From semantic analogy to theoretical confusion?

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Abstract: We briefly address three issues that might be important to evaluate the validity of the "emulation theory": (1) Does it really say something new? (2) Are similar processes engaged in action, imagery, and perception? (3) Does a brain amodal emulator exist?

In this nicely written paper, Grush proposes the "emulation theory of representation" as a unifying principle able to synthesize "a wide variety of representational functions of the brain" (target article, Abstract). This attempt to merge heterogeneous models into a single conceptual framework is meritorious. However, based on the following arguments, we feel that this idea remains highly debatable scientifically, although it is seductive intellectually.

A "new" theory? The idea that common emulators might be used by different functions of the brain is not new, as acknowledged in the target article. For instance, a motor theory of perception has long existed in psychology and has been convincingly supported by behavioral studies (e.g., Viviani & Stucchi 1989; 1992). In the same vein, optimal estimation has long been described as a pivot between the motor and sensory domains. Under this concept are grouped statistical methods devised to extract the valuable part of a noisy signal knowing a priori information. In the central nervous system, this information can be a motor command or an a priori belief on the sensor input. The theoretical concept of optimal estimation (and, in particular, Kalman filtering or KF) has been quite convincingly argued to apply to the analysis of sensory signals, whether visual or proprioceptive (Rao & Ballard 1999; Todorov & Jordan 2002; van Beers et al. 1999; 2002; Weiss et al. 2002; Wolpert et al. 1995), thus establishing a clear link between visual perception and motor control at the level of sensory processing.

The main aim of the emulation theory was to extend this link to a much larger variety of processes. However, only remote and conjectural arguments are presented in the target article with respect to this goal. In other words, there is a nice description of several items of evidence in favor of partial links that have been known to exist for a long time, but there is no clear articulation of these partial links into a general model. In this sense, Grush's theory cannot be considered truly new. Its scientific support reaches the same boundaries as the previous unarticulated theories. The only articulation between these theories lies in conjectural assertions and in the semantic confusion introduced by terms such as "emulation," "prediction," and "estimation." We do not want to seem excessively discourteous, but all that seems to hold at the end of the article might be something like "emulation processes take place in the brain for various functions." To make his claim more convincing, Grush has failed to address key issues such as: (1) What, besides the word "emulation," is common between the predictive activities involved in tasks as different as guiding the hand toward a target (motor control), generating a structured sentence (language), or determining where "Maxi will look" (theory of mind)? (2) What could be the nature of the common substrate that is postulated to be involved in those incredibly dissimilar tasks?

Are similar processes engaged in action, imagery, and perception? One of the main claims of Grush's article is that the "emulator" used for controlling action can be used for imagery. However, as far as motor imagery is concerned, strong interferences have been demonstrated to exist between actual and represented postures (Sirigu & Duhamel 2001). This may suggest an exactly opposite interpretation of the Wexler experiments (Wexler et al. 1998), which are presented as a key support to the emulation theory. How can Grush rule out the possibility that the conflict takes place between the *sensory* outcome predicted by the actual motor command (through the forward model) and the mentally rotated one, and not between the actual motor command and the command necessary to rotate the object? It is quite difficult to see how "emulating" the rotation would simply be *possible* when the motor cortex is engaged in a task incompatible with the mental rotation (does this imply the existence of dual forward models?). In contrast, it is understandable that the voluntarily imagined visual scene can dominate the (involuntarily) predicted one, since both are constructs which are unrelated to the actual, static, visual feedback.

In parallel to the previous remarks, we argue that Grush's model also meets a problem when faced with neuropsychological evidence. Abnormal timing of imagined movements has been found in parietal patients with normal overt movements (Danckert et al. 2002; Sirigu et al. 1996), showing that motor imagery necessitates more than the simple prediction of sensory feedback used for online control. A similar conclusion was reached by Schwoebel et al. (2002). A functional dissociation seems also to exist between perception and action (Milner & Goodale 1996). For example, movement guidance relying on forward modeling has been shown to be dramatically impaired in a patient presenting with a bilateral posterior parietal lesion (Grea et al. 2002; Pisella et al. 2000). When submitted to standard neurological tests, this patient does not present cognitive or perceptual problems. Trying to extend the concept of emulation to other sorts of imagery is still more problematic. Indeed, dissociations between intact visual imagery and profoundly affected visual perception have been found in several patients (Bartolomeo et al. 1997; Beschin et al. 2000; Goldenberg et al. 1995; Servos et al. 1995). These results openly contradict the notion that visual imagery emerges via an "emulation" of normal vision through top-down processes.

The brain amodal emulator in perception. We were truly puzzled by the suggestion that an *amodal* emulator of the external world could exist in the brain. This claim seems to negate the rich literature documenting dissociations between our different senses (see, e.g., the intermodal conflicts generated by prism adaptation or pinna modification). For instance, biasing the input in a given sensory modality leads to an adaptation of that modality (e.g., the waterfall illusion in vision). It is possible that Grush would interpret this result as a change in the emulator (it is a change in the prior probabilities of object motion, which is part of our knowledge of the world – supposedly analogous to the command of a KF). However, in contrast to the prediction of an amodal emulator, it can be shown that this kind of adaptation does not transfer to other modalities. In fact, besides this remark, what seems to emerge from the recent research is the rooting of high-level supramodal abilities (such as conceptualization) in modality-specific experience (Barsalou et al. 2003).

Does the brain implement the Kalman filter?

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Abstract: The Kalman filtering technique is considered as a part of concurrent data-processing techniques also related to detection, parameter evaluation, and identification. The adaptive properties of the filter are discussed as being related to symmetrical brain structures.

Since the 1960s the data-processing community has been fascinated by the appearance of the new filtering technique (Kalman & Bucy 1961), which had naturally extended Wiener's filtering theory into the multidimensional time-variant domain. The clarity and simplicity of its structural design has allowed this technique to dominate for more than 40 years in different fields: technology, biology, and economics. Its popularity has grown tremendously.