

sistent with the well-established nature of conceptual representations in STM (Sachs 1967; 1974) and LTM (Craik & Tulving 1975) linguistic processing.

Finally, the proposed model seems to implicitly assume at least one of Baddeley's (2001a) central tenets, that is, a strict architectural separation between posterior memory retention and anterior control systems. In so doing, the model seems noncommittal about the critical operations of shifting, updating, and reconfiguring of task-relevant information in working memory. Instead, the ERP evidence reviewed here suggests a less strict anatomical distinction between storage and control functions (Barceló et al. 2000b; 2002). As a plausible alternative to this dichotomy, a context processing model has been proposed as a simple representational mechanism capable of subserving both memory storage and control operations through the functional integration of activities from prefrontal and posterior association cortices (Braver et al. 2002). From a cognitive neuroscience perspective, a valid model of working-memory function should provide an integration of evidence across a wide range of task paradigms at the crossroads of a variety of higher cognitive functions (i.e., Fuster 2003).

Varieties of procedural accounts of working memory retention systems

Nelson Cowan

Department of Psychological Sciences, University of Missouri, Columbia, MO 65211. CowanN@missouri.edu <http://www.missouri.edu/~psycowan>

Abstract: The present commentary agrees with many of the points made by Ruchkin et al., but brings up several important differences in assumptions. These assumptions have to do with the nature of the capacity limit in working memory and the possible bases of working-memory activation.

I find much to like about Ruchkin et al.'s target article. The authors agree with my theoretical conception of working memory (Cowan 1995; 1999) and provide more support than I mustered from previous literature. However, here I will focus on some areas of disagreement.

One subtle disagreement relates to the brain representation of the focus of attention. Ruchkin et al. (sect. 5, last para.) state,

We assume that the "number of pointers" is limited (i.e., the amount of information that can be in the focus of attention is limited). Consequently . . . we attribute properties such as short-term memory capacity and displacement of information mostly to the functioning of the prefrontal system.

Instead, Cowan (1995, Ch. 8) distinguished between the mechanisms of the *control* of attention (heavily involving frontal areas) versus the *focus* of attention (heavily involving inferior parietal areas). This distinction matches evidence of the existence of anterior and posterior attention systems (Posner & Rothbart 1991). It considers that the parietal areas are loci for the convergence of information from all senses, making them suitable as multisensory integration areas, and that damage to these areas typically results in deficits of awareness, such as unilateral neglect and anosognosia.

My question about the pointer metaphor is whether the limit is how many pointers the frontal lobe can contain, or how much integrated information can be represented, to which frontal mechanisms can point. Perhaps one testable distinction is whether frontal damage results in a decrease in the capacity of the focus of attention, or only a decrease in the ability to maintain and shift that focus. Several theoretical suggestions for the mechanisms of capacity limits rely on the concept of confusion resulting from overlap in the representations of multiple chunks kept active concurrently (e.g., Luck & Vogel 1998; Usher et al. 2001), favoring the placement of capacity limitations in the posterior representational system rather than the frontal control system.

Another issue pertains to the classification of theoretical views. According to Ruchkin et al. (sect. 1.1, last para.),

Baddeley (1986; 2001a; 2002) posited that the working memory short-term storage modules are separate from long-term memory storage modules.

and (sect. 1.2, first para.).

Investigators such as Crowder (1993) and Cowan (1995; 1999; 2001) have been proponents of a contrasting view of short-term memory operation, namely, that long-term memory and short-term memory are different states of the same representations.

The latter position was termed *proceduralist* because the memory representation uses the same neural systems (procedures) involved in perception. I agree but, interestingly, my view has more often been considered similar to that of Baddeley and different from that of Crowder. Baddeley and I have differed from Crowder on the role of memory decay in short-term memory, a concept that Ruchkin et al. invoke to define how long activation lasts.

Decay can be conceived of as the loss of information from memory as a function of time (as in radioactive decay). There is a question of whether short-term memory representations do decay. I have posited so in most of my theoretical writing, whereas Crowder (1993) has eschewed that concept. According to Crowder (also Nairne 2002), the loss of information over time occurs only because the most recent information loses distinctiveness in memory. The common analogy is that if one stands near a telephone pole and looks down a long series of poles (a metaphor for a stimulus list), the nearest few poles look more distinct from one another than do farther-away poles. However, if one moves to a point far beyond the end pole (a metaphor for a long retention interval in a memory test), even the end pole begins to blend in with the others.

Although my colleagues and I have addressed this issue in several studies, the existence of decay is as yet neither proved nor disproved. In support of decay, Cowan et al. (1997) examined two-tone comparisons and found that performance decreased as a function of the time between tones, even when it was expressed as a ratio between that time and a prior inter-trial interval. However, when we reexamined the data to consider previous intervals in the trial series, we could not totally dismiss the possibility that information is lost at a rate that depends on prior intervals (Cowan et al. 2001). This method warrants more systematic investigation.

Baddeley's (1986) conception of working memory relies upon the assumption of decay of the short-term representation and bases that assumption on the finding that the serial recall of words depends upon the spoken durations of those words. For lists of long words there is more time for decay during rehearsal (or during recall; Cowan et al. 1992). Recent evidence suggests that, when one matches linguistic properties of lists of words that can be spoken quickly versus less quickly, word-length effects are unreliable (Lovatt et al. 2002; Service 1998). However, those studies involve only modest differences in the spoken durations of short and long words. In contrast, the original word-length effect was based on lists of monosyllabic words versus words with larger numbers of syllables, which produce much larger differences in spoken durations. Although one cannot use these uncontrolled stimuli to establish a time-based effect, Cowan et al. (2000) demonstrated word-length effects in comparisons of the identical word lists under instructions to speak quickly versus much slower.

Without decay, the notion of activation still can be preserved by assuming that it ends through displacement of one representation by another (cf. Atkinson & Shiffrin 1968). Supporting this idea, some amnesiacs retain story information for up to an hour if no other stimuli intervene, even after sleeping during the retention interval and therefore clearly not rehearsing the story continually (Della Sala et al., in press). A type of memory activation thus may preserve the most recent information for long periods.

The unitary view of short-term memory (Nairne 2002) and its precursor, interference theory, hold that short- and long-term

memory phenomena follow common rules. This goes well beyond the version of the proceduralist assumption held by Ruchkin et al. It repudiates not only separate short-term memory structures (Baddeley 1986; 2002), but also separate short-term memory processes. In defense of dual processes, Broadbent (1971) argued that we should, in fact, expect short- and long-term memory results to resemble each other, given that short-term memory is heavily involved in creating long-term memories. Cowan (1995; 2001) described how short- and long-term memory results differ in subtle ways.

Thus, psychological theory is more than dichotomous. The view of Ruchkin et al. resembles unitary memory theory in denying the existence of separate short-term memory structures, but differs in retaining separate short- and long-term memory processes. I agree, though I remain unsure of the nature of activation and capacity limitations. Regardless, the target article compellingly demonstrates the usefulness of electrophysiological techniques for understanding psychological processes.

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Some mechanisms of working memory may not be evident in the human EEG

Emrah Düzel

Department of Neurology II, Otto-von-Guericke University, DE 39120 Magdeburg, Germany. emrah.duezel@medizin.uni-magdeburg.de

Abstract: Ruchkin et al. use brain-activity data from healthy subjects to assess the physiological validity of a cognitive working memory model and to propose modifications. The conclusions drawn from this data are interesting and plausible, but they have limitations. Much of what is known about the neural mechanisms of working memory comes from single neuron recordings in animals, and it is currently not fully understood how these translate to scalp recordings of EEG.

In this commentary, I outline four types of physiological limitations to what can be concluded from the existing brain-activity data, such as that used by Ruchkin et al.

First, our current knowledge about the neural underpinnings of scalp-recorded EEG signals is not enough to conclude that a neural phenomenon does not exist. Is it legitimate to rule out the existence of specialized short-term memory buffers because their signatures are not evident to us in the EEG phenomena we select? This is especially difficult to judge, as we do not yet have a sufficient understanding of the basic neural mechanisms that underlie cognitive concepts such as “representation” and the “activation” thereof, let alone their reflections in the scalp EEG. Ruchkin et al. suggest that activations of semantic representations, for example, might be deduced from modulations of the N400 component. But it is as yet unclear whether the neurophysiological indices of these activations are DC-shifts. Stimulus-specific persistent neural activity as a neural mechanism underlying working memory was discovered thirty years ago, and it is neural firing that is hypothesized to be sustained by synaptic reverberation (Wang 2001). Oscillations of local field potentials associated with such reverberations might give rise to DC-shifts (Caspers et al. 1987), but they might be more directly visualized as EEG oscillations. The extent to which neural firing itself is visible in the scalp-recorded EEG is probably very limited (Logothetis et al. 2001). Ruchkin et al. acknowledge the importance of neural oscillations, but they limit their use to assessing interareal coupling and prefer DC-shifts as an index of intra-areal processing. Why not treat oscillations as an index of intra-areal processing too? It is quite reasonable to assume that certain types of neural oscillations do not

covary with slow neural changes, such as DC-shifts, and can reveal neural processes that otherwise remain undetected (Düzel et al. 2003).

Second, one element of physiological working memory models in animals is the robustness of delay activity to distracters (Miller et al. 1996). For example, the Miller et al. study showed persistent stimulus-specific neural firing in inferotemporal cortex as well as prefrontal cortex (PFC), but only the prefrontal activity was robust to distracters in the delay or retention interval. This finding suggests that PFC neurons can maintain stimulus-selective delay activity even when delay activity in inferotemporal regions is disrupted by intervening distracters, which in turn might suggest that stimulus-selective delay activity in PFC does not require delay activity in temporal or posterior brain regions (Goldman-Rakic 1995; Wang 2001). Currently we cannot tell if the DC-shifts recorded by Ruchkin et al. would also show robustness to distracters.

Third, animal studies suggest that prefrontal neurons code information in working memory that is more than a “pointer” to posterior stimulus-specific delay activity. My understanding of how Ruchkin et al. view “pointers” is that these do not store stimulus-selective information. Rather, they index where stimulus-selective information is stored. However, there is evidence that prefrontal neurons indeed store stimulus-selective information and that, contrary to what Ruchkin et al. assume, the firing patterns of prefrontal and parietal neurons could be compatible with duplication of information in both regions. A direct comparison of prefrontal and parietal delay activity in nonhuman primates in a spatial working memory task has shown that neurons in both cortical areas exhibit very similar sustained activity during the delay period, with nearly identical spatial tuning (Chafee & Goldman-Rakic 1998). This finding means that different prefrontal neurons have different spatial selectivity in the delay period and hence can code stimulus-specific information to an extent that is likely to go beyond being a mere pointer to other representations.

Fourth and finally, brain-activity data in healthy subjects cannot identify brain processes that are critical for a given cognitive function. Are the posterior DC-shifts that Ruchkin et al. have related to the maintenance of stimulus-specific information, epiphenomenal, or do they reflect critical processes for working memory? From Ruchkin et al.’s model this question will be difficult to test, because the model suggests that delayed maintenance of stimulus-specific information is accomplished in those brain regions that initially process the task-relevant aspects of the stimulus. A permanent lesion in such brain regions would impair stimulus processing even before any maintenance operation could start. What is necessary to answer this question, are “dynamic” lesions, which cause impairment selectively and transiently during maintenance. One way to achieve such dynamic lesions in humans would be by transcranial magnetic stimulation (TMS) (Düzel et al. 1996) applied during the retention interval over the areas where the posterior DC-shifts occurred in the Ruchkin et al. study. Two recent TMS studies are relevant in this respect. Both of them show that TMS over posterior neocortex disrupts working memory, but only if it is applied early, close in time to stimulus processing, and not later in the retention interval (Harris et al. 2002; Oliveri et al. 2001). One study shows that frontal TMS disrupts working memory only when applied later in the retention interval (Oliveri et al. 2001). These results are compatible with frontal areas acting as working memory stores and posterior areas acting as initial processors rather than regions of activated representations.