

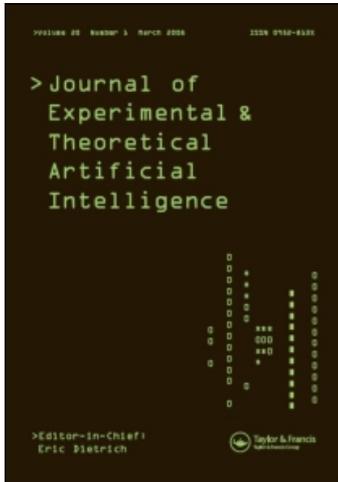
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Beyond red states and blue states in cognitive science

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RESPONSE

Beyond red states and blue states in cognitive science

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As in political debate, the commentaries in this issue mostly tend towards the two ends of a similar spectrum: 'one true path' *versus* 'anything goes'. We hope to persuade swing voters of a more moderate position: 'diversity without chaos'.

Spivey and Anderson (this issue) provide an excellent illustration of our claim that a belief in ontologically privileged descriptions leads to both epiphenomenalism and to thinly disguised contempt. In a contemporary variation on 'All animals are equal, but some animals are more equal than others' (Orwell 1946), they state:

... there is indeed no privileged level of organisation, but of any pair of adjacent levels, it is the lower level whose causal forces generally produce the global patterns that are observed at the higher level, not the other way around.

and

Modelling the mind with rules and symbols has been an extremely useful intermediate step towards understanding how human cognition works, providing a first approximation at a higher level of organisation. However, it is now time for cognitive science to continue its movement closer and closer to the original ontic stream of cognition.

In other words, as paraphrased in advance in our paper, 'while the theories of others may be *useful*, mine is *true*'. Their actual position is demonstrated in more direct claims such as 'global coherent phenomena are undeniably caused by the lower levels, not the other way around', and in their reference to 'the illusion of symbolic processing'.

A complete rebuttal of Spivey and Anderson's argument would require a thorough analysis of their billiard-ball view of the concept of causality. While there is not space for that analysis here, a quick summary of our instrumentalism-inspired approach is that causality is a concept of description, not of ontology. If we agree on metaphysical identity – that external reality is what it is – then causality arises from our need to divide that external reality into smaller pieces, hierarchies, and temporal orderings in order to comprehend it at all. Once we divide it up – a division imposed epistemically by us as observers – we need a concept of causality to describe the interactions among the parts. Under this view of causation, it is equally reasonable to say that the neural activity causes the symbols or that the symbols cause the neural activity, because causality is merely the

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relationship among our epistemically imposed hierarchical divisions. The unified whole does what it does, regardless of our choice of divisions or descriptions.

In any case, after reading Spivey and Anderson's commentary one wonders about the relevance of even discussing neural activity. After all, this is a mere 'cultural convenience' or 'descriptive shortcut', perhaps useful for describing certain phenomena, but it should not be separated from the original ontic stream that occurs at the level of atoms... or perhaps electrons and nuclei... or quarks, strings, turtles all the way down, or whatever is at the physical bottom of Spivey and Anderson's hierarchy. In their note 3, they claim that 'the dynamics of clouds are better understood when one takes into account the fact that they are collections of water droplets'. Would we not understand clouds even better by taking into account the fact that water droplets are themselves collections of water *molecules*? Surely Spivey and Anderson would not claim that the water droplets 'cause' the behaviour of the water molecules given their view on causality. Why are water droplets the right level of description? Spivey and Anderson consistently select intermediate levels of analysis that suit their purposes, and then claim that these levels have ontic priority.

Finally, they construct the straw man of 'a pluralist approach that allows the level of global coherent units of thought to exist without being reducible to their neural and biomechanical underpinnings'. Although we heartily agree that too many cognitive scientists have insisted on the symbolic-only approach, these scientists are not pluralists; indeed, they are generally of the same philosophical bent as Spivey and Anderson, merely granting ontological privilege to a different level of description. The point of the SAL architecture, as is illustrated rather at length in our paper, is to try to understand what is really going on between the different levels of description – not only to discover how symbols 'arise' from the emergent dynamics (reduction), but also to learn how the symbols 'influence' the lower-level dynamics (synthesis). Both directions (and others) are crucial. The physical sciences have long understood this mutually beneficial approach between levels of description without denying the epistemological importance of reductionist mechanics, and it is time for cognitive science to do so as well.

In contrast, we find much to agree with in Dietrich (this issue), even beyond the lavish praise he gives to our SAL architecture. Our primary difficulty here is in understanding the difference between his 'radical pluralism' (defined as 'multiple paradigms of wildly different kinds') and mere pluralism, aside from the addition of superlatives. Pluralism necessarily incorporates the idea that different theories are incommensurable – otherwise they are either isomorphic or could be merged into a single theory. Thus it appears that this is not really a philosophical difference, but a scientific claim that *computation* is an overly restrictive paradigmatic umbrella for theories of cognitive science.

Perhaps oddly, given that we all study cognition through computational simulations, we are not strict computationalists in the sense of treating the Turing equivalence of the brain/mind as axiomatic. Rather we are inclined to withhold judgement on this question until we *understand what the brain/mind is actually doing*, hence our emphasis on reproducing results from psychology and neuroscience laboratories. Although clearly we have a suspicion and intuition that key cognitive phenomena have a computational equivalent, our version of pluralism in cognitive science is certainly open to theories or descriptions outside the realm of computation. Thus, once again we do not really see a distinction between pluralism and Dietrich's radical pluralism.

We do find fault with Dietrich's argument that cognitive science must be radically pluralistic because 'the radically pluralistic *universe* in fact *resides* in the brain and its mind'. This cannot be right, not only because it results in an infinite regress, but also

because what resides in the brain is or results from only our *experience or perception* of the universe. Furthermore, this claim ignores the process of abstraction, the importance of which Dietrich earlier emphasises, as a way of simplifying the model of the universe that does reside in the brain, and perhaps even making all thinking possible:

without general meanings, one can make no assertions at all, not even such as are singular, and that one cannot talk of thinking, judging or knowing, in a sense relevant to logic, on a mere foundation of the direct presentations of individuals. The most ideal adaptation of the human mind to the multiplicity of individual things, the genuine, effortless realisation of adequate individual conceptions, would not render thought superfluous. For performances attainable in this manner are not performances of thinking. (Husserl 1901, p. 277).

Consequently, while it should be clear from our paper that we agree with the assertion that the brain is extremely complicated, it is nevertheless quite homogeneous in comparison to the complexity of the universe as a whole. In the end, it seems that our difference with Dietrich is that we are somewhat more optimistic about the prospect of ultimately mapping and connecting theories of cognition, so that even while there are different and incommensurable paradigms and viewpoints there will be at least some common ground among them all.

Like Dietrich, Van Orden (this issue) suggests that we perhaps do not go far enough. In both cases, this may be a consequence of our emphasis on the convergence and similarities between Leabra and ACT-R, suggesting that our approach is purely one of practical collaboration to enable the eventual merger of plural theories. While this possible benefit is certainly one of our main points, it was not intended to limit the scope of pluralism. Even while we pursue new learning through SAL, ACT-R and Leabra will almost certainly maintain separate research programmes, because they fundamentally explore different aspects of cognition. An analogy between the mind/brain and the wave/particle duality is apt: we maintain the complementary views because they explain different aspects or manifestations of the same physical reality. Pluralism is about the practical benefits of open-mindedness and collaboration, to be sure, but it also requires that we accept the possibility of incommensurable complementary theories holding sway simultaneously.

Markman (this issue) echoes and expands upon our position that pluralism is not tantamount to relativism, and provides valuable guidance on why and how this is important in cognitive science. We particularly like his incisive phrase ‘theoretical elegance is not a desideratum for biological systems’. For this very reason, we do not share his concern that parsimony is called into question by the pluralist.

The principle of parsimony (which, perhaps ironically or perhaps illustrating the point, has been stated in a number of different ways) only requires that simpler theories be preferred over more complex theories if they account for the same set of phenomena. To the extent that phenomena remain unexplained by an existing theory, additional incommensurable theories are justified even under a regime of parsimony. In physics, wave/particle duality again comes to mind as a good example of this. But pluralism is not a cultural diversity initiative – it does not encourage incommensurable theories for their own sake. Such theories are only valuable to the extent that they contribute novel explanation. As Markman recommends, the pluralist still seeks to integrate and unify theories, or to propose new more broadly explanatory theories if such are possible. As instrumentalists, our view is that simpler theories are better *because* they are simpler, not because there is a deeper ontological significance to the simplicity; this position should be easier to grasp in cognitive science due to the fact that we are dealing with an evolved biological system.

We would also augment Markman's criteria of theory evaluation to explicitly include *novel predictions* (Lakatos 1980). This partially overlaps with his third criterion of seeking theories that lead to new questions, but is important in its own right. There is nothing more stunning and convincing in science than when an unexpected and even bizarre prediction is confirmed.

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