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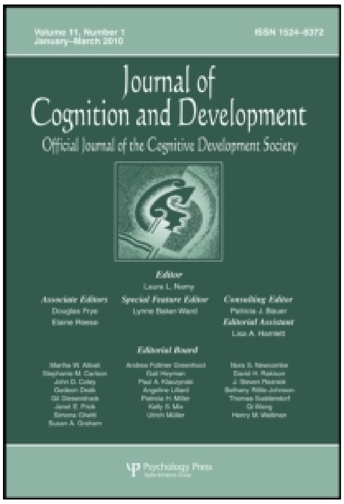

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Early Domain-Specific Knowledge? Nonlinear Developmental Trajectories Further Erode a House of Sand

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Rakison and Yermolayeva (this issue) argue that domain specificity is difficult to reconcile with U-, N-, or M-shaped developmental trends. They are justified because: 1) There is no compelling evidence that nonlinear trends require mechanisms beyond general, well-known cognitive processes; and 2) epigenetic neuroscience provides no clear evidence of strong domain-specialized representations. Evidence implies pervasive weak (i.e., experience dependent) neural specialization for different kinds of information.

The main points made by Rakison and Yermolayeva (this issue) are correct: The fact that U-shaped curves are really N-shaped is clear. Nonlinear trajectories can indeed be explained by changes in general cognitive resources, in the context of age- and learning-related changes in task demands. By contrast, as Rakison and Yermolayeva assert, nonlinear trajectories add an explanatory burden to domain-specific models. I will elaborate on the last claim by discussing first why domain specificity must overcome the advantage of parsimony enjoyed by domain-general accounts, and second, by clarifying why biological plausibility weighs against *strong* domain specificity.

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THE PROPER PLACE OF PARSIMONY

The concern about parsimony is not that a parsimonious account is more likely to be correct. It is that all else being equal, explanations should invoke the fewest *established* constructs and processes that can predict some given data. Additional (or other) constructs should be proposed only if the minimal account is inadequate. To determine when this is necessary, science stipulates conventional, obligatory practices, and skeptical criteria, for adding explanatory terms. As more accurate and comprehensive data are assimilated, new constructs and process models sometimes are needed. As a case in point, data might reveal that human judgments can only be explained by innate domain-specific conceptual knowledge. However, before accepting any such claim, we must ask whether those judgments can be explained using existing constructs.

By this model of science, the highly visible domain-specificity "movement" of the 1980s and 1990s was anomalous. Initially, evidence showed that infants and children respond to information in unexpected ways—for example, infants discriminated patterns of object motion or discriminated novel from familiar words earlier than scientists predicted. Such findings did not fit Piaget's theory, which was largely domain general. Thus, developmental psychologists sought an alternative. One alternative, Skinnerian behaviorism, was rejected for its long-recognized limitations. In doing so, however, learning was discarded as a "vanilla" process that could not yield nuanced judgments. Thus learning was rejected even as machine-learning techniques were yielding an ever-growing array of non-Skinnerian learning approaches (e.g., Hopfield, 1991; Sutton & Barto, 1998). The preferred alternative, *infant rationalism*, made strong domain-specificity claims for cognitive development: specifically, innateness of some "core" modular knowledge (Carey, 2000; Spelke & Kinzler, 2007). This work often did not discuss or investigate learning processes or general cognitive traits. Known processes like habituation or selective attention were relegated to methodological conveniences, not considered as possible components of alternative models.

Rakison and Yermolayeva (this issue) wish to separate the domain-specific/domain-general debate from the issue of so-called nature–nurture interactions; however, the reality is that strong claims for domain specificity in infants are intertwined with nativism—historically, philosophically, and in the current culture of our field. To be clear, we must distinguish *weak* domain specificity, which does not necessarily entail nativism, from strong domain specificity (i.e., Rakison and Yermolayeva's topic of concern), which *does* entail nativism. Under weak domain specificity, painting and piano playing, for example, are largely separable skills with substantially different neural resources. This claim is trivial and uncontroversial: Learned skills and sensitivities gradually acquire selective cortical resources (e.g., Gauthier, Tarr, Anderson, Skudlarksi, & Gore, 1999; McCandliss, Cohen, & Dehaene, 2003). However, if we take the "domain-specificity" label too seriously in these cases, we quickly proceed down a long regression that proves the uselessness of the label. For example, if there is a music module that emerges with practice, then we might have all sorts of other domain-specific modules, like playing white keys with left-ring finger, black keys with left-ring finger, etc. In fact, these fit neurological evidence (Indovina & Sanes, 2001). But it is unclear that anything is learned or revealed by adding the constructs "module" or "domain." Thus, the real controversy concerns the hypothesis of *strong* or *innate* domain-specific knowledge—that is, knowledge believed to be largely independent of experience. (What counts as "experience," and whether "innate" has any meaning in modern biological or social sciences, are questions to be dispatched elsewhere.)

The example of a regress from a music module to a left-ring-finger-twitch module illustrates why parsimony is fundamental to Rakison and Yermolayeva's (this issue) argument. If constructs for general cognitive processes, such as *attention window, working memory span*, and *visual habituation*, are "real" in some sense, we should first look to these to explain new phenomena. Nativist constructs like "intuitive mechanics" and "intuitive psychology" (Carey, 2000) can be posited if the general and better-grounded constructs (which after all predict many thousands of phenomena) cannot explain new phenomena—phenomena that, at first glance, might suggest some innate domain. Rakison and Yermolayeva argue that nonlinear trends can be explained by *general* cognitive traits but not by domain-specific faculties. In fact, Rakison and Yermolayeva claim that the latter are actually incompatible with nonlinear trends.

I am not sure that all of this last argument—that domain-specific faculties are inconsistent with nonlinearities—is valid. For example, Rakison and Yermolayeva (this issue) claim that an innate constraint, once triggered, should improve monotonically. But why is a monotonic progression inevitable? Traits might become dormant, or hold fairly steady for years. If other traits are maturing during this period, the expression of the steady-state trait will change (e.g., Thelen, Fisher, & Ridley-Johnson, 2002). Also, traits can be overridden or replaced as by formal education (Vosnaidou, 1995). So this stronger claim might not be valid, but it is really a secondary issue. Most important is this: The onus sits with proposers of domain-specific mechanisms to show that those mechanisms are necessary to account for complex data such as N-shaped trends, and that general mechanisms cannot explain those trends. Occam's razor does not necessitate a simple world. It *does* necessitate a reason for compounding hypothetical constructs. As

Rakison and Yermolayeva point out, general mechanisms *can* yield N-shaped trends. Thus, proponents of strong domain-specific conceptual development have an uphill battle.[1]

IS THIS EVEN AN ARGUMENT?

From a modern developmental neuroscience perspective, the domain-specific/domain-general argument makes little sense. The specificity with which different neural populations, networks, and pathways process information is entirely determined by their emergent physical instantiation (Stiles, 2008). If two patches of cortex develop different dendritic branching, different firing patterns, or divergent fiber tracts to subcortical nuclei, they will also develop distinct afferent and efferent properties, which relate to different stimulus properties and behavioral responses. If some of these differences are labeled "domain specific," that designation can only be meaningful if it is falsifiable. Consider that primary visual cortex and primary auditory cortex have anatomical, morphological, functional, and developmental differences. If anything should qualify as strong domain specificity, this should be it, because projections from thalamic nuclei begin extending to their final cortical target areas during the first trimester. Yet in what sense is it *further* informative to call these systems "domains" or "modules"? What does it teach us that the facts of the different sensory systems do not? Labels like "domain specific" highlight differences and minimize similarities. Vision-specialized and audition-specialized cortical regions do in fact share many similar neural processing and organizational properties. Moreover, neural specialization for different kinds of sensory energy is a dynamic epigenetic product that can be modified (Sharma, Angelucci, & Sur, 2000). This blurs the distinction between weak domain specificity, discussed above, and strong domain specificity. Here, again, we risk a long regress: If primary sensory areas are (somewhat) modular and (progressively) specialized, what about different columns in V1, or left versus right dorsal V2, or patches in V1 with different receptive fields? All are distinct in some ways. Thus, what we designate "domains" is arbitrary.

[1]Strong domain specificity runs against *methodological parsimony* (i.e., do not believe in anything "extra" without compelling evidence) and possibly *ontological parsimony* (i.e., assume that nature is no more complex than it need be; Crisci, 1982). The bar for ontological parsimony is set rather low in the biological and social sciences; for example, evolutionary biology has historically added many constructs to fit facts. Similarly, psychology has not been opposed to adding new constructs. Rather, the most central problem is methodological: Researchers proposing domain-specific faculties simply have not presented compelling evidence that the data cannot be explained by well-established domain-general explanations.

More damning, there is cortical redundancy for some functions—for example, in organized receptive fields for spatial maps (e.g., Hagler & Sereno, 2006). That is, directionally selective maps occur and recur in many patches of the occipital, parietal, and even frontal cortex. This suggests that the brain evolved to process the same spatial information in different ways for different functions. Such redundancy inveighs strongly against modularity and forces any notion of "domain specificity" into the aforementioned long regress.

CONCLUSIONS

The concept "domain specific" remains intractably vague and probably uninformative. Putative "core domains" (biology, psychology, mathematics, physics) seem to be based partly on intuition (or academic disciplines). Evidence of infants' and children's discriminations is sometimes interpreted through a prismatic lens that separates data into an orderly world of prior domains. Too often, factors like prior familiarity, information variability and entropy, and perceptual salience are not adequately considered. Rakison and Yermolayeva (this issue) argue, correctly, that domain-general processes can explain developmental trends. They also argue that domain-specific theories cannot accommodate nonlinear developmental trends. Although that is not strictly true, such theories are necessarily less parsimonious, and they therefore require quite compelling evidence—evidence that, to my knowledge, has never been reported for human infants. That aside, Rakison and Yermolayeva's point subscribes to a distinction that is itself questionable. In the biological and cognitive sciences, "domain specificity versus domain generality" is no longer a valid distinction. It is not meaningful (except in its weak version) from either a modern epigenetic approach or a current understanding of neuroscience. Constructs like "core knowledge" and "innate constraints" sidestep the challenge of explaining how children acquire knowledge for different tasks at different ages. Rakison and Yermolayeva's analysis points out one more reason why modular/nativist/domain-specificity approaches are no longer useful: They do not address how ecological, neural, and behavioral factors create a matrix of constraints that, in the context of biological change and experience accrual, progressively yield more complex phenotypes, including abstract conceptual knowledge.

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