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Brain and Language 93 (2005) 239–242
Brain and Language
www.elsevier.com/locate/b&l

Commentary

Commentary on Ullman et al.

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Accepted 5 October 2004
Available online 24 November 2004

Ullman and colleagues should be congratulated for an energetic assault on some of the core issues in neurolinguistics. This work attempts to detect an impact of productivity differences in morphology upon fluency differences in aphasia. This basic goal is well-motivated empirically and should eventually succeed. However, I believe that the links currently being proposed between language and the brain are not well structured conceptually. If these conceptual problems can be successfully addressed, then this work could make a contribution. Without these clarifications, I believe that the current formulation represents a conceptual step backward.

The idea that word formation involves both productive and non-productive processes is an old one, dating back to Panini and the Greeks. This contrast has served as a cornerstone of diachronic morphology for nearly two centuries. In developmental psycholinguistics, the decision to analyze words as based on rote or rule crops up at virtually every stage in the analysis of emerging grammars (MacWhinney, 1978).

From the viewpoint of neurology and aphasiology, the contrast between fluent and non-fluent aphasia is also fundamental. The traditional interpretation of this contrast has been one that focuses on the role of anterior areas in motor processing and posterior areas in sensory processing. In the 1970s, evidence that non-fluent aphasics also had disabilities in comprehending complex syntax (Zurif, Caramazza, & Myerson, 1972) led researchers to challenge this analysis. Following Jakobson (1955), neurolinguistics began to imagine that frontal areas might control grammatical processing, whereas posterior areas control lexical insertion.

The dual-route analysis of Ullman and colleagues represents an attempt to advance the Jakobsonian view of the brain as implementing a set of grammatical modules in clearly separated cortical regions. In this sense, it views brain organization as driven by the structure of linguistic theory. As presented by Ullman et al. neuronal dual-route theory commits itself to six core propositions:

1. the brain is a symbol manipulation system,
2. the brain implements linguistic rules,
3. the brain implements rote lexical retrieval,
4. the formation of words by rote and the formation of words by rule are computed in separate cognitive modules,
5. rote is processed in posterior areas and rules are processed in anterior areas,
6. brain organization to support this dissociation between rote and rule is a domain-specific adaptation that was required for the evolution of human language.

The first commitment is one that has been articulated most forcefully by Ullman's colleague Steven Pinker (Pinker, 1991). Pinker's basic argument is that the brain is a symbol processing system in the sense of the classical production rule systems formalized by Newell and Simon (1972) and Chomsky (1963). For Newell and Simon (1972) and Chomsky and Halle (1968), rules could assume truly wondrous proportions. Although Newell and Simon were committed to elementary information processes as the building blocks of rules, the actual rule systems they proposed could take up pages of text. In Chomsky and Halle, the formulation of a phonological rule for stress placement in English required dozens of branching conditions and subconditions, special complex variable symbols, and a rich set of annotated exceptions and subtypes.

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0093-934X/$ - see front matter © 2004 Published by Elsevier Inc.
doi:10.1016/j.bandl.2004.10.002

Most readers are familiar with the debates that have raged during the last twenty years regarding this issue. In various places (MacWhinney, 1994, 1998), I have argued that neurons cannot directly implement production rule systems, because axons cannot pass codes for symbols and neurons have no memory addresses that parallel those that digital computers use to address RAM. As a result, the basic level of computation in the brain must be sub-symbolic. However, most connectionists would also be willing to agree that sub-symbolic computation can give rise to emergent symbols. Because neural network models have been slow to construct adequate accounts of many syntactic phenomena (MacWhinney, 1999b), there is good reason to believe that this debate will continue without a final resolution for many years.

Ullman et al.'s core difficulty lies not in their first commitment, but in their failure to articulate the exact scope of their second commitment to linguistic rules. For Pinker and Ullman, the great powerful rule systems of 1970s have become much reduced in scope. In fact, the major target of their investigation has now become what I have called the "kinder, gentler" rule. An example of a more modest rule of this type is "add -ed." In effect, the great interwoven symbolic efflorescence of the 1960s has now been pruned back to set of small, non-overlapping buds, possibly including nothing more than the productive, default grammatical morphemes of the language. In English, this would reduce the rule set to something like a dozen elements.

Ullman et al. might well reply that they consider linguistic rules to also include structural frames such as wh-movement, passivization, and so on. However, in practice, the work of this group in the 1990s has focused entirely on concatenative inflectional morphology. Given this, why has this version of dual-route theory been stated in such global terms, when in fact the evidence provided is only relevant to a narrow range of linguistic phenomena? It is important to note, that while Ullman, Pinker and colleagues have been exploring the processing of inflectional morphology as rules, Chomsky has been retreating from his former commitment to rules, hoping instead to formulate syntactic structure through principles and constraints on subsystems. The movement away from rules in linguistics can also impacted phonological theory, where the dominant view of optimality theory (Kager, 1998) sees structure as emerging from constraint satisfaction, rather than rule application.

For the purposes of the current analysis, let us simply replace the concept of "rule" in this dual-route model with the concept of "combination" (MacWhinney, 1978). The competition between combination and rote is the dual route proposed in MacWhinney (1978) and it is the one which most properly matches up with the shape of the actual empirical evidence on this issue, as accurately reviewed by Ullman et al. With this substitution, and removing the first and last commitment as largely irrelevant to the debate, the revised dual-route position then becomes:

1. the brain combines lexical forms,
2. the brain implements rote lexical retrieval,
3. the formation of words by rote or combination is computed in separate neural areas,
4. rote is processed in posterior areas and combinations are processed in anterior areas.

In this revised account, the first two commitments are now well-grounded. Moreover, in this revised form, the third and fourth commitments seem empirically plausible and theoretically consistent. In this account, all words can be stored by rote in posterior language areas. Lexical look-up is performed by a large interactive neural network, perhaps organized in the form of self-organizing feature maps (Miikkulainen, 1993), although the exact architecture of this module is not important for the current analysis. Processing in this posterior network displays all of the patterns of similarity-based activation that have been emphasized in neural network models of the last 15 years.

Given the power and coherence of this posterior network, why would language need to construct a second method for lexical activation? Why would it place a second system in a separate module at such a great distance in the brain? The reason is exactly the one articulated first by Berko (1958). The idea is that the activation of frontal combinatorial patterns for lexical lookup is needed to support gaps in posterior processing. In production, frontal areas work to order posterior activations into longer serial plans. Some of this serial organization may already be achieved by posterior rote phrasal look-up for frequent combinations (Stemberger & MacWhinney, 1986). However, sentences are not stored by rote and items must be combined. Because combination is a fundamentally motoric process, it makes sense that premotor cortex and inferior frontal areas are deeply involved in these aspects of sentence production. Damage to these areas leads to an omission of morphemes in production and a relative insensitivity to omissions in judgment tasks. Recently, Hagiwara et al. have confirmed exactly this pattern for the Japanese deadjectival nominalizing suffix -sa in Broca's aphasics. This same pattern of omission detected recently by Hagiwara, Sukioka, Ito, Kawamura, and Shiota (1999) and so many other researchers (Bates, Friederici, & Wulfeck, 1987; MacWhinney, Osman-Sági, & Slobin, 1991) is also being picked up by Ullman and colleagues.

In comprehension, these same frontal areas use active tracking of sentential structure (Elman, 1990; MacWhinney, 1999a) to generate expectations about the shape of words to follow. This highly interactive and incremental process operates in a temporally interdigitated fashion with posterior lexical lookup, even though it is generated from a separate module. It is important to note that the interactive nature of the modules proposed in this revised analysis violates Fodor's original definition of a module. However, this assumed interactivity does not cause problems for the dual route model of Ullman et al. since the primary interest here is the development of a linkage between modes of lexical processing and specializations of neural tissue. As a result, the third commitment has been modified to refer to neural areas, rather than modules.

It would appear that our revised interpretation of the results of Ullman et al. matches up well with modern linguistic theory, traditional concepts in neurology and aphasiology, previous experimental work, and advances in neural network modeling. We reached this revised interpretation by shifting from notion of rule to the process of combination and by removing three irrelevant assumptions in Ullman's account. Ullman et al. might protest against the removal of their commitment to domain-specificity. However, they themselves seem quite ambivalent on this issue. Although they begin their paper with a bow to domain-specificity, they conclude by reducing the distinction between rote and rule to the contrast between declarative and procedural memory. Until Ullman et al. are able to generate a clearer set of conditions for what counts as domain-specific, it seems better to relegate this particular claim, along with that the debate about symbols, and the strict interpretation of Fodorian modules to the category of "currently irrelevant."

However, there is still one remaining problem with this "meaner-leaner" revised account. The problem is the claim made by Ullman et al. that regulars cannot be stored by rote. Ullman et al. believe that posterior areas cannot activate words with regular inflection. This analysis fails to consider evidence that high frequency inflected regulars are stored by rote (Stemberger & MacWhinney, 1986). This issue is important, because of the way it leads us to interpret what is going on in fluent and non-fluent aphasia.

In the strict dual-route view of Ullman et al. no regulars are computed posteriorally. This means that non-fluent aphasics should have virtually no access to regulars. But consider the evidence provided by the MEG investigation of Rhee, Pinker, and Ullman (Rhee, Pinker, & Ullman, 1999). In that study, production of both regular and irregular past tense forms produced dipole activations of left temporal/parietal regions, whereas only regulars produced activation of left frontal areas. Even more importantly, the frontal activations occurred after the posterior activations. This suggests that the basic lexicalization process for both regulars and irregulars is posterior. Ullman et al. argue that there is strong activation of anterior areas for regulars, even though no forms are activated. This is a rather peculiar version of activation theory and it would be interesting to see it developed more generally. It would seem to suggest that all areas of the brain are activated at all times, even though they lead to no impact on processing.

It is worth noting that, the task used by Rhee et al. provided no useful syntactic context. Therefore, it is unlikely that a full set of frontal syntactic activations was operative. Instead, one might guess that frontal activation in that study involved a brief moment during which the candidate forms generated from the posterior received additional support if they matched a frontal expectation. This would only happen in the case of the regulars. This interpretation of these results does not require that frontal activations always be confirmatory. On the contrary, frontal activations can lead to the preactivation of posterior forms, as mentioned earlier.

The other side of this coin is the finding that the subject FCL was unable to produce regular past tense forms. Stemberger and MacWhinney argue that subjects will vary in the degree to which they commit high frequency regulars to rote. Given this, studies such as the past tense production task with FCL which boil down to single-case studies are unable to tell us much about the issue at question. Worse still, the tendency of non-fluent aphasics to be satisfied with verbal output that constitutes correct words is reinforced by the task in this experiment. A better measure for patients of this type would be one grounded on studies of spontaneous production.

Let us imagine that this study has been run with a larger number of patients and that it turns out that non-fluent aphasics actually do fail to use regular inflections even for high frequency regulars. Such a finding would then lead us to wonder whether any observed failure to activate posterior forms could be attributed to the removal of a supporting cue that is usually provided by the frontal areas. To address this, we would want to look at the contrast between production and perception. Although the reading and judgment tasks used by Ullman et al. include perceptual components, a purer measure of comprehension would be something like picture interpretation task. If we found that non-fluent aphasics were unable to distinguish between pictures of "the frogs jump" and "the frogs jumped" but were able to distinguish between pictures of "the frogs run" and "the frogs ran," then the position assumed by Ullman et al. would be much more convincing.

It would be easy to spin out a further series of possible interpretations of additional experiments that would conceivably clarify these various issues, but that takes us far away from the current article and its contribution. Instead, let me attempt a general evaluation. First, I will repeat my congratulations to Ullman et al. for conducting an energetic assault on some of the core issues in neurolinguistics. It makes sense to study the impact of productivity differences in morphology upon fluency differences in aphasia. Having said this, I believe that three of the theoretical commitments provided by Ullman et al. constitute irrelevant barriers to conceptual and empirical development. In particular, their commitment to symbolic systems, modularity, rules, and domain-specificity only function to cloud the waters of what could become an interesting discussion of the neural implementation of a possible dual route. With these irrelevancies removed, we could begin to investigate a mechanistic account that really starts to get at the contributions of specific cortical areas to lexical and syntactic processes. I look forward to the examination of these interesting issues in this clearer context.

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