Feature Review

NEUROLINGUISTICS AND NEUROIMAGING:
Forward to the Future, or Is It Back?

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When the great 1950s experiments on the neurophysiological basis of vision were published, they generated enormous excitement. Lettvin, Hubel, Wiesel, and their colleagues showed that certain neurons respond selectively to stimuli, and that these neurons are organized in anatomically identifiable regions (Hubel & Wiesel, 1959; Lettvin, Maturana, McCulloch, & Pitts, 1959). These results motivated scientists to try and relate the visual world to the structure of the central nervous system. Some 40 years later, we have a well-articulated research program, with a dense body of relevant empirical knowledge. Despite debates about the interpretation of the results, there has never been, to my knowledge, a serious challenge to the kinds of stimuli the original studies used. At the level of basic visual operations, at least, there seems to be substantial agreement on the characterization of the problem, on units of analysis, and on what counts as an empirical argument. Lines are lines, angles are angles, and edges are edges—elemental parts that quite clearly play a constitutive role in forming our visual experience.

This relatively happy state of affairs does not characterize the neuro-cognitive exploration of language. Linguistic capacity critically involves the pairing of sound sequences and meanings, with the aid of an inventory of combinatorial rules, and stores of complex objects of several types over which these rules operate as language is practiced through its various modalities. It is quite difficult to home in on units that would be sufficiently elemental to have simple shapes (say, on a par with lines, edges, and corners in vision), and yet be complex enough to count as parts of our linguistic (as opposed to articulatory, auditory, or visual) system. As a consequence, a consensus (one that would hopefully reflect understanding) is hard to reach. A concrete example, from the study of the genetic basis of language, might be a good place to begin.

The logic behind this enterprise goes thus: Aspects of linguistic behavior must be part of our biology. If so, then something related to this behavior must be genetically determined, hence encoded; as such, it is subject to the same considerations that apply to other parts of the human genome. In particular, genes subserving linguistic ability should be sought. The genomes of individuals who suffer from language disorders that appear genetic are prime candidates for this study. Such a research program, in fact, might broaden the horizon of Chomsky's innateness hypothesis, in that it might provide tangible (rather than conceptual) reasons to believe the claim that linguistic capacity is part of our genetic endowment.

The discovery of the KE family, whose members' phenotypes manifest a severe disturbance of speech and language, gave linguistic geneticists a unique opportunity (Gopnik & Crago, 1991; Hurst, Baraitser, Auger, Graham, & Norell, 1990). The disorder seems to follow from an autosomal dominant trait: Half the members in three generations (15 out of a total of 31 individuals) are affected; the offspring

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Cognitive Neuroscience: A Reader, ed. by Michael Gazzaniga. Malden, MA: Blackwell Publishers, 2000. 536 pp. Cloth, $77.95; paper, $45.95.

Image, Language, Brain: Papers From the First Mind Articulation Project Symposium, ed. by Alec Marantz, Yasushi Miyashita, and Wayne O'Neil. Cambridge, MA: MIT Press, 2000. 272 pp. Cloth, $55.00.

The Neurocognition of Language, ed. by Colin Brown and Peter Hagoort. New York: Oxford University Press, 2000. 430 pp. Cloth, $95.00; paper, $39.95.

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of an unaffected member are unaffected; and half the offspring of an affected member are affected. The sex distribution is 9 males and 6 females, suggesting that the disorder is not sex linked (Gopnik & Crago, 1991; Hurst et al., 1990; Vargha-Khadem et al., 1998; Vargha-Khadem, Watkins, Alcock, Fletcher, & Passingham, 1995; Watkins, Gadian, & Vargha-Khadem, 1999). A series of highly sophisticated molecular and bioinformatic techniques successfully linked the disorder to a region on chromosome 7 with a reasonable degree of confidence, thereby making progress "toward the isolation of the first gene to be implicated in the development of speech and language" (Lai et al., 2000, p. 367; see also Fisher, Vargha-Khadem, Watkins, Monaco, & Pembrey, 1998).

This set of results may be a major breakthrough toward the discovery of the biological basis of language, for it appears to establish a direct link between language mechanisms and a genomic locus. But wait a minute: Are we clear about what the discoverers mean by "a severe disturbance of speech and language"? What is the nature of this disturbance? What tests were used to determine its character, why were they chosen, and what exactly do the scores indicate? These are critical questions, because the linking of a chromosomal abnormality to a behavioral aberration gives a clear picture if and only if both sides of the equation are precisely characterized and understood.

Unable to judge the molecular side, I restrict my discussion to behavior. The complexity of language and linguistic behavior makes myriad disorders imaginable. Upon examination, the problem in the KE family remains less than clear. Researchers have administered a fairly wide variety of speech, language, and other cognitive tests in an attempt to distinguish impaired from healthy members of this family. Yet among the many tests (e.g., memory, lexical decision, naming, grammar, and meaning tests), only three established "a core deficit, or a characterizing phenotype": Word Repetition, Non-Word Repetition, and Simultaneous and Sequential Orofacial Movement to Command. On these tests, effects were "shown by every affected member but not by any who [were] unaffected" (Vargha-Khadem et al., 1998, p. 12596). What was the nature of the errors, and are they likely to be interpreted as linguistic? In other words, how does the description of the behavioral deficit connect to the way language knowledge and use are

viewed by theoreticians? This critical information is somewhat lacking. The situation, in fact, is further complicated, as there are many more tests on which the affected members averaged lower than their unaffected peers (although these did not meet the criteria of the "core deficit"). How can these results be interpreted? Finally, do these findings provide a clear enough picture to conclude that the "*SPCH1* region" on chromosome 7 is a "language gene"? For the reasons just given, I tend to think that this conclusion is premature, and commend the caution of the discoverers, who aptly claim to have made a mere first step.

One could imagine the situation to be different: A consensual view on what language is, and a subsequent battery of tests, might determine with some certainty whether or not a disturbance is truly linguistic, and lead to a clearer view of the genetic basis of speech, perhaps even language. But such a consensus does not exist, nor does a standardized test battery. The problem is not restricted to linguistic genetics, but extends to virtually all language sciences. The very same equation—some physical aspect of the organism on one side, and something that relates to behavior on the other side—is instantiated in the neuroscientific study of language, because the same logic underlies genomic and cerebral localization. On the behavior side, there is disagreement, alas, not just about theory, methods, and goals, but also about the nature of the relevant evidence. The absence of a common goal and a consensual set of methodologies is detrimental to the field, and we should see whether this situation can be fixed, and at what cost.

The appearance of the three edited volumes that I consider here provides an excellent opportunity to inquire whether common ground is forthcoming. Gazzaniga's reader (G) is a proper choice, because it provides a spectacular view of cognitive neuroscience. It carries impressive sections on history and methods, and on evolution and plasticity, side by side with parts dedicated to more traditional aspects—perception, attention, imagery, memory, and action—and, finally, a small section on language that I discuss here.

The other two books focus on language, and are mostly concerned with the newest neuroimaging methods available—PET (positron emission tomography), fMRI (functional magnetic resonance imaging), ERP (evoked response potential), and MEG (magnetoencephalography). The centerpiece of Brown and Hagoort's volume (BH) is an interesting collection of chapters discussing recent experiments on cerebral reflexes of linguistic behavior, and Marantz, Miyashita, and O'Neil's book (MMO) revolves around the first results of a collaborative effort of linguists, neurolinguists, and vision neuroscientists in Japan and the United States to study neuromagnetic correlates of linguistic and visual generalizations. The approaches espoused in these volumes represent different theoretical perspectives, and some deep, perhaps irreconcilable differences emerge (even though some authors contributed to more than one of the books). One would like to understand why this is so.

To begin, consider this (nonexhaustive) compendium of sources of evidence used by students of language and its biological antecedents:

1. Normal language development (errors children make)
2. Congenital and developmental deficits involving language (the KE family; specific language impairment, SLI; Down syndrome)
3. Disruptions to input-output systems (language and speech in deaf and blind speakers)
4. Input deficiencies (language in children like "Genie," the girl who never learned to speak because of a severe deprivation that continued past her puberty)
5. Disruptions to central, nonlinguistic cognitive systems that interact with language (retardation, Williams and Asperger syndromes, and the like)
6. Normal linguistic behavior in adults (native speakers' judgments of grammaticality, plausibility, ambiguity, and synonymy; speech errors; timed responses to linguistic stimuli)
7. Adult brain failures (associated with diffuse lesions, e.g., Alzheimer's disease; or associated with focal lesions, e.g., aphasia)
8. Measures of cerebral activity during linguistic activity

These sources all allow the measurement of behavior, or biological correlates thereof. At first glance, no fundamental difference among them is apparent: Errors, judgments, and timed reactions, recorded in normal and pathological populations of children and adults, are bona fide human behaviors; electrical measures of neural activity, magnetic measurements of blood flow, measures of location and volume of lesions, and the like are bona fide indices of brain activity. Interested in the aforementioned equation—the relation between neural tissue and linguistic behavior—all researchers would appear to have a common point of departure, at the very least. In reality, some of these topics are widely considered psychological, and others neurological, whereas yet others are thought to be the business of linguists. This bureaucratic division is no mere sociological curiosity; it is a sign of fragmentation, for interaction among linguists, psycholinguists, and neurolinguists is not as intense as one would wish it to be.

Differences, however, should not conceal commonalities, which are also reflected in the three books: When their respective chapters evaluate the utility, the precision, and especially the empirical results of recent technological innovations, everyone seems to agree on the crucial role of advanced imaging instruments in the neuroscientific study of language. Many cognitive neuropsychologists view the introduction of noninvasive brain-probing methodologies with relatively high spatial and temporal resolution as the major advancement of the 1990s. The resolution (in both time and space) of earlier methods was too low, and hence, focal loss of neural tissue equated with loss of cognitive capacity was effectively the only clue for anatomical localization of cognitive natural classes. The current generation of functional neuroimaging methodologies, most notably PET, fMRI, and MEG, has added sophisticated (if expensive) machines to the psychologist's tool kit, and opened a way to a new, exciting episode in the cognitive neuroscience saga. Localizable changes in cerebral activity may now be detected, and equated with cognitive capacity in neurologically intact humans.

Gazzaniga's reader carries two extremely valuable (if somewhat dated) reviews of commonly used techniques—Hilliard's on electrical and magnetic recording, and Raichle's on functional imaging. Both writers are longtime leaders in the field, and their perspectives are important and informative. Brown and Hagoort's book has three chapters on experimental methodology: one by Rugg, on neuroimaging as compared with other methods; another by Kutas, Federmeier, and Sererno, on the ERP method as applied to language; and another by Büchel, Frith, and Friston, on analytic methods for assessing interaction among cerebral regions during cognitive activity. All these authors are true leaders of their fields, and hence their chapters are of high value and interest. Finally, the volume edited by Marantz et al. holds two chapters on methodology, also written by prominent figures: Frackowiak writes on fMRI, and Sekihara, Poeppel, Marantz, and Miyashita discuss technical aspects of MEG. In all cases, the presumed advantage of these new methods over more traditional ones—most no-

tably lesion studies—is of central concern. As my interest here is functional localization, I focus on imaging (relatively low temporal, high spatial resolution) rather than electrophysiological measurements (high temporal, low spatial resolution).

Raichle's chapter (G), originally published in 1988, discusses blood-flow-based methods for the detection of cerebral activity—rCBF (regional cerebral blood flow), PET, and fMRI. He reviews the subtraction method, presents some problems it suffers from, and argues that the growing sophistication of researchers has improved experimental designs and data analysis methods, thereby increasing the reliability of the results. His chapter is a good introduction, but the beef—the real intermethod comparisons—is elsewhere. Arguing that fMRI is advantageous to lesion studies, Rugg (BH) notes that "functional neuroimaging can go beyond lesion data in its capacity to provide information about the dynamics of neural activity," which "opens the door to more complex conceptualizations of the mapping between cognitive operations and their underlying neural substrates than that which underlies the traditional lesion-deficit approach" (p. 16). Imaging in normal subjects, he says, also gets around nagging problems inherent in the lesion-study method. For example, "the mere demonstration of a task dissociation [is not] sufficient to support the inference that functionally distinct cognitive operations have been uncovered" (p. 17). The advantage of functional imaging is that it "provides a means to separate and identify different cognitive operations in terms of their differing neurophysiological correlates" (p. 17). Moreover, as Frackowiak notes (MMO), lesion studies provide a less direct view of the brain, because "the damaged brain, like the learning normal brain, can adapt" (p. 165).

The claim, then, is that the image generated by instruments that monitor the working healthy brain is sharper than anything else, now that methodological problems are solved. Does that make functional imaging the definitive method for cognitive neuroscience? The field has responded in the affirmative: New journals and societies have sprung up around imaging; certain publications have an "accelerated route" for reports on imaging research; and more funds than ever are allocated to imaging by the major agencies. Yet, at least in the language domain, we should ask whether this sociological success means that the methods are indeed superior to more traditional ones. As someone working both with aphasic patients and in fMRI, I have long grappled with this question. My current conclusions tend to be more cautious than optimistic: There are, indeed, important advantages to the use of fMRI in neurolinguistic research, but there are problems, too, that we must solve before giving this method preference over others. These problems pertain to the quality of images, and to experimental constraints that some of the new technologies impose.

Consider, first of all, anatomical precision. Lesion data are obtained through "natural experiments" caused by focal brain damage. The experimenter's lack of control over lesion size and location leads to interpatient variation, resulting in a blurred picture. We expect images obtained from normal subjects through PET and fMRI to be more focused. And it is here that serious obstacles appear.

Brains vary in size and shape. Can we specify a brain location in a general, objective, observer-independent way? This problem has occupied the best minds in neuroanatomy, who have realized that general statements about the brain require a uniform map. The Talairach space (Talairach & Tournoux, 1988) is an attempt to create a map of this sort. Yet the definition of a uniform reference space is insufficient. In many regions of the normal brain, including the language ones, significant intersubject variability is apparent at all levels. At the gross level

of surface topography, huge variation in the shape of Brodmann's Areas 44 and 45—otherwise known as Broca's region—has been demonstrated. Tomaiuolo et al. (1999) have shown that if the two parts of this region are defined by sulcal boundaries, huge intersubject variability is found. Yet even this comparison may not be the right one: It appears that functional divisions in cortex best correspond to cytoarchitectonic, rather than surface, borders (Luppino, Matelli, Camarda, Gallese, & Rizzolatti, 1991; Matelli, Luppino, & Rizzolatti, 1991). This is what Brodmann (1909) originally thought; however, his seminal work, which spanned several species, was limited to one human brain. Thus, Zilles, Amunts, and their colleagues (whose work is duly represented in BH) have embarked on a Herculean project, aimed at mapping cerebral variation at the histological level (currently invisible in fMRI and PET) by examining 10 human brains. For the main language regions—areas 44 and 45—huge variation across individual brains was found not only in cytoarchitectonic properties, but also in the way these areas are situated within the local topography—the sulci and gyri that are visible through current imaging technology (Amunts et al., 1999).

Overall, then, great variation is observed in the language regions. Collapsing functionally defined regions together becomes a hairy problem. The consequence is that the fine structure of the main language regions in humans can only be delineated probabilistically: A given voxel can be said to be part of Broca's region only with a certain probability. Indeed, researchers have proposed probability maps for Broca's area (Amunts et al., 1999; Tomaiuolo et al., 1999), against which functional group data can be evaluated (cf. Indefrey et al., 2001, for an implementation in the language domain). These methods have improved the reliability of localizing claims, yet have also shown that at least for some brain areas, the resolution of images is not as high as we would like it to be.

In addition, there are unresolved technical problems, most notably those regarding image distortions caused by reconstruction algorithms, and the resulting picture is less sharp than we would like it to be. Whether or not the MR image that is created for neurologically intact subjects is sharper than the one obtained by lesion data still remains to be demonstrated. When experimental constraints (caused by the fact that experiments are done inside a claustrophobic, noisy magnet, in which the subject cannot move) are taken into account, Rugg's approach seems most rational: He proposes to view "functional neuroimaging studies as a valuable extension to the lesion deficit approach" (p. 16 in BH). Taking this view, the field can try and broaden the frontiers of knowledge, without losing precious, previously obtained, information. As of yet, unfortunately, Rugg's suggestions hold only in theory. In practice, neuropsychologists of language who work in imaging have attempted to start almost from scratch. Caught by the excitement that swept the field when neuroimaging techniques were introduced, many investigators have largely tended to dismiss aphasia data, rather than seek cross-methodological convergence. This problem is apparent in the books under review.

Consider the chapter by Price, Indefrey, and van Tourennout (BH), which reviews studies of the neural basis of word production and perception, as revealed through fMRI and ERP data. One important set of findings relates to picture, letter, and color naming. These tasks activated the inferior temporal lobe (Brodmann's Area 37) in the authors' study; in another, activation to object naming was observed also "in the inferior frontal, precentral, and subcortical cortices" (p. 214). I was quite intrigued by these results, and wondered whether they converge on lesion data. In particular, I looked to see if the authors draw

any connections between these data and the way Broca's and Wernicke's aphasics access their mental lexicon in a variety of tasks (e.g., Milberg, Blumstein, & Dworetzky, 1987; Shapiro, Gordon, Hack, & Killackey, 1993; Swinney, Zurif, & Nicol, 1989; Zurif, 1995). Regrettably, they offer no attempt to make such a connection, even though the picture that will emerge when multiple sources of data are pooled together will probably be intriguingly complex.

The Price et al. study, in fact, raises a further question, thereby bringing us to a second major problem that the field must solve: It appears that the focus on experimental methodology has deemphasized considerations that pertain to cognition. Although Brown and Hagoort rightly included five theoretical chapters dedicated to language structure and to language processing and its relation to modalities, the experimental and methodological chapters do not connect to the theory as much as they should. In the case of Price et al., it might be important to uncover the authors' underlying view on language, so that it can be reconciled with other existing approaches. Their studies involve language-related activities (language production, language reception, and naming), with stimuli for naming divided into pictures, colors, and letters. Clearly, these choices are not theoretically innocuous. They align, in fact, with traditional neuropsychological categories used for the description and diagnosis of aphasic syndromes (see Goodglass & Kaplan, 1983, for a widely used clinical application of this conception of language). Yet these activity-based analyses of language have proven inadequate: The fine structure of the patterns of impairment and sparing in aphasia requires distinctions borrowed from linguistic theory (e.g., Avrutin, 2001; Grodzinsky, 1984, 1986, 2000; Kean, 1980; Levy & Kave, 1999; Zurif, 1980, 1995; Zurif & Blumstein, 1978). Indeed, Price et al. do not justify their choice—their chapter contains no discussion of whether or not these theoretical building blocks indeed play a constitutive role in language and cognition. The authors, in sum, stopped short of integrating aphasia data into their work, and the motivation for their experimental categories is somewhat lacking. These two weaknesses are not atypical.

In this context, we may also consider the fascinating chapter by Büchel et al. (BH). These authors present compelling reasons for a quest for functional connectivity among cerebral regions during the performance of cognitive tasks. Neuropsychologists have long been interested in simple localization of function, yet complex relations between anatomy and function have not been investigated, even though it is quite likely that multiple regions operate in tandem to realize many higher functions. Büchel et al. propose a method (imported from neurophysiology) to detect such coordinated activity. In essence, their method seeks sizable covariance effects in the activation of two regions, or correlations between time series of response pairs. The idea is to search for similar response patterns in two anatomical loci and use this similarity to isolate the cognitive operations behind it.

The search for interregional covariance in response patterns may uncover temporal patterns that would otherwise go unnoticed. If valid, this method is a novel way to isolate cognitive components, because it is designed to discover distinctive patterns of neural activity over multiple cerebral regions, and correlate them with cognitive operations. It may also characterize an interestingly abstract notion of cerebral localization. Trying to make good of their method, Büchel et al. embark on a localizing project in the linguistic domain. They analyze the results of an experiment with two conditions: phoneme repetition and word generation after the presentation of an initial phoneme. Comparisons of the time series of activations allow the decomposition of the cognitive processes involved into their component parts. The results of

the analysis are that "the key cognitive difference between verbal fluency and phoneme repetition is the intrinsic generation as opposed to extrinsic specification of word representations and implicit mnemonic processing" (p. 342). I must admit that I found the last sentence difficult to understand. I did not see why "fluency" or "repetition" are constitutive elements of cognition. I am aware of no theoretical approach of any persuasion in which such concepts play a role, and in any event, Büchel et al. discuss none.

One could think of investigations that would capitalize on the methodological ingenuity of Büchel et al., but would have stronger theoretical motivation and relevance. This can happen if we change the units of analysis from speech activities to linguistic rules. For example, evidence has accumulated to suggest that in receptive language, certain central syntactic operations—most notably grammatical transformations—involve more than one cerebral region: Both Broca's and Wernicke's areas are likely involved. This possibility is raised by lesion studies (Grodzinsky & Finkel, 1998), which have guided the design of fMRI tests (Ben-Shachar, Hendler, Kahn, Ben-Bashat, & Grodzinsky, 2001; Röder, Stock, Neville, Bien, & Rösler, 2002). These findings point to a highly complex relation between the two regions. The functional connectivity method may help decipher the puzzle, and shed light on the precise characterization of the anatomy of syntactic movement.

This discussion brings us to another important issue—the combinatorial properties of language. Phonological, morphological, syntactic, semantic, and perhaps other rules underlie our ability to put linguistic elements together. Here language manifests its richness— "the creative aspect of language use," which Chomsky has so famously emphasized over the years, lies in rule systems—for combinatorial linguistic rules make novel forms, sequences, and meanings possible. Grammaticality and acceptability judgments form the bulk of the evidential basis for the construction of linguistic theory. For many readers of this journal, these may not count as belonging in psychology, let alone neuroscience, under normal circumstances. And still, sentence processing is a bona fide psycho- and neurolinguistic topic. As Hagoort, Indefrey, and Osterhout (BH) point out, "syntax matters," and "a complete theory of the neurocognition of syntax has to specify how grammatical encoding (speaking) and parsing (comprehension) are organized and embedded in the overall process of speaking and listening/reading. However, this is not enough. In addition, we need to specify which neural mechanisms enable and instantiate the combinatorial apparatus that is so central to natural language" (p. 274).

The attempt to go beyond the modalities through which language is practiced (which feature so centrally in many of the studies I have discussed) to structural properties of language and its combinatorial character is apparent in two of the books under consideration (BH, MMO); the third (G), otherwise a book with panache (featuring charming historical chapters by giants such as Hubel, Mountcastle, and Jerne), is different. Its language section includes one lesion study on how the brain presumably distinguishes fruits from vegetables (Hart, Berndt, and Caramazza), one general review by the editor on right-hemisphere language, and one valuable methodological chapter on ERPs (Kutas). I cannot see why the editor did not see fit to include even a single article on language structure or its relevance to the brain. There is one reference to Chomsky's work (p. 442), and even that one has an error. The linguistic approach to neurocognition is completely ignored in this reader.

Returning now to the studies on the neurocognition of sentence processing, we encounter three very interesting chapters. Friederici

(MMO) reviews the neural basis of linguistic rules, and then presents a study from her own laboratory. She describes the state of the art thus: On the basis of PET and fMRI studies, she concludes that "the posterior region of the left superior temporal gyrus and the adjacent planum temporale is specifically involved in auditory language comprehension"; there is also "an involvement of left inferior frontal regions in phonetic processing," and for syntax she concludes (on the basis of studies with a PET machine) that there is "maximal activation in the left third frontal convolution . . . but additional activation in the left Wernicke's area as well as some activation in the homotopic areas in the right hemisphere" (pp. 128–129). Against this background, she and her colleagues conducted an experiment in which they sought to identify "the phonological, semantic and syntactic subsystems by directly comparing the processing of different types of auditory language input within the same subjects" (p. 130). Active declarative sentences were presented along with sentences with the same syntactic "frame" but with nonsense words, and with unstructured word lists and nonword lists. When sentences with real and pseudowords were compared with word and nonword lists (as a reflection of syntax), the results showed certain temporal, parietal, frontal, and subcortical areas were activated. This leads Friederici to the conclusion that syntactic processing in the auditory modality causes increased activity "in the superior temporal gyrus bilaterally . . . and further . . . in the deep portion of the left frontal operculum, near the pars triangularis in the Broca's area" (p. 133).

These findings seemed quite interesting. I was not exactly sure about the logic behind the experimental design and the interpretation of the results, and wondered how these could be integrated with previous literature. So I went to the chapter by Hagoort, Indefrey, and Osterhout (BH). There I found a review of previous studies, which indicates that "syntax" activates Broca's region and its vicinity, and some temporal regions, mostly in the left but some in the right hemisphere. I also found a description of an experiment, conducted by some of the authors, that had "a grammatical encoding component next to a parsing component" (p. 303). In that experiment, subjects read sentences with nonwords, with and without syntax errors. In the former case, subjects had to detect and correct the error by reading the corrected version aloud—carry out "grammatical encoding." In another set of conditions, subjects merely had to judge the grammaticality of the strings that contained nonwords—"parsing." All the conditions activated parts of left Broca's region, whereas the judgment conditions also activated its right-hemispheric homologue.

I hinted earlier that there is evidence from pathological language that particular components within syntax can be teased apart from others. A central body of work points to the critical involvement of Broca's region in transformational analysis. Transformations are formal operations that are at the heart of grammatical theory. As this theory is a serious attempt to characterize human linguistic ability, the neural substrate of one of its central tenets is highly relevant to the study of brain-language relations. Sacrificing precision for brevity, one can briefly say that Broca's aphasics have little trouble understanding sentences that do not involve transformations—active sentences (*the man touched the woman*), subject questions (*which man touched the woman?*), subject relatives (*the man who touched the woman was tall*), and related constructions—but they have major problems when transformations are involved—in passive sentences (*the woman is touched by the man*), object questions (*which woman did the man touch?*), and object relatives (*the woman who the man touched was tall*), among others (see Grodzinsky, 2000, for a recent review). The empirical basis of this view is rich, and it comes from multiple experimental paradigms, different laboratories, varied syntactic constructions, and many structurally different languages.

It is difficult to see why no reference is made to this highly relevant literature (Friederici herself has made significant contributions to it, which she does not cite, e.g., Friederici & Graetz, 1987). This problem is rampant: Dapretto and Bookheimer's (1999) fMRI investigation of active and passive sentences is one recent example. Although the comprehension of this contrast by aphasics has been intensely investigated and debated (e.g., Hagiwara, 1993; see Berndt, Mitchum, & Haedinges, 1996, and Grodzinsky, Pinango, Zurif, & Drai, 1999, for recent overviews, analyses, and debate), Dapretto and Bookheimer did not cite a single study of the active-passive contrast in aphasics, but just noted that "unequivocal evidence of such a dissociation has thus far been elusive" (p. 427). This claim, even if true (which I doubt), is far from granting its authors a free pass: Brains are brains, whether impaired or not, and the contrast between this presumed elusiveness and the definitive finding reported by Dapretto and Bookheimer must be accounted for. No such attempt figures in their article.

Similarly, the chapter by Levelt and Indefrey (MMO) reviews imaging evidence for the cerebral localization of word production without a single reference to the huge relevant literature that comes from aphasia.

It is a pity that the necessity (or should I say obligation) to seek convergence among dense data sets has not yet become the norm. A chapter on the cerebral organization of word meaning in the same book (by Saffran and Sholl) focuses on disorders, but dedicates little space to the growing number of studies using functional imaging. The resulting statement that "for the most part these [fMRI and PET] results converge with the findings for the category specific deficits" (p. 261) is thus unclear, perhaps even puzzling.

So what is to be done? Like many people, I have expressed excitement at the use of imaging devices in neurolinguistic research; I have, however, been somewhat critical about some current approaches, because it seems to me that the new imaging technologies can be used to test finer distinctions than has thus far been done. We do not come to this new era empty-handed: Traditional neuropsychology has gone far beyond the attempt to find neurological correlates to gross distinctions. Aided by sophisticated analytic tools that current linguistic theory developed, it has provided us with a fairly advanced starting point: We can now test the fine structure of linguistic knowledge and processing through the images fMRI, PET, and MEG devices generate, which will, we hope, advance us toward true understanding of the way the neural substrate of language works to produce linguistic capacity. The view that Marr articulated very clearly, according to which there are levels of understanding when we approach the cognizing brain, should serve as a guide.

In the domain of language, we should use the best currently available views on language. In this respect, Geschwind's (1983) claim that "the notion that we need to know what language is before we ask where it is has little to justify it" (p. 62) is unacceptable. Knowledge of language should be our point of departure, which should then connect to the way this knowledge is implemented neurally. Chomsky's programmatic chapter (MMO) provides a similar view, through an interesting mix of a historical approach, philosophical reflections, and some particulars regarding the neural basis of language and cognition. In the same volume, Poeppel and Marantz demonstrate how we should search for linguistic natural classes in the brain: They set up an impressive experimental paradigm to study electrophysiological reflexes

of phonological generalizations. Their series of highly suggestive MEG experiments shows that as early as 100 to 200 ms after stimulus presentation, auditory cortex carries out phonological analysis. This is a nontrivial finding, showing that complex linguistic computations are carried out already at a very early stage of processing. Linguistic natural classes thus correspond to distinct cerebral reactions, providing the beginnings of a view of the way the human brain makes linguistic computations.

There is more: Some preliminary hints indicate the way syntactic natural classes are cerebrally represented in the normal brain. In particular, the manner and location in which grammatical transformations are computed has been successfully monitored and localized, to provide convergence with lesion-based findings (cf. Ben-Shachar et al., 2001, and Röder et al., 2002, for experiments in Hebrew and German, respectively, that produced findings leading to the same conclusion).

Thus, the new study of brain-language relations, which brandishes fancy technologies and highly sophisticated statistical methods and experimental designs, has taken us several notches up the experimental echelon. We should no doubt proceed with vigor. Yet the attempt to drop everything and start from scratch is mistaken. It will take us back, rather than forward. And forward is where we want to head.

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