different. None of this required theoretical or causal knowledge. Rather, ATRIUM was able to solve the task by acquiring three types of knowledge: rule knowledge, exemplar knowledge, and meta-knowledge about when to use different types of underlying knowledge.

Does this mean that models such as ATRIUM will be able to account fully for all the phenomena in the conceptual landscape? No, but relatively simple models such as these, as well as more complex theories (e.g. Rogers and McClelland's PDP models [3] and HAL [4]), are making considerable progress spelling out the types of knowledge that underlie people's concepts. These types of theories provide a promising start for concrete theoretical accounts of people's conceptual behavior.

Real-life explorers need both a sense of their ultimate goal and a way to take concrete steps toward that goal. Murphy provides conceptual theorists with a first rate overview of the empirical landscape in which they work. Although the knowledge approach does provide a direction that should lead to progress in this field, it is not yet well enough specified to map out a clear path. Additionally, Murphy fails to describe fully the progress that has been made along older and more established paths.

Is *The Big Book of Concepts* too big, too small, or just right? Although it is not quite big enough to spell out the potential existing theories have to account for the phenomena it describes, this book is an excellent resource for travelers in the conceptual landscape.

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Letters

Task switching and the pied homunculus: where are we being led?

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In his review of the literature on task switching, Monsell [1] focuses on switch cost and its moderating factors, presumably because 'switch cost might seem to offer an index of the control processes involved in reconnecting and reconfiguring the various modules in our brains, so as to perform one task rather than another' ([2], p. 16). More concisely, according to his view switch cost is important because it measures the time needed to perform 'a sort of mental "gear changing"' ([1], p. 135).

Monsell's narrative favors the processes and phenomena identified with his switching homunculus, but in the end it might pay to emphasize others. Familiar processes like perceptual encoding [3], priming (e.g. [4]), and proactive interference [5,6] are active in cognitive control, and may well be enough, in combination, to accomplish the task of task switching. Switch cost might then simply emerge from interactions among such processes, and would not, after all, measure the bustling activity of modules being reconnected and reconfigured.

Likewise, the phenomena of cognitive control are materially more diverse than Monsell's review suggests. There is, for example, an encoding cost that applies to every instructional cue, regardless of whether the cue switches the task [3]. This encoding cost is much larger than switch cost [7,8], and indeed switch cost can be interpreted simply as encoding moderated by repetition priming [9]. There is within-run slowing, in which response latencies gradually increase across an uninterrupted sequence of same-task trials [7,9]. (The data in Fig. 1 in Monsell's review [1] are not a counterexample to this effect, because in that study the task cue was not a memory load [9].) There is within-run error increase, indicating that within-run slowing is not simply half of a speed–accuracy tradeoff [7,9]. Finally, there is a qualitatively distinct switch cost, which spans all trials following a task cue and is measured not in terms of response latency but in terms of error [9]. Monsell reviews only the switch cost on the first trial following a task cue, which is always evident in response latency.

On the surface these diverse phenomena might seem to have little to do with one another (or with Monsell's switch cost), but any viable model of task switching will need to accommodate them, ideally in some functionally integrated way. There is a model that does [9], but one need not

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accept this particular model to recognize that Monsell has selected the literature according to one particular theoretical view; that is, to include only those phenomena that fit within his 'mental gear changing' perspective. This is his prerogative, of course, but we are not helping ourselves if we focus so intently on switch cost and the switching homunculus that we lose sight of the larger pieces of the puzzle and how they fit together.

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Letters Response

Task-set reconfiguration processes do not imply a control homunuculus: Reply to Altmann

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Strict limits on the length of my review [1] prohibited mention of numerous effects observed in task-switching experiments, as well as other 'phenomena of cognitive control' (Altmann's preceding Letter [2]). I focused on the widely observed phenomena of task-switch cost, its reduction with preparation, residual cost and mixing cost. Altmann's interesting but still controversial observations [3,4] were among many regretfully consigned to the cutting-room floor.

'Time needed' by task-set reconfiguration (TSR) processes was but one of three sources of switch costs I endorsed. The others (task-set inertia and associative retrieval) elaborated the ideas of 'priming' and 'proactive interference' – 'familiar processes' of the kind Altmann considers sufficient to explain switch costs (although how these carry-over effects prolong reaction time is so far rarely specified in detail).

Is time needed for TSR explanatorily superfluous? TSR operations are certainly triggered by perceptual cues and memory, and use and act upon (some kind of) memory, but it seems undeniable that they must be *done*. For example, to switch from naming objects to reaching for them requires some sort of enabling and/or disabling of connections between object perception, speech production and reaching 'modules'. When and how the durations of such operations are manifest in behaviour, and whether they are continuous [5] or discrete [6] processes, are debatable. But to assert that we do not need TSR operations, only 'familiar processes' of perception and memory, is like attributing the time it takes me to leave home each morning to my perception of the muesli and my remembering to have breakfast, ignoring how long it takes me to eat it!

Logan and Bundesens's [7] claim – that the task-cueing preparation effect is attributable solely to cue interpretation – is certainly troubling if we hope to use this effect to measure the time course of TSR operations. However, in my laboratory we are finding that although some pairs of tasks and cues replicate Logan and Bundesen's observations, others do not. Watch this space!

Strangest of all, why does Altmann think that in attributing part of the switch cost to the duration of TSR processes I favour a separate, unitary, homuncular agent?. Altmann's own minimalist trace-strength model [3] postulates time-consuming voluntary operations (instruction-trace strengthenings) – performed by what agent? Far from wishing to lure the good citizens of cognitive science back into the homuncular Dark Ages, I would like to pipe them towards the view indicated by the title, 'Banishing the control homunculus', and the contents, of my chapter with Driver [8] that Altmann quotes.

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