Block remarks, we cannot stop subjects thinking. At most, we can match their thinking across conditions (or intervals). However, to match thinking requires conditions that are indiscriminable in some relevant respect. Yet, indiscriminable stimuli look the same. Consequently, all sides will predict matching frontal activity between conditions with and without transitions. Thus, such paradigms cannot discriminate rival hypotheses concerning NCCs.

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Letter

Finessing the Bored Monkey Problem

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By recording from microelectrodes in monkey prefrontal cortex (PFC), researchers have decoded the contents of conscious perception in cognitive areas (lateral prefrontal cortex) in conditions in which perceptions are not determined by the stimulus, binocular rivalry, and flash suppression [1–4]. As I noted in my recent *Trends in Cognitive Sciences* article [5], such results cannot be taken to support cognitive theories of consciousness because of the ‘bored monkey problem’: the idea that subjects whose only task is fixating a dot may have thoughts about the noticeably different stimuli, causing prefrontal differences that do not reflect prefrontal consciousness. This was the negative point of my article, and in their commentary Phillips and Morales (P&M) [6] do not dispute it.

What they do dispute is my positive point: that Brascamp *et al.* [7] have evaded the bored monkey problem. One innovation introduced by Brascamp *et al.* is to use stimuli that have two related useful properties. The first is that the stimuli do not afford any ready-to-hand cognitive categories for characterizing them other than as moving dots. Subjects cannot say to themselves: ‘There is the face again.’ P&M say ‘Nothing in Brascamp *et al.*'s methodology prevents observers engaging in extensive cognitive processing’, both in the rivalry transitions and the similar real (objective) transitions. However, P&M are neglecting the fact that the stimuli do not naturally draw cognitive processing in either the rivalry case or the real case. P&M focus on the distinguishability of the rivalrous transitions from the nonrivalrous real (objective) transitions, emphasizing replay subtraction. However, the aforementioned monkey experiments [1–4] do not use any form of replay subtraction. This research does involve comparisons between perception of the rivalrous stimuli and perception of real stimuli, but the purpose is to ascertain which neurons respond to the percept rather than to the stimulus.

The second useful property of these stimuli is that they are subjectively different from each other without being conspicuously different. P&M [6] say: ‘Yet indiscriminable stimuli look the same.’ However, although the stimuli are not noticeably different, they are subjectively different: they differ from each other in the directions of movement of each dot and in the overall directions of motion of the dots. Indeed, they are sufficiently subjectively different to trigger conscious rivalry.

I mentioned [5] that rivalry occurs in fruit flies and can occur in unconscious perception. P&M conclude that the rivalry in Brascamp *et al.* might be invisible. However, one cannot generalize in this way from rivalry when subjects do not consciously see the stimuli. Rivalry involves the dominance of one whole neural coalition over another. I know of no evidence that rivalry in the case of consciously seen stimuli can somehow slice off the conscious part of the coalition. The competing stimuli are subjectively but not noticeably different. Not being noticeably different, rivalry transitions are less likely to draw more attention than real transitions – as confirmed by Brascamp *et al*.

Would the differences between the neural representations of such stimuli be decodable in the brain at all given how similar they are? Recall that the explanation of binocular rivalry is that pools of neurons that represent each of the stimuli are mutually inhibitory. In the presence of neural noise, one pool wins out. The dominant pool of neurons then weakens due to adaptation, the other pool taking over in the winner-takes-all process of perception, then the cycle repeats. If this weakening and strengthening of content representations were happening in PFC, it would be detectable, either with fMRI or with electrophysiological methods (microelectrodes inserted into the cortex). Not finding differences between the rivalrous changes and the real changes is evidence against PFC differences and hence against cognitive theories of conscious contents.

This point interacts with the issue of the not-noticeably different stimuli. With readily characterizable (e.g., face/house) stimuli, weakening and strengthening in visual cortex could have caused a cognitive reflection of that weakening and strengthening in PFC, misleading us as to the role of consciousness in PFC.

Returning to the comparison of the rivalrous transitions with the nonrivalrous real transitions, P&M concede that failure to find PFC differences between rivalrous and real transitions shows that the causes of rivalrous transitions are not to be found in PFC. However, they go on to say that the conscious contents may nonetheless be in PFC. They use this point to conclude that methodologies that compare real with rivalrous transitions – including subtracting replay from rivalry – 'cannot discriminate rival hypotheses concerning NCC's' (neural correlate of consciousness).

Note that the neural bases of transitions and contents are linked. In particular, if perceptual contents are based in PFC, the strengthening and weakening of content representations that are intrinsic to binocular rivalry would make rivalrous transitions neurally different from real transitions, as noted above. Contents and differences in content have consequences for transitions. So, failure to find differences between rivalrous transitions and real transitions in cognitive areas of PFC disconfirms cognitive theories of conscious contents.

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Spotlight

The Hippocampal Cognitive Map: One Space or Many?

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New evidence reported by Solomon *et al.* that hippocampal activity tracks distance in semantic space during recall supports the growing consensus of a domain-general cognitive map. Nevertheless, are all inputs equally processed into a 'universal map', or are there input constraints (e.g., space, semantics) that lead to differentiated multiple maps across the hippocampus that have distinct properties?

Observing the impressive learning of rats in complex mazes, Edward Tolman concluded that their brain must form an internal representation of the environment that transcends individual associations between maze junctions and actions needed for reward [1]. He termed this system a 'cognitive map' and argued that humans share this with rodents. He also postulated that humans map abstract entities. The discovery of 'place cells' in the rodent hippocampus, which encode location in the environment via their spatially localized firing patterns, led O'Keefe and Nadel [2] to propose that the hippocampus is the neural locus of the cognitive map, and endows an animal with an environment-centric spatial memory system which, in a literal sense, maps space. The hallmark of this hippocampal map was the capacity to take efficient detours and exploit shortcuts through unexplored space. The decades following the discovery of place cells have witnessed an explosion in the discovery of spatial coding cells within and beyond the hippocampus, and their extension to humans [3].

Researchers have recently revisited Tolman's original suggestion that the hippocampus also maps non-spatial information [3,4]. The argument that space is simply one of many entities processed by the hippocampus has long been argued [5], but recent evidence reveals that non-spatial information can be 'mapped' in the hippocampus along stimulus dimensions such as sound frequencies, visual properties, and social hierarchies [3,4]. Notably, O'Keefe and Nadel [2] also argued that the human hippocampus maps abstract space because of encroachment of language processing into the temporal lobes, but did not