he has chosen badly, but because there are none that do better. But he is correct, I think, in saying that this is what many cognitive scientists believe.

Another influential version of the idea that language does not really exist is illustrated in a contribution to a handbook of child development by Michael Tomasello (2006). According to the conception he proposes, there are no linguistic rules and little to say about apparent descriptive regularities—say Empty Category Principle (ECP), as in (1)–(3). Rather, there is nothing but “a structured inventory of meaningful linguistic constructions—including both the more regular and the more idiomatic structures in a given language (and all structures in between).” All of these are “meaningful linguistic symbols [that] are used in communication,” his topic, there being no language apart from this inventory. The inventory is structured only in that its elements—words, idioms, sentences like this one, etc.—are acquired by processes of pattern-finding, schematization, and abstraction common to primates, and a few other processes, all left obscure. So there are individual instances of an “interrogative construction,” a “passive construction,” and so on, but we must not analyze them into more fundamental processes that hold also for a variety of other constructions, for example, displacement principles that apply quite broadly.

To take a standard example from decades ago, we can assign (4) to the *passive construction* and (5) to the *raising construction*, illustrated by such expressions as “John seems to be intelligent” (where *John* is interpreted as in “it seems that John is intelligent,” and presumably is derived by raising from its infinitival counterpart “seems John to be intelligent,” *John* being barred in that position by standard case principles). But we must assign (6) ambiguously to both:

- (4) John was elected
- (5) John seems to have been elected
- (6) John was expected to be elected

We are forbidden to notice that a very general rule of displacement applies—in fact, the elementary rule Internal Merge (IM) discussed below—to all three cases (and many others), one step in dissolving the constructions, now recognized to be descriptive artifacts, into elementary components of great generality. We are, in short, forbidden to pursue the path of normal rational inquiry.

The same must be true of examples (1)–(3): (1) and (2) are learned just the way the child learns “river,” “how do you do,” “kick the bucket” (meaning “die”), and so forth. The child somehow learns the same way that (3) is not usable for communication as (1) and (2) are. I leave it to others to try to give some rational interpretation of such conclusions, which abound.

Presumably expressions could have virtually any other properties in the next language we look at. The inventory is, in effect, an arbitrary collection of unanalyzed “linguistic symbols,” and also is finite, apart from some hand-waving (see, e.g., Tomasello, 2006).

These influential approaches and others like them ignore even the most elementary properties of language, or sometimes propose alleged solutions that simply beg the question. A harsh judgment, but easy to support, I think. They are also typically concerned not with language but with communication, and hence, sensibly, insist that social context is essential, virtually by definition, as in communication among organisms generally, from bacteria to humans.

Enfield (2010) expresses a closely related belief that is also widely held: “There are well-developed gradualist evolutionary arguments” to support the conclusion that there is no such thing as language, except as a complex of independent cognitive processes. Again, no relevant source is cited, nor does one exist. He invokes these gradualist evolutionary claims in a critique of what he calls the “saltationist argument” that the transition from finite to unbounded was not gradualist. He attributes the saltationist argument to me, but that is like attributing to me the claim that $2 + 2 = 4$. It is a logical truth that the transition was saltationist—a dirty word in many circles on the basis of a curious but widespread misunderstanding of evolutionary biology, worth exploring, perhaps, but I will put it aside here apart from noting that the “saltationist” heresy is considered unproblematic within actual evolutionary biology. Thus, one recent book by two prominent evolutionary biologists (Kirschner & Gerhart, 2005) takes as its “central problem” the question “how can small random genetic changes be converted into complex useful innovations,” giving many examples. A leading paleoanthropologist (Tattersall, 1998) concludes that the innovation “that set the stage for language acquisition . . . depended on the phenomenon of emergence, whereby a chance combination of preexisting elements results in something totally unexpected,” a “sudden and emergent event,” presumably “a neural change . . . in some population of the human lineage, . . . rather minor in genetic terms, [which] probably had nothing whatever to do with adaptation.”³ All “saltationism” with a vengeance, at least if the term is supposed to have any meaning.

Though we know very little about the evolution of language, there are a few fairly clear conclusions, and they are suggestive. It is perhaps worth mentioning that the phrase “evolution of language” can be misleading. Languages do not evolve, organisms do—at least in the biological sense of the term “evolution.” Languages are constantly changing, but that is not evolution. When we speak of evolution of language, then, we mean evolution of the species.

There is good evidence that language capacity is the same for all human groups. If an infant from a remote tribe in the Amazon is raised in Boston, its language will be that of my grandchildren, and conversely. There are individual differences, but no known group differences. It follows that there has been no meaningful evolutionary change with regard to language since the time our common ancestors, perhaps a very small group, left Africa and spread around the world, about 50,000 years ago, it is commonly assumed. If we go back roughly 50,000–100,000 years before that, there is no evidence in the archaeological record for the existence of language. Somewhere in that narrow window, there seems to have been a sudden explosion of creative activity, complex social organization, symbolic behavior of various kinds, records of astronomical events, and so on—a “great leap forward” as Jared Diamond (cited in Carroll, 2005) called it—generally

³Elsewhere Tattersall (2005) suggests that human intelligence more generally is an “*emergent* quality, the result of a chance combination of factors, rather than a product of Nature’s patient and gradual engineering over the eons.”

assumed by paleanthropologists to be associated with the emergence of language. Since then there appears to have been no relevant evolutionary change. If one prefers to double or triple the numbers, nothing much changes. These simple observations, reasonably well supported, provide some plausible suggestions about language design, to which I will return.

Suppose now that we assume that language exists—that is, that this symposium is not a waste of time, based on a false presupposition—and that we want to look at language as a normal biological system, a module of the mind/body. We, therefore, adopt what has come to be called “the biolinguistic perspective,” which began to take shape in the 1950s. The standard questions for any module then arise—roughly, *What*, *How*, and *Why* questions: *what* are the properties of acquired languages (understood here as *I-language*, a system that is *internal* to an *individual*, and *intensional* in that it is a generative process in the sense described below)?; *how* are they acquired?; and *why* do they have these properties and not others? The questions have a logical order, but that is not, of course, the order of research; some understanding of *why*, for example, can inform research into *what* are the properties of the mature system.

Perhaps the most elementary property of language is that it consists of a discrete infinity of interpretable expressions. Each speaker has internalized a generative process that yields an infinite array of hierarchically structured expressions, interpreted at two interfaces, sensorimotor (SM) and conceptual-intentional (CI; thought and planning of action). Thus, language is “sound with meaning,” in Aristotle’s common sense dictum. The unbounded range is an elementary fact that does not seem to have received much, if any, notice until the 17th century scientific revolution, where it played a significant role. An approach to language that does not at least capture this property cannot be taken seriously.

For language or any other internal module, its growth and development in the individual involves at least three factors: (1) external data; (2) a genetic endowment that converts data to experience and guides the general course of development; and (3) principles of broader scope, including laws of nature, invariably a major factor in evolution and development. For language, we can analyze the genetic endowment into a component specific to human language (UG), and others relevant to language development (other cognitive systems, neurophysiological structures). It would seem likely, *prima facie*, that UG is critically involved in determining such phenomena as (1)–(3), mentioned earlier (though third factor principles may well be crucial). But we can explore its role in the very early stages of language development. We know, for example, that human infants instantly and reflexively extricate language-relevant data from the blooming, buzzing confusion, no trivial task, apparently a human-specific element of UG. They also quickly acquire knowledge of the general prosodic structure of their language (in part prenatally, it appears), and of the phonology generally—all, it appears, dependent on UG.

Some early steps may also involve other cognitive processes. In my own work on this topic, in the 1950s (Chomsky, 1975), I took for granted that extraction of words from running discourse must be based on calculation of transitional probabilities, since there seemed to be no other relevant evidence.⁴ But the matter turns out to be more complex and more interesting. Recent work shows that this method fails, though results sharply improve when it interacts with UG principles: six-month-old infants segment into elements with word-like properties when low

⁴The suggestion is borrowed from Zellig Harris (1951), who proposed it for identification of morphemes, keeping to the procedural assumptions of the day on largely “level-by-level” analysis. But morphemes are much more abstract elements, lacking the beads-on-a-string property necessary for the statistical analysis.

transition probabilities are aligned with phrasal prosodic constituents (Shukla, White, & Aslin, *in press*).⁵

Though real results remain sparse, except in the novel sense of “success” adopted by recent computational cognitive science, the role of statistical reasoning and other cognitive processes in language acquisition is potentially a significant area of research, something that has never been in doubt since the early origins of current work, contrary to many misperceptions. There are also presumably conditions imposed on language by the structure of the brain, though too little is known to draw conclusions as far as I am aware, despite interesting recent progress in neurolinguistics. It may be, as Randy Gallistel (Gallistel & King, 2009) has argued, that a fundamental reorientation of centuries of study of the brain will be necessary to discover the neural roots of the computational capacities not only of human language but also even of insects, where they are indeed astonishing.

Let’s turn to UG. The question of whether language exists is, basically, the question of whether UG exists. Though, as already noted, this is commonly denied, I know of no coherent alternative. In the early work in the 1950s, it appeared as though UG must be extremely rich to achieve a degree of descriptive adequacy. One major goal of theoretical linguistic research since that time has been to reduce the postulated complexity of UG in accounting for phenomena of language. The reasons are straightforward. The first is standard rational inquiry, seeking to achieve greater explanatory depth, for example, to analyze (4)–(6) into simple and general components, overcoming redundancy, eliminating the stipulated artifacts (“constructions”), and deepening explanation, relying on the third factor principle of minimal computation. Another reason is the hope for an eventual serious study of evolution of language. Evidently, this task, to the extent that it is feasible at all, is rendered more difficult to the extent that the postulated target, UG, is more complex.

The nonexistence approach I referred to shares the same goal: to reduce UG, to zero in this conception. There are several salient differences between these distinct approaches. The first is with regard to results. I think it is fair to say that there are virtually none in the nonexistence literature, except in terms of the curious notion of “success” that has been contrived, departing from all of science. In contrast, there are quite substantial results in the existence literature. In part these derive from investigating interaction between UG and other cognitive systems, as in cases mentioned earlier. But, overwhelmingly, they result from investigating third factor considerations of computational complexity, even though the inquiry has often not been phrased in these terms. These steps include, for example, dissolving “constructions” into more general components, eliminating phrase structure grammar with its rich stipulations, radically reducing the complexity of the transformational grammars that were designed to accommodate noncontiguous relations such as the ubiquitous phenomena of displacement and morphological discontinuity, and finally unifying these two generative systems under the simplest computational operation, which functions in some manner in any generative system. Recent inquiry into these topics is often called “the minimalist program,” but the term is misleading: it is just ordinary science, extending the main thrust of theoretical linguistics since the early days of the biolinguistic approach in the 1950s.

⁵The Shukla et al. (*in press*) paper expands on results of Charles Yang (2002). For more on such interactions, see Yang (2003), and for a lucid introduction, see Yang (2006). This material is rarely if ever cited in the triumphalist literature on successes of computational cognitive science, perhaps because it commits the heresy of assuming that language exists.

These developments are rather natural. Language is clearly a computational system. Therefore, it makes sense to seek the role of general principles of computational efficiency that apply far more broadly and may well be rooted in organism-independent natural law. This is particularly natural given the very little we know about the evolution of language. When a generative process (GP) emerged in some individual, perhaps by some slight rewiring of the brain, it should have been as simple as possible, the minimal change from the earlier state, presumably by mutation. Furthermore, there were no selectional pressures at that initial stage, so we should therefore expect the emergent GP to be determined solely by natural law. The same should be true as this change is transmitted to offspring. There is increasing evidence that something like this may be the case.

With sufficient progress in these efforts one can envision a new research paradigm, which a number of researchers are pursuing: “approaching UG from below,” that is, seeking to determine where hypotheses about UG depart from what would be an optimal solution to basic conditions of adequacy that have to be met by any such theory, and to overcome these departures where possible.

Consider again the *What* questions, and more specifically, what is special about the properties of the acquired languages. As soon as the question was addressed within the framework of generative grammar, many puzzles came to light, previously unnoticed, some still on the agenda. The moment is somewhat reminiscent of the early modern scientific revolution. For millennia, scientists had been satisfied with traditional answers to simple questions such as why a rock falls to the ground while steam rises to the sky: they are seeking their natural place. When 17th century scientists allowed themselves to be puzzled by such facts as these and to investigate them (quickly refuting conventional beliefs), science entered a new phase. The capacity to be puzzled is well worth cultivating, as history amply reveals.

Of the myriad of puzzles that arose when the first efforts were made to go beyond resort to “analogy,” “training,” “pattern formation,” and the like, I will mention two, because they are among the very few cases for which answers have been sought relying on nonlinguistic cognitive processes, and typically adopting the nonexistence assumption.

One hoary example has to do with Aux(iliary)-inversion, as in (7):

(7) Can eagles that fly swim?

We understand that the question is whether eagles can swim, not whether they can fly: the Aux element *can* is associated with *swim*, not *fly*. That is obvious from the interpretation, and also from morphology, as in “are eagles that fly swimming,” “have eagles that fly been swimming.”

But why should that be the case? Ease of computation would suggest that fronted-Aux should be associated with the closest verb, hence *fly*, not *swim*. Putting the problem differently, two concepts of minimal distance conflict: minimal linear distance would relate (7) to “eagles that can fly swim,” while minimal structural distance relates it to “[eagles that fly] can swim.” The question, then, reduces to why the language learner reflexively minimizes the property of structural distance (the principle informally called *structure-dependence*) rather than adopting the computationally far simpler property of linear distance.

Note that while there is a good answer to the *What* question—namely, minimal structural distance—the *How* and *Why* questions remain. We can clarify what is at stake by spelling out the structure of (7) a little more carefully. It has been recognized for a long time that clauses

at a minimum are introduced by a *complementizer* *C*, which at least indicates *force* (declarative, interrogative, etc.) and provides a position for extra-clausal elements such as *can* in (7). Furthermore, the fronted Aux is really an inflectional element, with a dummy Aux attached if the affix cannot stand alone, as in “did eagles that fly swim.” Hence, a more complete representation of (7), including the obvious phrase structure, is (8), with *can* in the *C* position, and *v*, *v** in the positions of the inflectional elements that head the predicates of (5) and of the embedded subject, respectively:

- (8) Can [[eagles that [*v** [fly]] [*v* [swim]]]?

Minimal linear distance relates *can* and *v**; minimal structural distance relates *can* and *v*. Interpretation keeps to the latter, reflexively. While *v* here is only a notational device indicating the position of interpretation, it is actually more than that, as we see directly.

Aux-inversion has been the topic of a considerable industry in computational cognitive science, seeking to show that the child acquires this knowledge on the basis of statistical analysis of a corpus of data, in accord with the nonexistence thesis. New papers come out regularly, some now in press in major cognitive science journals (see Berwick, Pietroski, & Chomsky, 2010, for a review). They have curious properties. One is that each fails, dramatically so, though they are regularly cited as successes in the literature, as they are, in terms of the novel conception of “success” mentioned earlier: roughly approximating unanalyzed data. Another is that each effort ignores the simple explanation, which in fact generalizes to many other constructions in all languages: structural distance is minimized. A third is that it would hardly matter if the approaches succeeded, since they would leave the basic question untouched: why is structural rather than linear distance minimized universally, in all languages and constructions in which the question arises? For the most part, the methods, or very similar ones, would work just as well in a pseudo-language that used linear rather than structural distance for interpretation. A background question is how the child even knows what the intended interpretation is in such cases as (7), unless it is already relying on the structure-dependence principle without any data at all.

Another case discussed in the nonexistence literature has to do with binding theory, as illustrated in (9):

- (9) Do they expect (John) to see each other next week?

If “John” is missing, then *they* is the antecedent of *each other*, but not if it appears. This again is one of the rare cases of any significance discussed in the nonexistence literature (Chater & Christiansen, 2010).⁶ The authors propose that the binding theory relation between *they* and *each other* is simply “an instance of a general cognitive tendency to resolve ambiguities rapidly in linguistic and perceptual input,” specifically, to establish the antecedent-anaphor relation as

⁶Chater and Christiansen (2010) also pose what they take to be a lethal dilemma for the minimalist program (concerning poverty of stimulus arguments), but their concerns are based on complete misunderstanding both of the program and these arguments, and failure to understand the reasons why for half a century theoretical linguistics has sought to overcome these arguments—adopting the minimalist program, even if not the name. For a sophisticated study of binding theory, within the minimalist program as it is actually understood, see Reuland (2011).

quickly as possible in comprehension.⁷ Hence, the facts might rely on an innate constraint but not one that is domain specific.

The conclusion might turn out to be correct, but it is hard to see how their proposal supports it. Thus, if *John* is present in (9), then the quickest way to find the anaphor is to take “they” to be its antecedent, since *John* cannot be. Even if there is some way around this apparent refutation of their proposal, it also fails even if *John* does not appear, as in (10):

(10) Who do they expect to see each other next week?

The reason is intuitively clear: there is an antecedent for *each other* closer to it than *they*, namely the unpronounced element in the position of the variable in the interpretation of (10) as (11):

(11) For which persons *x*, they expect persons *x* to see each other next week

As soon as we pay attention to the most elementary facts, it appears that we have to reintroduce rule systems of the kind that Chater and Christiansen (2010) are trying to avoid, and much else if we go beyond these. Or, preferably, we should follow the course of theoretical linguistics for the past half century and try to determine to what extent the apparent complexities can be eliminated in favor of other considerations, general cognitive processes if this proves possible (though not what they propose) or third factor considerations of computational complexity, which have typically succeeded in the past.

Note that these are not exotic examples, constructed to test the Chater-Christiansen proposal. Rather, they are among the earliest cases that binding theory sought to address.

Despite their widespread endorsement, it is hard to find evidence or argument to support the nonexistence approaches. I will therefore continue to assume that there is indeed something special about language, and that this symposium accordingly has a topic.

If so, then the first question is to determine the nature of the GP that yields structured expressions over an infinite range and their interpretations at the interfaces. Embedded somehow in any such process is a combinatorial operation, call it Merge, which takes objects already generated and forms from them a new one. In the simplest case, then, $\text{Merge}(X, Y) = Z$. Uncontroversially, we should seek to adhere to the overriding condition of Minimal Computation (MC), unless the evidence requires further complication. We therefore take $\text{Merge}(X, Y) = \{X, Y\}$, leaving *X* and *Y* unaltered and unordered. *X* and *Y* in $\text{Merge}(X, Y)$ are either distinct or they are not; in the latter case, unless further complications are introduced, one is contained within the other, say *Y* is contained in *X* (technically, is a *term* of *X*, an object already generated at an earlier stage of the computation). Call the case of (*X*, *Y*) distinct *External Merge (EM)*, and the case of containment *Internal Merge (IM)*. Barring some stipulation—hence complication of UG—both are freely available. Skipping details, GP will generate *S* = “John read that book” by EM, forming successively {that, book}, {read, {that, book}}, {John, {read, {that, book}}}. These generated objects are submitted to the conceptual-intentional (CI) interface (roughly, systems of thought and organization of action), for determining semantic relations on the basis of the structure provided. *S* so far is unordered, but order appears to be irrelevant for this interpretation. The sensory-motor (SM)

⁷Putting aside the irrelevant matter of use of language, the basic observation has an important element of truth, and in fact restates the formulation of binding theory principles in terms of minimal search (with the crucial qualifications investigated long ago).

interface of course requires some kind of ordering (depending on the modality, e.g., different for speech vs. sign). Hence, the *externalization* of S imposes linearization.

Suppose it can be shown that linearization is never required for interpretation at CI (conceptual-intentional). Then we would expect it to be introduced solely as a reflex of SM (sensory-motor), where it is plainly needed. That would carry us a step farther towards answering the *How* and *Why* questions that remain for Aux-inversion: minimal structural distance (structure-dependence) is the only option (given the third factor consideration MC, Minimal Computation): linear order is simply not available to the computational system at the point where the C-inflection relation is established. Though the conceptual arguments supporting this move are clear, there are many empirical difficulties to face. There also remain interesting additional puzzles, not recognized in the published literature, but I will put them aside here.

Suppose we proceed further, generating S' = "that book, John read." The semantic properties of "that book" include its properties in S, along with the extra property of topicalization. Hence, S' will be generated from S by IM, Internal Merge, yielding (12):

$$(12) \ S' \{ \{ \text{that book} \}, S \} = \{ \{ \text{that book} \}, \{ \text{John}, \{ \text{read}, \{ \text{that}, \text{book} \} \} \}$$

In (12) there are two *copies* of the object {that, book}. S', with the two copies, has the right form for CI (conceptual-intentional) but not of course for SM (sensory-motor)—first because linearization is required and second because the hierarchically less prominent copy is not pronounced. The latter property follows at once from MC: at least the most prominent copy must be pronounced, or there will be no evidence that topicalization took place at all, but computation is reduced if at most one copy is pronounced—massively reduced in nontrivial cases.⁸

The same considerations hold for the earlier examples discussed. In (8), *v* is actually *can*. The expression with two copies has the right form for CI: the hierarchically lower copy enters into the appropriate semantic relations, as in (7); the higher one, in the position of C, indicates that this is an interrogative construction. The lower copy is not pronounced. The basic facts adhere closely to the overriding third factor consideration MC.

The same is true of (10), generated as (13), with two copies of *who*:

$$(13) \ \text{Who do they expect who to see each other next week?}$$

The interpretation (11) follows directly: deletion of the lower copy yields (10).

These and many other examples suggest consequences for cognitive architecture. Keeping to the principle of MC, the GP yields forms that are appropriate for semantic interpretation at CI but not for production and perception at sensory-motor (SM), though this SM inadequacy also follows from MC. The inadequacy is severe. Anyone who has worked on parsing programs knows that a major difficulty is posed by "filler-gap" problems: given *who* in (10), the problem for parsing/perception is to locate the gap where it receives its interpretation in the argument structure of the sentence, not a trivial matter in general. These problems would largely be obviated if all copies were pronounced, violating MC. There are many similar cases. Garden path sentences and ambiguities, for example, raise difficulties for perception, but they appear to be produced by allowing GP to function without stipulation or constraint. To the extent that they

⁸There are interesting cases where some residue of the lower copy is pronounced, or when some other modification marks position of copy erasure, lending further support to the general approach outlined here.

are understood, the same seems to be true of “islands,” such as (3): an expression that can be “thought” but not articulated, without a complex paraphrase.

In brief, where there is a conflict between communicative and computational efficiency, the latter seems to win, hands down. It appears that Aristotle’s dictum should be reversed: language is not sound with meaning but meaning with sound, a very different matter. Externalization by the SM system appears to be a secondary property of language. Externalization is also in part independent of modality, as work of the past few decades on sign language has revealed. Sometimes externalization is employed for communication—by no means always, at least if we invest the term “communication” with some significance. Hence, communication, *a fortiori*, is a still more ancillary property of language, contrary to much conventional doctrine—and of course language use is only one of many forms of communication.

These conclusions seem exotic to many commentators, possibly, in some cases at least, because of “gradualist” mythologies of the kind already mentioned. In contrast, they seem not only natural, but almost obvious to leading evolutionary biologists and paleoanthropologists.⁹

The general approach just informally outlined (in one of several variants) is the only one I know of that offers any hope of answering *What*-questions: what are properties of human language, and what is special about them? Every approach to the *How*-questions is based, necessarily, on some assumed answer to the *What*-questions. The more we understand about these, the more seriously we can address questions of language acquisition. And the relation is reciprocal: what is learned about language acquisition can in principle, and has often in practice, served as a guide to investigating the *What*-questions.

As for *Why*-questions, in particular why UG has this rather than some other form, the best answer would be that a sudden and very slight evolutionary event yielded Merge, and that the rest follows from natural law. That thesis—“the Strong Minimalist Thesis (SMT)—would fit well with the little about evolution of language that seems reasonably well confirmed. We are far from reaching that goal, and do not know whether the huge gaps can be overcome or whether much richer assumptions are required about UG (or, perhaps, about second-factor considerations involving cognition and the brain, now unknown). But, though of course remote, the goal seems a good deal more realistic than it did not very long ago.

Suppose that the SMT is even approximated, that the core system of human language is something like a snowflake. That possibility, if even close to accurate, yields some suggestions about the *How*-questions and other matters. Externalization is a hard problem. It requires relating two systems that arose quite independently, so it appears. Current evidence indicates that the sensory-motor system was present hundreds of thousands of years before the emergence of human language and the “great leap forward,” and evidence about possible adaptations for language is thin. In learning a language, first or later, one attends almost entirely to the externalization: phonetics and phonology, morphology, ordering, and so forth. No one is taught Empty Category Principle (ECP), structure-dependence, the binding properties illustrated in (10)–(11), (13), or any nontrivial properties of syntax, nor can they be learned from unanalyzed data, so it appears, despite many claims of the kind mentioned earlier. For semantic properties, evidence is virtually non-existent, and it is generally assumed, quite realistically, that the principles and properties are universal. Externalization is also easily subject to change, sometimes radical change

⁹For quotes from Ian Tattersall and evolutionary biologists (Nobel laureates) Salvador Luria and François Jacob, see Chomsky (2010).

(e.g., the Norman invasion). That does not appear to be true for syntax-semantics. It is hard to make any sense of the invention of sign languages (sometimes recently invented, as in the case of Nicaraguan sign) except on the assumption that the language was already basically present, internalized, as is almost certain anyway given the very strong evidence that there has been little if any evolution of the language faculty at least since the trek from Africa. What was missing was a form of externalization.

With emergence of the GP for language—in the simplest case, just binary Merge satisfying MC—primitive elements of an already existing conceptual system would enter for the first time into a “language of thought,” for a single individual. Transmission to offspring would yield a community sharing this capacity, so that the secondary process of externalization might be usefully undertaken—a hard cognitive problem, as mentioned, and one that might be solved in various ways, of course subject to overriding constraints, at least in some cases traceable to UG, so it appears. It is sometimes claimed that there must have been a prior “language of thought,” but that speculation adds nothing, merely transferring the problem of its origin one step back. The same is true of the belief that there must have been “protolanguages,” simplified forms of externalization (or maybe of language itself). There is, of course, no empirical evidence for that, and no conceptual argument either. Transition from protolanguage to full language, like transition from 7 word sentences to the unbounded character of human language, is no simpler than “one fell swoop.” Similar questions arise about acquisition and have been investigated with interesting results that should be familiar in work by Lila Gleitman and others (Shipley, Smith, & Gleitman, 1969; Gleitman, Cassidy, Nappa, Papafragou, & Trueswell, 2005).

I cannot end without at least mentioning another extremely serious problem, which has been barely addressed. A computational procedure requires certain atoms of computation—in our case, a *lexicon* of minimal elements. But even the simplest of these pose fundamental problems: how do they relate to the mind-external world?

There are two aspects to the question: meaning and sound, the latter ancillary, if the reasoning above proves accurate. For sound, the answers lie in articulatory and acoustic phonetics. The problems are difficult. They have been studied intensively for many years, yielding some answers but leaving many outstanding problems. What about meaning? A standard answer for the core cases is provided by referentialist doctrine: the word *cow* picks out cows, maybe by a causal relation, and so forth. Something like that seems to be true for animal communication. Symbols appear to relate to physically identifiable external or internal states: motion of leaves elicits a warning cry (maybe an eagle is coming); “I’m hungry”; etc. Nothing remotely like that is true for even the simplest elements of human language: *cow*, *river*, *person*, *tree*—pick any one you want.

There are inklings of that understanding in classical philosophy, in Aristotle’s *Metaphysics*, particularly. It was considerably enriched, with a shift from metaphysics to epistemology and cognition, in the 17th and 18th centuries, in the work of British neo-Platonists and classical empiricists. They recognized that there is no direct link between the elementary elements of language and thought and some mind-independent external entity. Rather, these elements provide rich perspectives for interpreting and referring to the mind-independent world involving Gestalt properties, cause-and-effect, “sympathy of parts,” concerns directed to a “common end,” psychic continuity, and other such mentally-imposed properties. In this respect, meaning is rather similar to sound: every act of articulating some item, say the internal syllable [ta], yields a physical event, but no one seeks some category of physical events associated with [ta]. Similarly, some (but by no means all) uses of the word *river* relate to physically identifiable entities, but there

is no category of such entities identifiable in principle by a physicist investigating the mind-external world. In David Hume's phrase, summarizing a century of inquiry, the "identity, which we ascribe" to vegetables, animal bodies, artifacts, persons and their minds, and so on—the array of individuating properties—is only a "fictitious one," established by our "cognoscitive powers," as they were termed by his 17th century predecessors.¹⁰

Most of this has been forgotten, unfortunately, but there is strong evidence that it is basically correct. Once again, failure to be puzzled is a serious error. If so, these elements so fundamental to human language and thought reveal another vast chasm between humans and other animals. They pose a huge problem for evolutionary biology, and a comparably huge Poverty of Stimulus problem. The *What*, *How*, and *Why* questions raised by these systems are virtually unexplored. Their origins remain entirely unknown, and if Lewontin (1998) is correct, perhaps never will be known.

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¹⁰For some discussion, see Chomsky (1966), the first two chapters of Chomsky (1996), and particularly McGilvray (2005).

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