

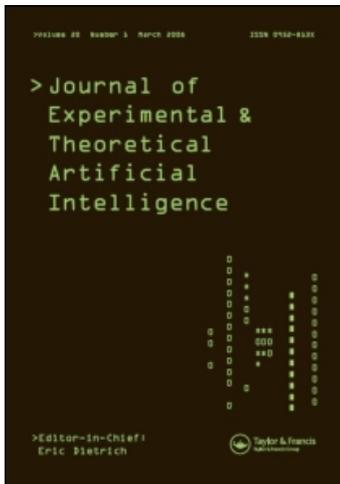
This article was downloaded by: [Dale, Rick]

On: 14 August 2008

Access details: Access Details: [subscription number 901619995]

Publisher Taylor & Francis

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Journal of Experimental & Theoretical Artificial Intelligence

Publication details, including instructions for authors and subscription information:

<http://www.informaworld.com/smpp/title-content=t713723652>

Sloughing ontology

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Online Publication Date: 01 September 2008

To cite this Article Dale, Rick(2008)'Sloughing ontology', Journal of Experimental & Theoretical Artificial Intelligence,20:3,251 — 256

To link to this Article: DOI: 10.1080/09528130802319292

URL: <http://dx.doi.org/10.1080/09528130802319292>

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RESPONSE

Sloughing ontology

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I am surprised to be replying to a series of commentaries that agree with the basic premise of pluralism. Two pairs of commentators were invited to offer evaluations of pluralism from contrasting positions.¹ One pair, Dietrich and Markman, have previously identified ‘the importance of...representational pluralism in cognitive science’ (Dietrich and Markman 2003, p. 116) – but such statements are reserved for a modest paragraph at the very end of a long paper, with most of that paper centred around rather traditional notions of cognition (see Markman 1999, for a thorough treatment of multiple forms of representation). In this issue, in their separate commentaries, Markman and Dietrich enthusiastically endorse the pluralist view; I critique some specifics of their commentaries below. The most ‘fundamentalist’ perspective, that of Spivey and Anderson, espouses a family of frameworks rather than just one: ‘... a complex dynamical systems approach (fuelled *also by ecological psychology, embodied cognition, and connectionism*)’. (Spivey and Anderson, this issue, my emphasis). The frameworks they refer to are of course intimately inter-related, and this is one aspect of the *integrative* pluralism that I argue for in my article (Sections 4.1 and 4.4). So along with the endorsement of Van Orden (however reluctant), it seems that there is a unanimous recognition of the value of integrating diverse perspectives, rather than inflexibly arguing for one at the expense of all others. So, if I may be so bold, now that all contributors to the special issue agree that some form of pluralism is needed, we can work out further details.

One concern for pluralism is how diverse the co-existing frameworks might be. I suggested in my article that symbolic and dynamical perspectives, though apparently inconsistent, could nevertheless co-exist as important schemes in cognitive science, contributing explanations to differing sub-domains of cognitive phenomena. But Spivey and Anderson wish to relegate all symbolic descriptions to ‘descriptive shortcuts’ in hopes of taking cognitive science closer and closer to the ‘ontic stream’ of cognition. Their clearly stated and direct challenge to pluralism is based on an ultimate perspective on cognition based in neural dynamics. If a true reductionism were adopted, as recommended in their commentary, then ‘neuron’ must itself be a descriptive shortcut:

Even if cognition and brain function should turn out to be described as a temporally coded dynamics with no static symbol structures, this would not adequately describe the quiescent molecular structures that form the genome and the coding constraints that have been controlling protein synthesis for billions of years. (Pattee 2001, p. 8)

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This remark by Howard Pattee does not include the ultimate ontic stream of the conventional reductionist view: the various subatomic particles and their laws that are theorised to make up our physical reality (which Pattee includes in subsequent discussion). It may not be uncommon in one's early philosophy training to find oneself discussing how, according to a reductionist approach, apples and chairs are even convenient fictions. So Spivey and Anderson's bold position cannot be a true brand of reductionism in one sense, because they choose to adopt the descriptive shortcut of the neuron and the neural pattern.

If one accepts that there are hierarchies of *explanation*, then the neuron is a wonderfully natural place to start (just as many cognitive scientists do). But, from that very logic, it may be useful to start from far more abstract units of measure for some problems: words, sentences, conversation topics, events (e.g. going to a movie), lifespan episodes (e.g. depression), and more. In fact, in some sense, the workaday application of cognitive science is often more amenable to its units of measure that are more abstract than neurons or neural patterns. This is because we are concerned with *understanding*, *explaining*, etc. a system, not identifying some 'underlying ontological character' of a system.

But isn't understanding achieved best by identifying the system's underlying ontic stream? Not if you embrace even modest emergentism, and Spivey and Anderson do. If reductionism were truly espoused, then neurons cannot actually be that underlying ontically streaming thing anyway (for reasons just stated). Suppose they were. Even in this case, the very idea of emergentism regards how we *understand* a system in the context of *measurement*. Emergentism, in the clichéd phraseology, means that a system's properties are 'more than the sum of its parts'. It is true that a radical version of this would deny looking to reduction as one strategy to contribute to that understanding. I suggest otherwise in my target article, contrary to Spivey and Anderson's interpretation (Section 4.1). However, this common conception of emergent properties does gainsay the pejorative 'descriptive shortcut' phrase used by Spivey and Anderson: the very idea of an emergent property means that it does not make sense to overemphasise low-level characteristics, because something is lost in scientific understanding. Insisting on focusing on some low-level ontology for cognitive science can lead to neglect of this fundamental epistemological aspect of emergentism. Besides, if they are right about adding ecological psychology to their plural mix, along with (by implication) elements of an externalist perspective, then neurons and neural patterns can only be part of the story: structured cognitive performance emerges out of the inextricable interaction between brain and environment. What is cognition's ontic stream then?

The philosophical bugbear for Spivey and Anderson is downward causation. But there is, in my mind, good reason why we cognitive scientists need not worry. One primary reason derives from the concerns just described: even if we assumed 'causal closure' (Lowe 2000) of high-level properties by low-level properties, there may exist emergent high-level relations that are 'causal' in epistemological character. On this view, we should identify causes where they lie, in the richness of inter-relations among our measures. Godfrey-Smith (in press) discusses causation as a concept that is, in fact, not poorly defined but rather multiply defined. 'Cause' is about relations among events, and events can of course exhibit substantial ranges of granularity in their definition (Zacks and Tversky 2001). Consider this scenario: your friend bets a thousand dollars that you can't drink 12 shots of Jägermeister in a night. You take him on. You find yourself out of sorts quite rapidly. What 'caused' your state? Was it your friend? Was it Jägermeister? Was it

the 12th shot of Jägermeister? Was it your GABA receptors? Was it the academician's propensities to indulge? Was it the promising purse of a victory? Any such identification can be causal, and which one you grant privilege to is determined by what spatiotemporal grain size (Bechtel and Mundale 1999) concerns you in your own pocket of cognitive science.

But this diversity of causal attribution, rich epistemological relations among multiple measures, need not cause us to scream 'run for the hills!' as Dietrich (this issue) seems to suggest. Though Dietrich and I are in much agreement over the fundamental need for accommodating theoretical diversity, we seem to disagree over how radical cognitive science's pluralism might be. In anticipation of this immediate reaction to plural possibilities, I argued that meta-theoretical strategies in cognitive science are an important and almost wide-open avenue of investigation (Section 4–4.3). These strategies, in fact, invoke a pinch of the reductionism Spivey and Anderson desire. I believe systematic, high-level measurements and their causal relations can almost always be usefully informed by identifying relevant low-level (such as neural) implications, as recommended by Bill Bechtel and his colleagues in several papers (e.g. Bechtel and Mundale 1999; Bechtel 2001; as McCauley and Bechtel (2001) note, this information can flow in the opposite direction as well). Dietrich seems to downplay the potential of this *integrative* pluralism, in which it is an active and interactive research programme to interface apparently competing theories. We can therefore combat *radical* pluralism by engaging in meta-theoretical and integrative strategies. Jilk and co-workers (this issue) offer an example of one kind of integration, by developing simulated systems intended to generate complex behaviour. As I argued in my article, complex cognitive performance is currently more successfully simulated in artificial systems that have plurality as a central feature. The goals of this and other strategies are to lend coherence to a scientific enterprise that is pluralistic. More often in science that coherence is sought by urging one particular framework over others. In a pluralistic cognitive science, coherence could be pursued by finding ways in which apparently competing theories can communicate or complement each other – or even both derive from some other meta-theoretical framework.

Dietrich supplies some examples of outstanding problems with which cognitive science would seem to have trouble, with the apparent intention of striking fear in the hearts of new graduate students: 'how emotions fold into thoughts', the 'thought required to write a book', and of course consciousness and creativity. I suspect that a plural approach to cognition, and an integrative one, will more likely succeed at suggesting answers to these and other tricky problems.

A radical pluralism like Dietrich's has greater risk of falling into the relativism that Markman (this issue) warns about. I agree with many of Markman's points, and his invocation of broader criteria for 'good' scientific theories is a valuable topic to discuss in the context of considering pluralism. Many cognitive scientists often focus on falsificationism as an important criterion, a Popperian strategy that quickly had philosophical detractors (and is a perspective that no longer deeply influences in philosophy of science; Boyd 2001). This and other criteria, such as simplicity, cannot be used as firm rules, but instead function as loose rules of thumb, particularly in view of complex, historically contingent biological systems (as Markman also notes). But even radical pluralism would import these heuristics, and apply them to the various pockets of application of the micro-theories on offer. An integrative pluralism, however, not only imports similar criteria, but could add new conditions that make a theory seem more promising. For example, one such criterion could be the facility with which that theory can

jive with other successful theories in cognitive science. This ‘integrative’ criterion would not be unlike the ‘cross-scientific relations’ of theories discussed by McCauley and Bechtel (2001), noting that a theory that interacts well with other successful ones makes it more robust and desirable. An integrative pluralism thus imports all our common conceptions of what makes good theories, while inviting new ideas about what we should expect from our theories.

Spivey and Anderson facetiously describe my contribution as stating, in short, that pluralism is right because everyone is right. This is surely relativism (were it true). I deliberately focussed on the positive thesis of pluralism: possible research strategies that grow out of an attitude of constructive inclusion, rather than assumptions of mutual theoretical exclusivity. I argued that this plural attitude is rare, and my focus on it admittedly may have lent the article an air of the Partridge Family. However, if such strategies were more thoroughly carried out, I did not intend to imply that everyone is or will be right. In fact, the positive thesis of pluralism entails conditions of ‘wrongness’. By developing new criteria in an integrative approach, we could dismiss some theories (or some versions of some theories) that are not particularly helpful in either their local sphere of application, or this hoped-for integration. Historical examples of such theories might include strict Aristotelian notions of categories and concepts (Medin 1989), or the tiny mathematical micro-theories of the learning-to-learn movement that eventually died out (e.g. Restle 1955) – both of which were simply too inflexible to accommodate new data, and do not easily accommodate theoretical neighbours.

So I would not argue that all theories get a pass in an integrative pluralism. When theoretical domains overlap, they naturally compete to account for these overlapping phenomena. Even when such debate is mitigated (I briefly offer examples in Section 3), these competing theories could nevertheless co-exist in non-overlapping data domains. Such theories may invoke sharply differing theoretical constructs. If one accepts pluralism then, for Van Orden (this issue), this implies an adoption of a diverse metaphysics, wherein one accepts the *reality* of these constructs. As Van Orden observed, my contribution argued from Putnam that ‘ontology talk’ is not a particularly useful way of speaking, and may generate pointless debate when disputants concern themselves with some very narrow notion of ‘exist’, a word that is, in fact, diverse in meaning (Putnam 2004). In this view, I think identifying some ‘underlying ontology’ of a system as complex as human beings should be seen as silly (Section 5), and based on idealistic notions of the physical sciences, which to some extent still carry out such idealistic aspirations (e.g. Deutsch 1998), even if they are applied in only very circumscribed experimental contexts, far away from higher-level lawful relations in our everyday lives (Cartwright 1999).

Does it matter whether one argues for the existence of a symbol, if it’s a powerfully useful unit of analysis in some high-level cognitive contexts? Maybe – but I argue in my article that we should be more careful about this focus on ontology (Section 5). I would lean on the contribution of these units instead to *problems of knowledge*, like many pragmatic philosophers (Dewey and Bentley 1949; Putnam 2004), rather than ‘problems of reality’. The former problems have pretty clear criteria for success: prediction, understanding and application, among others. The latter problems seem more vague, and given considerations of emergentism and reductionism above, less coherent in important ways.

I do agree with Van Orden that the plurality of explanatory schemes is intimately linked to the diversity of measurement contexts. These contexts vary widely in their

spatiotemporal scale and explanatory goals. Human beings are sufficiently complex systems that diverse, sometimes apparently inconsistent, theoretical frameworks may account for these different modes of measurement. I argued that this is, in fact, a consequence of emergentism and complex systems. Complex systems science thus invites a plural approach, embracing a theoretical vocabulary where it fits best to handle particular measurements. When one is anchored to the idea that science is in the business of uncovering some ‘underlying reality’, and in particular *one* such underlying reality, then pluralism can indeed befuddle. Such is the idea of the title of this response. Like an organism sloughing its skin and entering a new phase of growth, so should cognitive science perhaps slough the notion of ‘pure ontologies’ for its science. The goals of an integrative pluralism are to identify the families of theories that together, in all their diversity, capture what humans are like in various modes of measurement. Through this integrative strategy, through linking higher- and lower-levels of explanation, driven by dynamics and capitalising on the power of computationalism where needed, cognitive science can embrace the complexity of its subject matter, while looking for new ways of attaining explanatory coherence.

Note

1. I supplied a caveat about such words as *theory*, *framework*, *explanation*, *description*, etc. in Section 2 of my target article. I apply this caveat here.

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