7 The Influence of Chomsky on the Neuroscience of Language

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What is the state of the neuroscience of language – and cognitive neuroscience more broadly – in light of the linguistic research, the arguments, and the theories advanced in the context of the program developed over the past 60 years by Noam Chomsky? There are, presumably, three possible outcomes: neuroscience of language is better off, worse off, or untouched by this intellectual tradition. In some sense, all three outcomes are true. The field has made remarkable progress, in no small part because the questions were so carefully and provocatively defined by the generative research program. But insights into neuroscience and language have also been stymied because of many parochial battles that have led to little light beyond rhetorical fireworks. Finally, a disturbing amount of neuroscience research has progressed as if the significant advances beginning in the 1950s and 1960s had not been made. This work remains puzzling because it builds on ideas known to be dodgy or outright false. In sum, when it comes to the neurobiology of language, the past sixty years have been fabulous, terrible, and puzzling. Chomsky has not helped matters by being so relentlessly undidactic in his exposition of ideas germane to the neurobiological enterprise.

The present moment is a good one to assess the current state, because there are energetic thrusts of research that pursue an overtly anti-Chomskyan stance. I have in mind here current research that focuses on big (brain) data, relying on no more than the principle of association, often with implicit anti-mentalist sentiments, typically skeptical of the tenets of the computational theory of mind, associated with relentless enthusiasm for embodied cognition, the ubiquitous role of context, and so on. A large proportion of current research on the neuroscience of language has embraced these ideas, and it is fair to ask why – and whether – this approach is more likely to yield substantive progress.

It is also fair to say that the traditional four (and now five) leading questions that have always formed the basis for the generative research program as
formulated by Chomsky have had a profound effect on research in cognitive neuroscience, although most often implicitly: (1) What is it that one knows as the native speaker/listener of a given language? That is, of course, the domain of linguistics. (2) How is this knowledge acquired? Language acquisition and psycholinguistics are at the center of this area of inquiry, as is developmental psychology more broadly. (3) How is this knowledge put to use, or processed online? Here, psycholinguistics and computational linguistics are the dominant fields. (4) How is this knowledge implemented in the brain? This issue has been addressed by the research called, variably, neurolinguistics, cognitive neuroscience of language, or neurobiology of language. (5) What is the evolutionary history of the computational language system? Paleoanthropology, genetics, and comparative ethology have played important roles addressing this more speculative question.

1 The Chomsky-Marr Challenge

The answers we consider here, namely, answers to the fourth (and fifth) questions about the neuroscience of language, depend primarily on the substantive proposals about the first question, the nature of knowledge of language, and on the acquisition and processing theories. In a perfect world, the answers to all five questions would be tightly connected and mutually constraining, to yield an explanatory model of language. Actually, in a perfect neuroscience-of-language world, researchers would be aware of – and worry about – the approach championed by David Marr for the study of the visual system. Marr (1982) famously distinguishes between the computational, representational/algorithmic, and implementational levels of description to study a complex system such as vision. The computational level characterizes the goal of the computation (say, spatial localization of a visual target to apprehend a visual scene) and what is the logic of the strategy by which it can be carried out. The level of representation/algorithm specifies how the computational theory can be carried out, including by determining what representations can form the basis for executing the algorithm that transforms input and output (e.g., Lego blocks? algebraic symbols? C++ data structures?). The implementational level of analysis asks how the representations and algorithms can be realized physically, for example, in neural tissue. The goal, a high bar no doubt, is to characterize a complex system such as vision or language by being sensitive to the demands of each level of description.

Chomsky typically endorses this Marr-ian decomposition as a research strategy, at least vis-à-vis experimental scientists (although a strict reading of Marr might conflict with Chomsky’s internalist agenda, e.g., Chomsky 1986), presumably because the strategy aligns in obvious and important
ways with the leading questions described earlier that form the basic questions for much of the generative research program since the 1950s. Broadly speaking, the computational level corresponds to linguistic theory, the algorithmic/representational level corresponds to psycholinguistics and computational linguistics, and the implementational level corresponds to the neurobiology of language. It was (implicitly by Chomsky and explicitly by Marr) assumed that the different levels would be investigated together. An explanatory theory of language that goes beyond observational and descriptive adequacy would need to capture these three levels in a unified manner. A more systematic analysis of this problem is provided in Embick and Poeppel (2015), Poeppel (2012), and Poeppel and Embick (2005), where we discuss what it would mean to develop principled linking hypotheses between neurobiology and computational-representational linguistic theories.

This is the issue: if we take seriously the linguistic primitives postulated by research in the generative tradition for the past 60 years, we end up with a list of basic operations and representations that has been highly successful at accounting for a wide range of (cross-)linguistic phenomena. These primitives include, say, “distinctive feature” and “morpheme” as well as basic operations such as “feature spreading” and “concatenation.” Analogously, the neurosciences traffic in basic elements equally well supported, including, for example, “dendritic spine” or “cortical microcircuit” as well as “long-term potentiation” or “synchronization.” The problem one faces is that one cannot “draw lines” connecting the putative primitive categories provided by the two domains and expect such an alignment to make sense or withstand scrutiny. There is no known—or even vaguely plausible—alignment between the primitives posited in these areas of inquiry. However, without any way of aligning the different theories’ primitives, it is impossible to unify neuroscience and linguistics.Mappings between the domains would be, at best, correlational. Correlational data are a positive result but do not constitute anything remotely like explanation. Ultimately, we seek causal (and mechanistic) explanations for particular linguistic representations and computations. We want to unify the computational level of description (the inventory of basic linguistic computations) with the implementational level.

We lack hypotheses, perhaps algorithmic-level hypotheses, to generate principled links between the domains. In previous work, Poeppel and Embick (2005) diagnosed two problems that stand in the way. The first is the Granularity Mismatch Problem (GMP): a mismatch between the “conceptual granularity” of the atomic concepts of linguistics and the atomic concepts of neurobiology. The mismatch hinders the formulation of theoretically motivated, neurobiologically grounded, and computationally explicit linking hypotheses. The second problem is the Ontological
Incommensurability Problem (OIP): the primitives of linguistic theory cannot be reduced to the fundamental units currently identified by neuroscience. This problem results from a failure to be able to answer the question of how neurobiological structures (as now understood) could be constructed and specialized for performing specific types of computations, linguistic or otherwise. Although the focus here is on language, the GMP and OIP apply to all domains of investigation in cognitive neuroscience in which the relationship between cognitive systems and neural circuitry is sought.

For linguistics, a straightforward approach to the GMP and OIP would spell out the ontologies and processes in computational terms at the appropriate level of abstraction. A variety of strategies are possible here, for example, taking primitive operations such as “concatenate” and working to identify the neural mechanism. Poeppel and Embick (2005) suggest a program of research in which using linguistically motivated computational categories could support the study of computation in the brain. In short, rather than pursuing the typical approach in which linguistically postulated categories must be validated by neurobiological data, we recommend taking linguistic categories seriously and using them to motivate studies about how the brain computes with abstract categorical representations. This perspective advocates an integrated approach to the study of linguistic computation, such that linguistic theories are accountable to all forms of evidence, that is, psycholinguistic and neurolinguistic findings. (For an argument with a similar flavor, discussing specifically a cross-level approach to the lexicon, see Boeckx and Theofanopoulou 2014.) The challenge of this research program is to further decompose the hypothesized atoms of language into computational primitives likely to be reasonable in investigating neural computation. For example, an operation such as “concatenate” might be tractable, whereas an operation such as “label” is not (but see Murphy 2015). These are empirical questions. In any case, progress on this line of research would advance two intellectual challenges set forth by Chomsky since the origins of the research program, namely, identifying the formal building blocks of the linguistic computational system and striving for unification with the disciplines that are necessary for a full accounting of the language faculty.

Departing from these (to-date largely aspirational) goals, we turn now to how research has actually progressed in the cognitive neuroscience of language, because the hallmark is (most typically, as I see it) the profound disconnection between the insights of the language sciences and those of the neurosciences. In my view, attention to this challenge – or the failure to attend to it – has led to work on the neuroscience of language that is either truly insightful, outright bad, or orthogonal and irrelevant.
2 The Pursuit of the (Very) Broad Questions

One perspective from which to read and interpret the history of research on the neuroscience of language is to recall the broader assumptions that arose from the research program as formulated sixty years ago. One fundamental assumption is the notion of species specificity. Barring gross pathology, all children learn to speak, even with limited input, but no pets do the same way we do, even garrulous ones such as vocal learning birds. Because even animals that are very close to humans in terms of their anatomy and physiology do not use a comparable language system, a search for “special areas” of the brain seemed like a straightforward approach. This particular research program is difficult to pursue, because it is not at all clear what to look for – or what is absent. Is the famous Broca’s area (a region in the left inferior frontal lobe defined in 1861) a fully specialized language region seen only in humans? (No.) Are there islands of tissue that are reserved in humans for language? (We still do not know.) Once one looks beyond macroanatomic features, it is difficult indeed to discern what neural circuitry might be unique to humans. The obsessive interest with what is different, unique, and species specific – or, as is currently popular, the equally obsessive interest in showing that there are no significant differences between human and nonhuman systems – has not led to a rich body of results that teach us how the brain makes possible language. Many controversies are celebrated but few issues illuminated.

There are, to be sure, examples of careful research attempting to investigate reasonable questions that have arisen from this tradition. For example, interest has been currently renewed in this species question in the context of defining fiber tracts in the brain. Maybe the connectivity of the human brain differs in systematic ways, and maybe the human “wiring diagram” (the human “connectome”) will reveal crucial differences. One area of active research concerns fiber tracts connecting more posterior sensory areas to more anterior motor areas. These investigations tell us some interesting facts about anatomy (e.g., Rilling et al. 2008) and provide insight into the mapping between perception and action (i.e., listening and speaking), but they are not yet particularly informative about knowledge of language and how it might be implemented.

Nevertheless, identifying the special areas of computational infrastructure can be an interesting program of research and is actually currently developing in innovative ways, with an emphasis on the basic computation that neural circuits provide, rather than specialization of one form or another across species. The research has, thus, changed from species specificity (old school) toward trying to learn more details about the properties of neural circuits, on the assumption that much in terms of circuit properties is shared across species (new school, with a focus on mechanisms). In fact, neuroscience models of
human visual perception or human memory are both deeply influenced by insights from animal preparations. Of course, the language system differs in ways that make this research more problematic, but a finer-grained computational analysis will help.

In sum, the older approach to species specificity has led to much research on animal communication, animal concepts and categories, and animal “language” (for some discussion, see Petitto 2005) — and to many debates and acrimony — but the arguments have not been particularly insightful concerning the neurobiological mechanisms of language. The newer research departs in notable ways and asks more nuanced questions, for example, can we learn something about the neural circuits that in humans support functions X, Y, or Z by investigating relevant comparable structures and functions in animals. Unsurprisingly, more progress is likely (and is, in fact, observed) on topics that deal with the sensorimotor interfaces, rather than on questions of, say, lexical semantics or syntactic operations. For example, work on the structure of nonhuman auditory systems and how they might generate phonetic-like representations (e.g., Mesgarani et al. 2008; Petkov et al. 2006; Rauschecker and Scott 2009) has been informative in the attempt to understand how the human auditory system generates phonetic and phonological codes (e.g., Mesgarani et al. 2014).

A second topic that has led to a number of studies on the neuroscience of language concerns domain specificity. Domain specificity is a theoretical conjecture primarily discussed in the cognitive sciences that argues that distinct cognitive domains — such as language or vision or reasoning about specific areas of experience — are subserved by specialized functions, regions, learning mechanisms, data structures, and so on. Approaching the neurobiology of language from that perspective has led to a large body of work aiming to identify how or to what extent neural structures and computations are separate from other domains. Much like the search for specialized properties in the human brain that differ from those of other species, this research has looked for properties within the human brain that differ between language and, say, memory, attention, vision, and so on. The computational system that we call “language,” composed of different subroutines (that differ as a function of precise theoretical proposals) is, in the original Chomskyan conception, domain-specific. For example, notions such as X-bar representation, case-theoretic assumptions, or movement, as developed in the standard theory, are by hypothesis completely specific to language. Later theoretical proposals in the context of exploring Minimalist Program concepts identify ever fewer species- and domain-specific operations, thus ending up with a conceptualization of the narrowly construed faculty of language (FLN) and the broadly construed faculty of language (FLB) that encompasses a range of mechanisms that can contribute to other
domains and even function in other species (Hauser et al. 2002). This area of study has been tremendously controversial, with some researchers arguing vigorously for specialized neural mechanisms and others arguing as vigorously for general mechanisms that are not restricted to language. The difficult part of these debates – from the neurobiological point of view – is that the domains under consideration are quite broad relative to the mechanisms one attempts to study as a neuroscientist; that is to say, “language,” considered as an undifferentiated, monolithic cognitive domain, is arguably not domain specific, since it draws on memory, attention, and other cognitive and perceptuo-motor capacities (cf. FLB). However, once one decomposes language more carefully, the issue becomes subtle and biologically interesting. Domain specificity, in other words, is a fruitful conceptualization for neurobiological inquiry only once one gets to a computationally decomposed view of language in which the granularity mismatch discussed at the outset is directly addressed. Such a view, discussed in Sections 4 and 5, is congenial to the biolinguistic research program.

One special perspective on the domain specificity debate deals with the concept of *modularity*. This notion was defined and discussed for the cognitive sciences by Fodor (1983), and it has played a huge role in the cognitive and neurosciences. In the context of the issues under consideration here, the interpretations of the term “modularity” differ between the cognitive and neurosciences in a way that has obscured progress. In the cognitive sciences, a key feature is the idea of specialization and *informational encapsulation*, that is, that certain perceptual or linguistic computations, by virtue of the representations (e.g., “morpheme”) and computations (e.g., “concatenate”) they build on, are encapsulated from (or impenetrable by) information from other domains. This is a clear hypothesis and has been explored across a range of linguistic phenomena. On balance, the experimental data have favored a view that few if any processes are truly encapsulated. This is not uninteresting from a biological point of view because it suggests that cortically mediated processes are typically not sharply demarcated; this is quite different from a variety of subcortically mediated processes (e.g., sound localization) that are highly specialized and modular.

The neurobiological perspective, however, views modularity as anatomic modularity – and therefore argues for functional isolation by virtue of the absence of neuronal connectivity. Now, the brain, and in particular the cerebral cortex, is massively interconnected, in both feedforward and feedback directions, so that type of modularity would be hard or impossible to observe. Consequently, the standard argument against functional modularity from neuroscientists is that anatomic non-modularity speaks against functional modularity. One can see why neuroscientists might be seduced by such connectivity-based arguments – and it underscores the perverse fascination
with studying the connectome – but it is worth remembering that the wires alone do not suffice as an argument. It depends on the representations (the informational content) and the specific computations over representations. The very same set of wires can be operating over different representational primitives and executing or transmitting distinct neural codes (a rate code, i.e., a neural code carried by the activity of cells per unit time, say, carried over short time constants versus a temporal code carried over long time constants can be accommodated in the same neural structures). The circuit details and the neural code are such that functional modularity could be preserved. We simply do not know. In any case, in many cases of language processing, it turns out that surprisingly “distant” information is brought to bear on putatively modular functions (see, e.g., Lewis and Poeppel 2014 for an example concerning the low-level ingredients of lexical access). In other cases, the details are not sufficiently well understood.

From the perspective of neurobiology, too many discussions about domain specificity have led to parochial and ultimately unhelpful battles. As linguists have articulated the operating principles of the language system with increasing specificity, the discussions about how such elements might be neurobiologically implemented became baroque and not insightful about biology. It is in this sense that Chomsky’s (indirect) contribution to neuroscience has been less than optimal. As the approach developed from the rule-based work of the 1950s and 1960s to the principle-based research of the 1970s and 1980s, many studies tried directly to link new theoretical proposals to cognitive neuroscience. In the context of these studies, neither neuroscientists nor linguists were particularly helpful, and somewhat entrenched ideological fights ensued. An interesting case study comes from the research of Grodzinsky and colleagues on the possible role of Broca’s area for the representation and computation of movement and displacement in syntactic structure and processing (e.g., Grodzinsky 2000). Grodzinsky argues for a sophisticated and subtle view of the relation between neural structures and this linguistic operation. The hypothesis tested is that this operation is one domain-specific basic computation that is supported by Broca’s region. (Currently, related debates are being carried out concerning the notion of Merge, another – or perhaps the – putative basic operation.) The embedding of this approach into decidedly domain-specific linguistic rhetoric, paired with the relatively coarse neurobiological analysis, has prevented ideas such as “movement is a basic computation” from being examined more carefully and extensively. The current fate of the proposal exemplifies the failure of more radical interdisciplinary thinking. Neuroscientists cannot suspend their disbelief and accept “movement” as a potential basic computation that merits investigation as being implemented in some neural circuit; linguists, similarly, are unable to translate the conceptual architecture into terms permitting the statement of approachable linking hypotheses.
Regardless of where one falls along the ideological divide, it is clear that the fundamental questions of species specificity, domain specificity, and modularity have played a large role in stimulating a wide range of research on neuro science. The extent to which we have satisfying mechanistic answers to neurobiological questions is not so clear; my own view is that these questions are asked at the wrong level of abstraction to forge a link in systematic and principled ways between linguistic proposals and neurobiological infrastructure. Nevertheless, the original framing of these questions has been enormously influential.

3 The Pursuit of the (Medium) Broad Questions: Textbook Organology

In the past 25 years, research on the neurobiology of language has changed in dramatic ways in light of the advent of modern neuroimaging technologies. Until 25 years ago, the main insights about brain organization had come from deficit-lesion correlation research on patients. Such neuropsychological data yielded a number of fundamentally important insights. However, neuropsychological data have been more informative about adjudicating between cognitive theories than about neurobiological organization. Neuropsychological data—because of their relative anatomic and physiological coarseness—have not been very informative about detailed mechanisms of biological organization. If this is so, what has constituted progress in this domain?

Two factors play a critical role. On the one hand, the development of noninvasive recording tools has enabled researchers to investigate the intact human brain online, during the processing of linguistic information. At the same time, cognitive neuroscientists have come to appreciate the componential analysis of language processing that linguistic research has supported. Nineteenth-century neuropsychology worked on notions such as “speech production” or “comprehension” as more or less undecomposed wholes. Modern neuroimaging research is now sensitive to distinctions made in language research. Specifically, distinctions between linguistic subdomains now form the basis of many if not most neuroscience experiments. Typical studies seek to identify the “brain basis of syntax,” or the “regions underlying semantics,” or the “cortical network supporting phonology,” and so on. In fact, since the late 1980s, such studies have dominated the literature and have added substantial new insights to our understanding of brain organization.

Electrophysiological tools such as electroencephalography (EEG; available for many decades) and magnetoencephalography (MEG) have enabled experimenters to probe language processing with superb temporal resolution (milliseconds), if with a limited spatial resolving
power (~1 centimeter). In parallel, hemodynamic imaging approaches such as functional magnetic resonance imaging (fMRI) form the basis for many hundreds of studies executed with high spatial resolution (~millimeter) but more limited temporal resolving power (~seconds). The cumulative effect of the availability of these two types of techniques – paired with a more decomposed and well-informed view of language processing – has yielded what we may now consider the state of the art, or the “textbook organology,” as reflected in the current summaries (see, e.g., Hickok and Small 2016 and Kemmerer 2014 for excellent textbook treatments of the state of the art).

The classical view of brain and language, illustrated in numerous textbooks for more than 100 years and dominant in clinical neurology until recently, encompasses a left hemisphere in which a frontal region (Broca’s area) and a posterior temporal region (Wernicke’s region) are the two main regions, connected by a fiber bundle (the arcuate fasciculus). Current data illustrate, in contrast, the many additional brain regions that are robustly implicated in the execution of language processing and highlight, as well, the critical importance of right hemisphere structures. There exist hypothesized networks of regions for syntactic processing (e.g., Bornkessel-Schlesewsky and Schlesewsky 2013; Friederici 2011; Vigneau et al. 2006), semantic processing (Lau et al. 2008), speech processing (Hickok and Poeppel 2007), and other functions. Research that I, too, have participated in with my colleagues has contributed to this strand of investigation, and one can reasonably say that most current studies build on functional anatomies that show distributed networks underpinning various segregated language functions. Large-scale models generated in the past 25 years, for example, based in large part on the new insights coming from neuroimaging, highlight many cortical and subcortical areas in both hemispheres, with streams of information processing (so-called dorsal and ventral stream models) responsible for separate subroutines.

The question one might ask, given the concepts developed in a Chomskyan approach, is whether such analyses suffice. Is this the level of biological characterization that yields mechanistic causal accounts (i.e., explanations) of the brain basis of language processing? It is by now reasonably well accepted that this is still an overly coarse granularity of analysis. Discussing the brain implementation of “phonology” or “syntax” or “semantics” does not yet bring to the question the right level of pieces of elementary processes to generate neurobiologically sensible answers. In what follows, I describe this issue briefly. This approach is strongly influenced by the generative tradition, and specifically the desire to identify the smallest, “primitive” or “atomic” representations and computations.
4 The Computational Neurobiology of Language

This section presents some examples of neuroscience research that are rather more directly motivated by Chomsky’s overall approach as well as some of his specific theoretical proposals. There are, broadly speaking, three flavors of this research: one line of work that has been pursued in recent years concerns computational proposals that are typically subsumed under the locution formal language theory (cf. Chomsky hierarchy). A second line of work is motivated by several themes running in Chomsky’s approach since the beginning of the generative research program, namely the role of cyclicity; the role of statistical information; and the role of structure, hierarchy, and abstraction. A couple of studies illustrated later address abstract structure and hierarchy. A third line of research, newer and not yet well formed, concerns the aim often articulated since the 1950s: to identify basic operations and representations. This last issue has been a consistent theme, but its implications for neuroscience are only recently being appreciated. The complications of linking computational linguistic primitives and neurobiology will be outlined. The first two research directions are illustrated here.

Several neuroscience experiments address relatively (perhaps even surprisingly) broad questions about the nature of language that are motivated by fundamental questions formulated by the approach first laid out in the 1950s and 1960s by Chomsky, in the context of research on formal language theory. The first line of research concerns experiments focusing on the overall computational nature of the language system. Discussions of species specificity and domain specificity have led to studies testing whether there exist structural and/or functional neurobiological data supporting differences between the processing of finite-state grammars and phrase-structure grammars. In the experimental literature, this is (for better or for worse) taken as a principled cut differentiating types of operations that the human language system performs and needs versus operations subsumed under finite-state automata theory that many creatures, by hypothesis, have access to. An important distinction is made in these studies between ways of generating and processing purely sequential structures (e.g., ABABAB . . . ) and the requirement to process hierarchical structure in which sequence information alone does not suffice (as is typical of human language; for discussion of many studies investigating this issue in various systems, see Fitch and Friederici 2012 and Fitch 2014).

Friederici and colleagues have performed influential studies examining these concepts from formal language theory (e.g., Friederici et al. 2006). A conjecture advanced by this research is that there exist structural features (i.e., properties of a brain region) and connection-based features (i.e., properties of a region’s connectivity pattern) that underlie the specialization for
language processing. These authors used neuroimaging data, in particular anatomic tract-tracing data (diffusion tensor imaging) paired with functional activation data, to argue that different regions in left inferior frontal cortex—and different connectivity tracts or pathways originating/terminating in these areas—separately underlie finite-state processing capacities (say building a local constituent) and phrase-structure-grammar types of processing constructing hierarchically structured sequences. On this view, Brodmann area 44 in the left inferior frontal cortex (part of the traditional definition of Broca’s area) has the capacity to build complex, nested, hierarchical structures; in contrast, the (phylogenetically older) frontal operculum (a near neighbor, anatomically) supports “mere” sequential structure building. Since these cortical regions with these specific connectivity patterns are to date only seen in the human brain, it is argued that these regions, specifically Brodmann area 44 and its partners in the superior temporal gyrus (STG), constitute a specialization for the internal grammatical computational system humans use.

The second line of research on computational operations in language (the second flavor) is well exemplified by recent experiments examining the fundamental notion of constituency and structure building. One would think that this is a settled matter, given the extensive evidence in theoretical linguistics, psycholinguistics, and language acquisition for the relevance of constituent structure, hierarchy, and abstraction. Although the concept of constituency, for example, is by and large taken for granted in the cognitive sciences in general, and in language research in particular, how such an elementary aspect of linguistic computation is implemented is not understood. As a consequence, there has been a growing body of experimental research in the neurosciences evaluating this fundamental concept. Constituent structure is, of course, assumed to be at the very basis of building interpretations; it is rather remarkable that even now it is a poorly understood—and in some circles even controversial—issue, that is, where and how in the brain this calculation is executed and represented.

One important paper describes an imaging study that creatively addresses this crucial ingredient of language processing. This experiment attempts to characterize which areas of the brain are particularly sensitive to the operations underpinning the construction of constituents. Pallier, Devauchelle, and Dehaene (2011) used fMRI to identify brain regions that are selectively sensitive to the size of constituents. Their rationale was that as constituent size increases, more neuronal ensembles would be recruited to process this information. For example, a constituent such as “James’s table” would require fewer representational and processing resources than a constituent such as “James’s father’s wooden table.” The increased constituent size goes along with increased complexity, and so they investigated
the straightforward prediction that there exist brain regions reflecting such constituent-based processing. Participants were presented with word sequences always containing 12 items, but the structure of the sequences was parametrically manipulated such that sometimes there were sequences of two-word constituents, sometimes the constituents contained four words, sometimes six, and so on. By way of a clever analysis of the neuroimaging data, the authors were able to identify a group of brain regions selectively activated by – and differentially sensitive to – the existence and size of constituent structure. These regions included tissue in the left inferior frontal lobe, as might be expected from the various previous studies on brain and language. Interestingly, though, a number of regions along the temporal lobe, in the superior temporal sulcus (not restricted to the poorly defined “Wernicke’s area”) were especially relevant for this linguistic computation. The data also succeed in making important distinctions between abstract structure building alone (e.g., by way of functional, closed class items that “work” even when hearing uninterpretable Jabberwocky) and processing in the context of semantically interpretable information. Aspects of the important Pallier et al. (2011) experiment motivate further study to bolster and extend these findings. First, the experiment was performed using written materials, that is, participants rapidly read serially presented words. Second, the method of study was fMRI, a technique with excellent spatial resolution but limited temporal resolution, as mentioned earlier. An experiment by Ding et al. (2015) investigated the issue from a closely related perspective but used spoken language comprehension in the context of MEG and electrocorticography (ECog) neural recording to obtain evidence for hierarchical structure building. This study was motivated by the presupposition that the combinatory potential of language derives from the fact that the operations joining elements can occur recursively, generating a hierarchy of linguistic structures, for example, words (colorless), phrases (colorless green ideas), and sentences (colorless green ideas sleep furiously) (Chomsky 1957). Despite the fundamental importance of hierarchical linguistic structure for comprehension, the dominant carrier of language, that is, auditory speech, does not regularly and unambiguously encode hierarchical structure by any physical cues. Yet, the speed of language processing forces online, incremental building of internal representations of hierarchical structures, which can only be achieved reliably by deploying one’s tacit linguistic knowledge.

Ding et al. capitalized on a range of new electrophysiological studies and analytic niceties that show that oscillatory brain activity (roughly, neural rhythms of different frequencies) entrains to – and therefore tracks – input information in a faithful way, including speech (Ding and Simon 2012; Luo and Poeppel 2007; Park et al. 2015; Peelle et al. 2013) and music
(Doelling and Poeppel 2015). The study uses carefully crafted materials in Mandarin and English in which word rate changes at one (faster) rate, for example, 4 Hz. The materials are then sequenced such that local phrases (e.g., “long papers”) occur at a slower “phrasal” rate, for example, 2 Hz, and sentences (e.g., “long papers take time”) at a rate of, say, 1 Hz. The study tests whether neural entrainment and tracking are not just visible for physical aspects of the input (i.e., spoken syllables/words at 4 Hz) but also for abstract structural information that is only encoded structurally. It is shown that when listening to connected speech, brain activity is entrained to linguistic structures at different hierarchical levels concurrently, including words, phrases, and sentences. Importantly, entrainment to larger linguistic structures such as phrases and sentences was not confounded by the encoding of acoustic cues or by statistical cues signaling transitional probabilities. Moreover, the cortical dynamics generalized across languages as well as across sentence durations and syntactic structures. The conclusion of this study is that the online neural codes underlying the construction of hierarchical abstract structures occur on time scales commensurate with the building of the structures, and that neural oscillations provide a window onto investigating how tacit knowledge is deployed in online computation in the brain.

It is worth remembering here that one surprising anatomic aspect of many of these studies binds them together in an additional way – beyond the underlying motivation to investigate fundamental aspects of the neurobiology of language. These studies systematically implicate brain regions and brain mechanisms that are not part of the traditional, classical view of how brain and language are organized! The canonical textbook view has had a stronghold on the popular and scientific imaginations. But many recent studies, including the ones mentioned here, implicate areas of the brain that are not part of the classical view. For example, the fact that left anterior temporal lobe regions appear to be critical for aspects of conceptual combination in language (Bemis and Pylkkänen 2011) was unanticipated; the now well-supported role of the right hemisphere also contrasts with standard assumptions. Furthermore, neurophysiological hypotheses, such as the conjecture that oscillations play a logistical role in supporting language processes (Giraud and Poeppel 2012), are recent insights from ideas incorporated from other parts of neurophysiology. In other words, the emerging view from contemporary experimentation departs in considerable ways from what the traditional view has conveyed. And these new insights are in important ways stimulated by pursuing the fundamental questions outlined originally in the generative tradition, that is to say, linguistic research of the past 60 years. Crucially, its foundational aspects, if not the momentary developments of a technical nature, have led to important new insights that depart from historical convictions.
5 Revisiting Linking Hypotheses and Proper Granularity: Big Data versus Specific Data

The third line of research concerns an aim often articulated since the 1950s: to identify basic operations and representations that underpin knowledge of language, the acquisition and processing of this knowledge, and then linking these results to neuroscience—the implementational level of description, sticking with Marr’s terminology. These considerations have not yet resulted in a substantive body of empirical work in the neurosciences and should therefore be thought of as the “aspirational computational neuroscience of language.” But it is worth speculating where this research might lead—and contrast such a decompositional approach with alternative research strategies that are currently popular.

One widely and enthusiastically endorsed recent approach to the neuroscience of language might best be described in direct opposition to the idea of going after the atoms of language (and being splitters), namely, studying the system in a purely data-driven fashion without preconceived, theory-driven distinctions. This “lumper approach” has some merits, of course. One complaint about linguistically and psycholinguistically inspired neuroscience is that the experiments are typically not naturalistic (relying on artificial experimental situations using crafted materials that can seem just plain weird), not contextually embedded (but based on, say, single sentences in single sensory modalities in single participants), and driven by selective, narrowly construed data streams. The worry is that comprehensive accounts of brain and language must answer to a wide range of data and phenomena, and the restricted, theory-driven approach practiced by splitters might lead to underspecified (at best) or totally misleading and fundamentally misguided (at worst) models of brain and language.

The most radical lumper view (actually put into practice in research such as the “speechome project”; Roy et al. 2006) capitalizes on the orgy of data that current recording technologies afford: place people in natural, socially interactive contexts and record as much as possible of the ecologically valid, contextually rich, multisensory linguistic experiences that people have. Instead of single participants, study dyads or entire groups; instead of single sentences, words, or syllables, use “real” multisensory, contextually embedded information; and instead of weird experiment-o-centric constructions (like, say, sequences of sentences with weak crossover violations), study the utterances that people use. (Neuroimaging and neurophysiology experiments along these lines study conversing participants in different scanners or multiple interacting participants wearing EEG recording equipment.) Operating on the crucial presupposition that the exploration of the data will yield emergent patterns of some sort that then have some relation to language processing, the massive
amounts of data acquired are then mined for patterns. Machine-learning approaches might explore the data by seeking to identify some abstract similarity space, attempting thereby to generate broad classes of data that cohere along some dimension that is not a priori specified. The interesting aspect of such techniques is that truly enormous amounts of data are considered, a technical feature that the currently available computing power makes possible for the first time.

This way of investigating a biological problem is, however, a clear departure from standard scientific practice. It relies critically on the belief that so-called “big data” – when properly mined – allow the identification of relevant dimensions of the domain based solely on hypothesis-free, data-driven interrogation of data. These new approaches have a certain seductive appeal because the “cutting-edginess” of the quantitative methods used have exciting engineering applications – and are just plain fun. Whether such research tactics yield insights that generate even a descriptively adequate characterization of the domain is totally unclear, however. Chomsky in lectures occasionally makes a somewhat unkind but very funny remark on such research strategies: something along the lines of “it’s like pointing a camera out the window and recording the weather – and from that developing a theory of meteorology.”

The big-data-plus-machine-learning-plus-correlation-to-heretofore-unseen-structures strategy reflects a modern research aesthetic: assume (in my view optimistically, at least for the domain of language) that correlational structure in data identify dimensions that offer mechanisms and potential explanations. It makes epistemological assumptions that, ultimately, must be judged on the merits of the research successes obtained. Can (largely) hypothesis-free, data-driven research actually find the mechanisms that underpin the representational and computational infrastructure of language? More provocatively, will the constructs that such an approach determines bear any relation to what we currently believe to be the structure of the domain? We will have preliminary answers to these issues within the next few years. If the research of the past 60 years has been at least in part sensible, one tempting prediction is that what will be (re)discovered by big-data-plus-machine-learning are well-motivated concepts such as “distinctive feature,” “syllable,” “morpheme,” “intersective modification,” “linearization,” and so on. It would be fantastically exciting, of course, to discover either completely different primitives or even discover that there is no defensible “parts list” of language.

Because it is a wonderful luxury to have access to enormous amounts of data, and because it is a privilege to be able to draw on the formal and quantitative expertise of many related disciplines in the analyses (physics, mathematics, computer science, engineering, neuroscience, statistics), it is likely that many
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engineering “solutions” will be identified. This alone will be valuable: the lumpers will, almost certainly, make tremendous progress on quantitative methods. What, in the meantime, will splitters do? One (already widely practiced) approach is to adopt existing splits (as listed earlier, i.e., “feature,” “morpheme”) and design experiments that investigate how the brain computes with such objects. Such a research program capitalizes on the fact that a substantial body of evidence for a concept such as “morpheme” exists and it behooves the neuroscience community to identify the neural circuitry that underpins this object. In these types of studies, the data are highly specific and relatively narrow. Experiments are designed to manipulate attributes of such an object and test how the object enters into processing. A thoughtful and thorough research program in this line has been pursued on morphology (e.g., Fruchter and Marantz 2015). The questions include foundational issues such as whether or not morphemes are in fact primitives and then whether and how they enter into various linguistic processes, say lexical access or composition. The accompanying cognitive neuroscience studies aim to (i) adjudicate between theoretical alternatives and (ii) identify the neural locations and processes that form the basis for the morphological effects.

This research is, in my view, highly successful as cognitive science and cognitive neuroscience research – but we learn only limited amounts about the implementational level of description itself: the relation between the neural data and the linguistic phenomena remain in large part correlational. Splitters moved by Marr-inspired decomposition and the achievements of linguistics might aim, in addition, to connect even more directly to neuroscience and pursue identifying hypotheses linking the putative primitives of linguistic representation and computation and the primitives of neuroscience. To overcome the joint challenges of the granularity mismatch problem and the ontological incommensurability problem discussed at the outset, it seems practical to consider how the hypothesized primitives might be further decomposed to yield to a kind of “circuit-level” linguistics. For example, if an elementary operation such as Merge requires a concatenation step and a labeling step, are these the types of minimal operations that could be cashed out as a circuit that could be neurophysiologically implemented? And what other operations might be relevant to investigate? The task of linguists is, in my view, to put forth a well-motivated parts-list of atomic representations and operations and further decompose these into steps/primitives that might be implemented. It is then the role of neuroscientists to identify how such primitives can be neurobiologically supported. We do not yet know the biological answers. Maybe neural oscillations play some logistical role, maybe a critical feature lies in single-neuron firing rates; maybe cell-internal genetic mechanisms are more relevant – the
mechanisms have not been identified, and we do not even know what exactly we (as neuroscientists) are looking for.

Finally, the area that is in some important sense understudied considers the stored items. What exactly is stored, how, and where in the brain? While fascinating insights derive from better understanding of the basic operations underlying language processing, the fact that we can store tens of thousands of items – and retrieve and use them in a remarkably fast fashion with a low error rate – is perhaps as fascinating, and it is an attribute that also sets us apart quite sharply from our nearest evolutionary neighbors. Again, one can be a lumper or a splitter about this issue. The results of the past 60 years suggest that the knowledge reflected in stored linguistic information is rich, subtle, structured, and abstract. This invites experiments of the splitting variety, being sensitive to the complex information associated with stored items. Research on lexical storage, too, has been heavily influenced by big-data, context-dependent, and embodied approaches, but explanatory models that link neurobiology and the subtleties of linguistically relevant stored information are also not yet on offer. This area of investigation, too, merits more attention. Studies on what the stored primitives are should make more substantive contact with linguistics – a problem that can be solved. However, there exists, for now, a real showstopper: we do not have even the foggiest idea of how information of any type is actually stored in the brain (notwithstanding descriptive and largely metaphorical statements on patterns of synaptic connectivity, which begs the question); until this truly fundamental problem is solved, comprehensive and satisfying theories of brain and language will remain elusive.

References


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