Predictions about neurobiological mechanisms of language may be helpful for planning experiments in cognitive neuroscience and for interpreting their results. However, these considerations are at present necessarily preliminary, as pointed out in the target article, not only because the proposals may be falsified by future research, but also because they leave so many questions unanswered. For example, how is it possible to model multiple occurrences of a particular word (same form, same syntax, same meaning) in a given sentence? A not so attractive possibility would be that there are multiple representations for every word type in the processing model or its neurobiological counterpart. Other solutions may make the models much more complicated. Although it is clear that we can, at present, only scratch the surface of lexical processes in the brain, Levelt et al.'s target article clearly evidences that the insights obtained so far are worth the scientific enterprise.

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Authors' Response

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Relations of lexical access to neural implementation and syntactic encoding

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Abstract: How can one conceive of the neuronal implementation of the processing model we proposed in our target article? In his commentary (Pulvermüller 1999, reprinted here in this issue), **Pulvermüller** makes various proposals concerning the underlying neural mechanisms and their potential localizations in the brain. These proposals demonstrate the compatibility of our processing model and current neuroscience. We add further evidence on details of localization based on a recent meta-analysis of neuroimaging studies of word production (Indefrey & Levelt 2000). We also express some minor disagreements with respect to Pulvermüller's interpretation of the "lemma" notion, and concerning his neural modeling of phonological code retrieval. **Branigan & Pickering** discuss important aspects of syntactic encoding, which was not the topic of the target article. We discuss their well-taken proposal that multiple syntactic frames for a single verb lemma are represented as independent nodes, which can be shared with other verbs, such as accounting for syntactic priming in speech production. We also discuss how, in principle, the alternative multiple-frame-multiple-lemma account can be tested empirically. The available evidence does not seem to support that account.

Pulvermüller discusses possible neural mechanisms for the implementation of our computational model of lexical access (Pulvermüller 1999, reprinted here). His starting point is clear and correct: The processing model is not a theory about brain mechanisms. It is not a "'neural model" or anything of the sort. It is a psychological processing model formalized in terms of a rather classical spreading activation architecture. The issue of the model's potential neurological underpinnings is of great importance. On the one hand, the model should not be incompatible with existing neuroscience. For instance, WEAVER's chronometric properties should not violate known neurological limitations. Providing potential neural mechanisms for implementing fragments of the model amounts to providing existence proofs for compatibility. On the other hand, the theory of access can be a guide or tool for exploring the patterns of cerebral activations obtained in neuroimaging studies of word production, which involve tasks ranging from picture naming and verb generation to word and nonword reading. A rather coherent pattern of mappings between processing mechanisms in the theory and brain localizations emerges from study.

The first issue addressed by **Pulvermüller** concerns inhibition. Clearly, neuronal functioning would get disrupted without inhibitory regulation. How can this be compatible with the absence of inhibitory connection in the WEAVER model? Here Pulvermüller correctly observes that WEAVER incorporates an equivalent of inhibition, namely, in the lexical competition governed by Luce's rule.

Next, **Pulvermüller** addresses the issue of the brain loci corresponding to various computations in the model and we agree with several of his proposals. To shortcut somewhat, we refer to the above-mentioned meta-analysis by Indefrey and Levelt (2000). There each word production task used in the literature was analyzed as the combination of a "core" process and a "lead-in" process. A core process is any consecutive subset of stages in the target article's theory, ranging from conceptual preparation to articulation. The lead-in process for a given task is the task-specific initiation of these core processes. For instance, picture naming has visual object recognition as its lead-in process, followed by a core process consisting of all stages of word production. Word reading has visual word recognition as the lead-in process. It is followed by core processes from phonological code retrieval, via syllabification down to articulation. Nonword reading has some form of grapheme-phoneme conversion as the lead-in process. There is no phonological word code retrieval here; the core process begins with syllabification and is completed with articulation.

By comparing observed cerebral activations between critical pairs of task, and using a statistical criterion, the various core processes in the theory could be related to smaller or larger foci. For instance, the critical difference between word and nonword reading resides in accessing a word's phonological code. The meta-analysis indicates Wernicke's area as being involved in this core operation. Similarly, the studies indicate that syllabification involves the left inferior frontal gyrus, whereas phonetic encoding and articulation show the expected bilateral involvement of ventral sensorimotor areas. The tasks used in the imaging literature did not allow us to distinguish between conceptual preparation and lemma access. The statistically common region in the imaging studies relating to this pair of processes turned out to be in the midpart of the left middle temporal gyrus. However, the subtraction logic of the meta-analysis would necessarily miss the variability in cortical representation for different semantic fields, such as tools, vegetables, and animals. **Pulvermüller** correctly points to this issue, which has become a hot topic in imaging and patient studies of

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word processing (see Martin 1998 for a review). It complicates the search for the localization of lemma-related operations.

In one point, **Pulvermüller** overstates the role of lemmas. In our theory, lemmas do not have a direct role in binding the word's articulation pattern and sound image. We do assume lemmas are shared between production and perception of speech, but on the production side their direct link is to one or more morphemes (i.e., abstract phonological codes), not to articulation patterns (see Fig. 2 of the target article). The articulation pattern is the product of phonological encoding, phonetic encoding, and articulatory motor action; it has a quite variable, indirect relation to lemmas. It is therefore not necessary to relate lemmas to an extensive network ranging all the way from auditory to primary motor cortices, as Pulvermüller suggests.

A further issue addressed by **Pulvermüller** concerns the ways in which one-way connections in the processing model can be neurologically implemented. This is an important issue. It is convincingly argued in the commentary that any pair of cell assemblies must involve bilateral connections. But the model contains several one-way connections, in particular, those leading from lemmas to word forms (morphemes/phonological codes). If the corresponding linguistic operations involve different, but connected, regions, then why does one region's activation not affect the other region's operations? According to Pulvermüller, existing feedback between brain regions need not have behavioral consequences. For instance, the fast operation of lemma selection may be completed before the region is reactivated by feedback from a phonology-dedicated region. Although it is satisfying to see there is no threatening incompatibility here either, we would not like to shortcut the issue this way. Psychologically, it would predict evidence for feedback in cases where lemma selection is slow (e.g., when there is strong lemma competition). There is no evidence this is in fact the case. Neurologically, it seems to imply that reciprocal connections serve activation. They may as well serve inhibition or far more complex forms of control, such as the equivalent of the verification operation in WEAVER, which serves binding (see sect. 3.2.3 in the target article).

Finally, **Pulvermüller** considers possible neural mechanisms for realizing "numbered connections" in WEAVER. In the model, the segments in a retrieved phonological code are numbered. For instance, the code for *dense* consists of numbered segments /d/, /e/, /n/, and /s/, where the numbering specifies the position of the segments in the word. The same segments are numbered differently in the code for *send.* Pulvermüller proposes to handle this by means of synfire chains (Abeles 1991). He rejects the simplest version of this, that is, chains linking the neuronal representations of the phonemes /d/, /e/, /n/, and /s/ as d→e→n→s for the word *dense,* and as s→e→n→d for the word *send.* Our reason for rejecting this would be that the experimental evidence reported in section 6.4.1 of the target article supports the notion that all of a code's segments are simultaneously, not sequentially, activated. Pulvermüller's stated reason is: "not phonemes, but phonemes-in-context are the elements of representation." So, for example, the /e/ phoneme will be slightly different in *dense* and in *send.* Although there is good phonetic evidence for this type of difference, it cannot be an argument for proposing the more complex synfire representation given in Pulvermüller's Figure 1, where phonemic representations are slightly different in different contexts. A first problem is that Pulvermüller's synfire chain produces sequential activation of a code's phonological segments. However, as mentioned, this is not what we find in our experiments. A second problem is that such representations will hamper the variable phonological encoding the model must allow for. Take the phonological encoding of *send.* If the speaker formulates the utterance *What shall I send?,* the speaker will encode /send/ as the final syllable of utterance. But if the speaker prepares the utterance *To whom will I send it?,* the final syllables will be /sen-dit/. The phoneme /d/ ends up as a syllable-final in the first case, but as a syllable-initial in the second case. But in both cases it emerges as a segment in the same phonological code for *send.* This shows that segments in the phonological code itself must be context-neutral, not context-sensitive as Pulvermüller proposes. If the retrieved segment /d/ would be context-sensitive – namely, one that is clustered with /n/ and syllable finally – it could not possibly end up in the syllable-initial position of /dit/. In our model, the phonetic context sensitivities that Pulvermüller observes are handled at a later stage, namely, after the phonological syllables have been computed. It is the stage of phonetic encoding discussed in section 7 of the target article. Therefore, it seems to us that more work needs to be done to develop a potential neurological account of our numbered phonemic representations.

Branigan & Pickering correctly point out that our model does not capture syntactic integration, which is, evidently, an important part of language production. However, as we stated in several places, including the title, the target article was never intended to capture syntactic processing.

Branigan & Pickering's proposal concerning the representation of grammatical information about verbs appears to be fully compatible with our view. In particular, we agree that nodes representing lexical grammatical information should be shared between words (see our treatment of the representation of grammatical gender in sect. 5.4 of the target article). Evidently, much more theoretical and empirical work is needed to gain a fuller understanding of the way syntactic information is represented and used. An open representational issue is, for instance, whether alternator verbs like "give" are represented in one lemma with two sets of syntactic nodes, as Branigan & Pickering propose, or as two separate lemmas permitting exactly one frame each, as proposed by Levelt (1989), following Bresnan's "lexical rule" analysis (Bresnan 1982). In Branigan & Pickering's own account (with which we sympathize), the syntactic priming results obtained since Bock's (1986) original study and including the recent strong findings by Pickering and Branigan (1998), cannot distinguish between these theoretical alternatives. In both cases each syntactic frame is represented by an independent syntactic node, accessible to all lemmas that share that frame. Priming results from "reusing" such a node.

There are, however, theoretical reasons for adopting the one-lemma-multiple-frames type of representation. Most verbs have multiple lexical frames, as is increasingly apparent from parsing studies of large text bases (e.g., see Bangalore & Joshi 1999). In many cases these multiple frames do not correspond to multiple verb meanings; hence they are not cases of homonym. Our account of homonyms in section 6.1.3 of the target article assigns multiple lemmas to multiple lexical concepts; homonyms only share their

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morphological word form node. A multiple lemma account of a verb's (or other category's) multiple syntactic frames would create an enormous proliferation of lemmas that share the same lexical concept and the same word form. This is not attractive theoretically. It can also be tested empirically. A multiple lemma account predicts lemma competition, given Luce's rule for lemma selection (sect. 5.1 of the target article): the more co-activated lemma nodes for a given verb (or other category), the slower the selection of any one of them. This type of lemma competition is exactly the one we suggested (sect. 5.3.5) for the case of *eyes* (= gaze) versus *eyes* (= plural of sense organ), the co-activation of their lemmas leading to relatively slow selection of either of them. The test for a multiple lemma account of multiple syntactic frames would be to compare selection latencies for verbs (or nouns) that vary in number of frames, but are comparable in all other respects. If no corresponding difference in selection latencies shows up, the multiple lemma account is without support. In fact, the only available evidence (Ferreira 1996) points to faster rather than slower access for multiple frame verbs.

Whatever the solution will be, both accounts require a mechanism for choosing among alternative frames. In the multiple lemma account, this is primarily a choice among lemmas. If this choice is not conceptually driven, how does it function? No particular proposals have been made so far. In the single lemma account, the choice is one among co-activated syntax nodes. Will the choice exclusively depend on the relative accessibility of the alternative frames? This would not be in the spirit of WEAVER. There is always a verification operation to check whether a potential selection is the appropriate one. In the case of the choice of syntactic frame, this verification may involve a check of the availability of the relevant arguments for the frame at hand.

Branigan & Pickering suggest that the results of their syntactic priming experiments offer stronger support for the assumption that lemmas are shared between speech production and comprehension than the results of picture-word interference experiments. We fail to see in which respect the evidence can be viewed as stronger than ours, but it is certainly an excellent additional argument in favor of our proposal.

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[Note: The letter "r" before author's initials stands for CC Response article references]

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