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Commentary

The babies, the representations, and the nativist–empiricist bathwater. Commentary on "Stepping Off the pendulum: Why only an action-based approach can transcend the nativist–empiricist debate" by J. Allen & M. Bickhard

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Various versions of the 'nativist–empiricist debate' have been with us at least since John Locke formulated some of the basic principles of empiricist philosophy in the 17th century (Locke, 1690). As Allen and Bickhard suggest in their target article (this issue), empiricist and nativist perspectives have dominated the scientific landscape in pendulum-like alternation across the centuries. Most recently, from the 1970s into the present, theories and data consistent with nativism have assumed positions of prominence in the Developmental Psychology literature (Baillargeon, 1987; Meltzoff & Moore, 1977; Spelke & Kinzler, 2007; Wynn, 1992). However, for more than 50 years, several theorists have been arguing forcefully that nativism in all its variations is inherently non-developmental (Beach, 1955; Blumberg, 2005; Gottlieb, 1981; Johnston, 1987; Moore, 2001). Although there are nativists who disagree with this assessment (Carey, 2009; Spelke & Newport, 1998), the rise of nativism has been accompanied by a growing number of critics.

Among these critics have been those arguing that systems theories such as Dynamic Systems Theory (Thelen & Smith, 1994), Connectionism (Elman et al., 1996), and Developmental Systems Theory (Oyama, Griffiths, & Gray, 2001; Spencer et al., 2009) can help us dispense with the nativist–empiricist debate, and thereby facilitate the study of development. I, too, believe transcendence of the nativist–empiricist debate is a worthy goal (Moore, 2001, 2009), so Allen, Bickhard, and I agree on this point. But even though achieving transcendence is likely to entail utilizing an approach similar in some ways to Allen and Bickhard's, certain aspects of their arguments could nonetheless hinder progress toward our goal.

In their introduction, Allen and Bickhard define emergent-constructivism as "the assumption that representational knowledge can be emergent in the construction of action systems" (Allen & Bickhard, this issue, p. 97). Emergence and construction are concepts that now seem likely to be essential elements of any comprehensive theory of psychological development; certainly it has become clear

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that the biological processes that underlie psychological development involve emergence and construction. In fact, functional genes—once thought to be the basic, atomistic elements responsible for biological development—are now known to be constructed in a developmental process, so genes as we conceived of them through much of the 20th century do not actually exist (Keller, 2000; Moss, 2003; Noble, 2006). Once constructed, genes are best thought of *not* as agents that cause development, but rather as resources that interact and collaborate with other, nongenetic developmental resources in ways that lead to the emergence of complex biological organs like the brain, an organ characterized by structures and functions that are never determined by genetic factors alone (Griffiths & Gray, 1994; Lickliter & Berry, 1990; Moore, 2001; Robert, 2004). Discoveries about the roles of emergence and construction in biology have influenced thinking among infancy researchers so much that the XVIIth annual presidential address to the International Society on Infant Studies was strongly critical of nativistic theories that disregard these findings (Lewkowicz, 2011).

Notwithstanding the ascendance of such ideas among infancy researchers, Allen and Bickhard remain concerned that developmental psychologists—including nativists, but also those who consider themselves to be empiricists or systems theorists—still lack the theoretical resources needed to adequately account for the emergence and construction of *representational knowledge* in the human mind. Because of this focus on representational knowledge, Allen and Bickhard believe transcending the nativist–empiricist debate will require more than adopting a perspective that highlights the roles of emergence and construction in development. Specifically, they argue that this transcendence will require abandoning a central idea in cognitive science, namely that "representation is fundamentally constituted as encodings" (manuscript p. 2). Furthermore, they argue that only an "action-based framework" will be able to explain "the nature, origins and development of our knowledge" (Allen & Bickhard, this issue, p. 96). It is on these latter two points that our opinions diverge.

Allen and Bickhard's conclusions result from their commitment to maintaining a clear distinction between "epistemic contact (detection) and epistemic content (representation)" (Allen & Bickhard, this issue, p. 114). Epistemic contact occurs whenever an organism detects "objects and properties in the world," whereas epistemic content is generated only when epistemic contact generates "*knowledge*. . . of those objects and properties" (Allen & Bickhard, this issue, p. 127, emphasis added). So, for example, a tree detects (i.e., makes epistemic contact with) its environment inasmuch as the climatic conditions in which it is growing are reflected in the width of its growth rings; however, trees do not *know* anything about (i.e., have any epistemic content about) their environments. Similarly, Reznick (2000) offered the example of infants who respond with the same rash to allergens that derive from a variety of categorically related sources, thereby indicating that they have *detected* something similar about the allergens even if they do not actually *know* anything about the category. In contrast, an adult who has allergic reactions to cherries, peaches, and plums might avoid novel fruits like nectarines because she *knows* she has an allergy to drupes, fruits that contain a single hard pit at their core.

Central to Allen & Bickhard's argument is the claim that "standard encoding models of representation" (Allen & Bickhard, this issue, p. 121), intrinsically conflate the ideas of epistemic contact and epistemic content; among infant researchers, such conflation often takes the form of using the single word "representation" to refer both to what occurs when stimuli are detected and to what occurs when knowledge is generated. Remarkably, Allen & Bickhard believe their indictment of such encoding models applies to *any* model that fits within "the information-processing framework of the cognitive revolution" (Allen & Bickhard, this issue, p. 99), whether it is fundamentally empiricist, nativist, connectionist, or interactionist. Consequently, they seem to be critical of virtually all mainstream psychological theories developed over the past five decades, because all of these theories include an important role for encoding processes.

The use of the word "representation" to refer to several different things is a problem that has been noted in the infant development literature. Writing about this issue, Haith (1998) pointed out that infant researchers sometimes use this word "to refer to the coding of information in neural networks" (p. 173), wherein specific patterns of neural activity are present (or are hypothesized to be present) even in the absence of specific, corresponding stimuli. But, Haith notes, these researchers also sometimes write as if" . . . infants can <u>re</u>-present events to themselves by calling them up from memory, to generate a schema or image that they reason about, create expectations and beliefs from, and make inferences about. . .[and in such cases, infant researchers] are talking about . . . something that begins to sound like a *symbolic* representation" (p. 173, emphasis added). So, infant researchers have used "representation" to mean several things, and by considering the implications of this practice, Allen and Bickhard have provided a valuable service, because confusion is bound to arise in the presence of this kind of polysemy (Keller, 2010).

Allen and Bickhard seem to believe "representation" should be reserved for *symbolic* representations, or knowledge. Therefore, one might suppose that their criticism of encoding models of representation would apply only to findings about symbolic encoding, not to findings about *non*-symbolic encoding. (As a possible example of the latter, consider Goldman-Rakic's 1995 finding that spatial locations of no-longer-seen but to-be-remembered stimuli *are* encoded neurologically.) But in response to Haith's suggestion that infant researchers distinguish between sensory encodings and symbolic representations, Allen and Bickhard write "while this distinction is able to illuminate the important difference between detection (sensory encoding) and representational knowledge of what those detections are about (symbolic representation) it does not consider the possibility that encodingism may be equally present in both. That is, that sensory encodings might only differ from images and schemas with respect to the nature and complexity of the presumed encoding-correspondence relationships involved" (manuscript p. 51)[1]. Similar statements in their article suggest that Allen and Bickhard believe incorporating even just the notion of sensory encoding (i.e., detection) into a theory of cognitive development undermines that theory's ability to posit emergence or construction.

It is undoubtedly worth noting that polysemy can sow confusion, but rejecting all modern theories of psychology seems unlikely to be helpful, given how productive these theories have been. Clearly, infant researchers should be careful when using variants of the word "represent," always specifying if they are talking about symbolic representation or, for instance, an encoding of a stimulus in primary visual cortex. But given that brains do detect stimuli, eliminating the idea of encodings from our theories will probably not be constructive. Although Allen and Bickhard recommend such elimination on the grounds that "encoding models inherently conflate . . . epistemic contact [detection] . . . and epistemic content [representation]" (Allen & Bickhard, this issue, p. 127), there is a good reason for this conflation, one that can help us recognize why encoding models are likely to continue to be of use to cognitive developmentalists.

The reason infant researchers often conflate detection and representation can be surmised by reading further into Haith's (1998) criticism of nativist research. There, he notes that there is "substantial neurophysiological evidence that many of the same neurons that are active during short memory-delay periods are the sensory neurons that objects or events activate when present" (p. 173–174). He then cites Ungerleider (1995), who wrote "many studies have found cells whose response to the initial cue is maintained at some level through the delay period. Thus, the memory of the cue appears to endure by maintaining the activity of cells that represent the cue" (p. 174). Here, Ungerleider used the word "represent" in a standard way that Allen and Bickhard would nonetheless object to (i.e., *not* to refer to symbolic representation). But more important is the extent to which Ungerleider's observation undermines the idea that sensory/perceptual processes and mnemonic/cognitive processes can be clearly distinguished. If infant researchers conflate detection and representation, perhaps it is because the distinction between them cannot be as strictly maintained as Allen and Bickhard would have it.

As noted above, Allen and Bickhard's criticism of theories that use the idea of encoding is rooted in their concerns about conflation between detection and (symbolic) representation. But even though our intuitions tell us that knowledge is distinct from detection, it is not necessarily clear how to construe "knowledge" as different from "perception" given that both utilize the same kinds of neurological resources (e.g., Martin, Haxby, Lalonde, Wiggs, & Ungerleider, 1995; Martin, Wiggs, Ungerleider, & Haxby, 1996). In discussing the need to distinguish detection and representation, Allen and Bickhard write that "a thermostat's sensitivity to temperature constitutes an ability to detect differences. . .[but] the thermostat does not have representational knowledge regarding what those detections are about

[1] This quotation comes from the manuscript that the author of this commentary was asked to comment on; it was subsequently deleted from the target article after the commentaries went to press.

– temperature" (Allen & Bickhard, this issue, p. 121). This analysis seems reasonable, of course, but a human brain is quite a bit more complex than a thermostat, and although we do not yet understand how conscious, symbolic knowledge emerges in our brains, the fact remains that the structures and processes involved in detection appear to be the same kinds of structures and processes that are involved in higher-order representation; consider, for example, Penfield's classic studies (1954, 1975) in which neurological stimulation was able to produce the subjective experiences of hearing, remembering, dreaming, and seeing.

Although we all have the subjective impression that we "know" things about the world—that is, that a conscious, epistemic agent in our heads is able to interpret the meaning of the information that reaches it—neuroscientists have overwhelmingly rejected this Cartesian perspective as untenable (see Dennett, 1991). Instead, it now seems plausible that detection—along with accurate and inaccurate recollection of what was previously detected—might produce non-symbolic "representations" that are responsible for later-emerging, truly symbolic representations. Perhaps Allen and Bickhard need not be as concerned as they are about conflating detection and representation, because symbolic representations in human minds might result from aggregations of sensory encodings in human brains. Just as there are properties of water that cannot be detected in a single molecule of H_2O—properties that *emerge* when enough molecules of H_2O are in proximity to one another—it is not unreasonable to think that qualitatively novel properties of mind (e.g., symbolic, *meaningful* representations) could emerge from mere 'detection' processes operating in our brains.

This possibility significantly weakens Allen and Bickhard's criticisms of Dynamic Systems Theory (DST), Connectionism, and Dynamic Field Theory (DFT). DST can tolerate the possibility that knowledgeable systems like ours might develop merely as a consequence of our brains representing the world the way tree rings do, that is, without any built-in mechanisms for interpretation by an epistemic agent. Likewise, notwithstanding Allen and Bickhard's protestation that "the meaningfulness of any ["representation" generated by a connectionist network] . . . depends entirely on an external observer" (Allen & Bickhard, this issue, p. 124), the meaningfulness of any "representation" generated *by a person's brain* might not depend on the interpretations of an epistemic agent, either inside or outside of the person's head. Instead, perhaps conceptual representations emerge when perceptions are "represented" in a sufficiently complex nervous system merely as a result of epistemic contact. If so, then Allen and Bickhard's criticisms of DFT can also be dismissed, by arguing that the DFT model of representation does *not* require the cognizant "interpreter" (Allen & Bickhard, this issue, p. 125) they think it does. Although they state that the "encodingism [characteristic of DFT, Connectionism, and any other information-processing model of cognitive development] precludes the possibility of emergent representation" (Allen & Bickhard, this issue, p. 120), it was not clear from their arguments in this paper why this must be the case. Nor was it clear why they believe all encoding conceptions of representation necessarily induce "oscillations between nativism and empiricism" (Allen & Bickhard, this issue, p. 129).

In fact, some of our symbolic representations might best be thought of as epiphenomena. Allen and Bickhard distinguish between a genuinely "informational" relationship "in the semantic or representational sense" (Allen & Bickhard, this issue, p. 128), and a "technical—covariational—informational relationship" (Allen & Bickhard, this issue, p. 120) in which a system's sensitivity to the environment results merely in covariation between the environment and the system. Consistent with this conceptualization, a baby's brain might, through the simultaneous firing of a specific collection of neurons, establish a "technical informational relationship" with a dog she sees, but Allen and Bickhard would not consider this to mean the baby has generated a symbolic representation of the dog. But even if this specific neural activity does not constitute a symbolic representation of the dog for the baby, the "technical" relationship provides information about the dog, in which case the symbolic representation might not be as important as Allen and Bickhard seem to think it is. If detection—along with recollection and manipulation of that which has been detected—can provide information required to survive and reproduce in this world, it is not necessarily clear what the importance of "representational knowledge" might be. One bit of evidence relevant to this suggestion is the finding that Schöner and Thelen (2006), using a dynamic field model of habituation, were able to provide a coherent explanation of all of the extant data related to Baillargeon's (1987) occluded object paradigm, without needing to presume that infants have any representational knowledge of objects at all.

Of course, the development of imagination and some other distinctively human cognitive processes might still require additional explanation (but see Martin et al., 1996, for neurological evidence from a task requiring "imagined" behavior). However, the preponderance of human psychological processes—for example, all of those we share with other mammals, including "error guided behavior and learning" (Allen & Bickhard, this issue, p. 126)—probably do not require the kind of "representational knowledge" Allen and Bickhard fight so hard to save. A *complete* understanding of human cognition requires a theory that explains how people can sometimes understand the conceptual basis of an abstract category, but a science of cognitive development can still make progress without first resolving such issues.

If perception and cognition are not as distinct as Allen and Bickhard seem to believe, then the idea of encoding might be unproblematic, and theories of cognitive development can posit emergence and construction even if they are not action-based. Although there are many empirical reasons to believe action contributes to cognitive development (Campos et al., 2000; Sommerville, Woodward, & Needham, 2005; Soska, Adolph, & Johnson, 2010), it seems unnecessarily restrictive to maintain that "*only* an action based approach" (Allen & Bickhard, this issue, p. 96, emphasis added) will provide a workable theory of cognitive development, or that "action is essential and intrinsic to the ontology of representation itself" (Allen & Bickhard, this issue, p. 124). After all, if, as Allen and Bickhard assert, "knowledge just *is* competent (inter)action with the environment" (Allen & Bickhard, this issue, p. 127, emphasis in original), one implication would be that damage to the human spinal cord at the second cervical vertebra—and its consequent tetraplegia—must necessarily entail an immediate loss of "knowledge," or at the very least, the loss of the ability to generate new knowledge, a proposition that seems *prima facie* to be false. While Allen and Bickhard are probably right that a truly passive mind cannot come to know the world, this does not necessarily require the kind of *action* (i.e., motor activity) they insist on. Instead, approaches like DST or Connectionism seem equipped to account for the emergence and construction of representational knowledge even if some of that knowledge arises from interactions between the world and non-action-based processes like perception (as in Martin et al., 1995). Such approaches need not posit the existence of either conceptual or perceptual "innate representational features" (Allen & Bickhard, this issue, p. 116).

For reasons detailed in Allen and Bickhard's comprehensive critique—including, importantly, the "lack of perceptual level controls" (Allen & Bickhard, this issue, p. 117) used in typical nativist infant research (see also Clearfield & Mix, 1999, 2001; Cohen & Marks, 2002; Moore & Cocas, 2006)—the findings and arguments of nativists like Baillargeon (1987), Wynn (1992), Spelke and Kinzler (2007), and Carey (2009) cannot support claims that young infants have full-blown representational knowledge like that which cognitive scientists sometimes assume exists in adults. Allen and Bickhard's criticism is particularly appropriate given the importance of recognizing that all psychological characteristics arise in development. The primary effect of notions like "innate representational primitives" (Carey, 2009, p. 29) or "core knowledge" (Spelke & Kinzler, 2007, p. 89) is to "short-circuit . . . investigation of . . . developmental relationships" (Lehrman, 1953, p. 359); clearly, any approach that relies on the existence of featural or conceptual representations that are foundational—that is, present prior to development—will fail to explain how such representations *develop*, so such "foundationalism," as Allen and Bickhard emphasize, has no place in truly developmental theories of cognition. Nevertheless, it seems unnecessary at present to dispense with all psychological theories that posit a role for encoding processes, or to restrict ourselves in the future to using only action-based approaches in elaborating our theories. Transcending the nativist–empiricist debate is a critical step in bringing a developmental perspective to cognitive psychology, but transcendence is within reach given the tools provided by DST, Connectionism, and other interactionist approaches to the emergence of knowledge.

References

Baillargeon, R. (1987). Object permanence in 3.5 and 4.5-month-old infants. *Developmental Psychology*, *23*, 655–664.
Beach, F. A. (1955). The descent of instinct. *The Psychological Review*, *62*, 401–410.
Blumberg, M. S. (2005). *Basic instinct: The genesis of behavior*. New York: Thunder's Mouth Press.
Campos, J. J., Anderson, D. I., Barbu-Roth, M. A., Hubbard, E. M., Hertenstein, M. J., & Witherington, D. (2000). Travel broadens the mind. *Infancy*, *1*, 149–219.
Carey, S. (2009). *The origin of concepts*. New York: Oxford University Press.

Clearfield, M. W., & Mix, K. S. (1999). Number versus contour length in infants' discrimination of small visual sets. *Psychological Science*, *10*, 408–411.

Clearfield, M. W., & Mix, K. S. (2001). Amount versus number: Infants' use of area and contour length to discriminate small sets. *Journal of Cognition and Development*, *2*, 243–260.

Cohen, L. B., & Marks, K. S. (2002). How infants process addition and subtraction events. *Developmental Science*, *5*, 186–201.

Dennett, D. C. (1991). *Consciousness explained*. Boston, MA: Little, Brown.

Elman, J. L., Bates, E. A., Johnson, M. H., Karmiloff-Smith, A., Parisi, D., & Plunkett, K. (1996). *Rethinking innateness*. Cambridge, MA: MIT Press.

Goldman-Rakic, P. S. (1995). Cellular basis of working memory. *Neuron*, *14*, 477–485.

Gottlieb, G. (1981). Roles of early experience in species-specific perceptual development. In R. N. Aslin, J. R. Alberts, & M. R. Petersen (Eds.), *Development of perception: Psychobiological perspectives. Audition, somatic perception, and the chemical senses* (pp. 5–44). New York: Academic Press.

Griffiths, P. E., & Gray, R. D. (1994). Developmental systems and evolutionary explanation. *The Journal of Philosophy*, *XCI*, 277–304.

Haith, M. M. (1998). Who put the cog in infant cognition? Is rich interpretation too costly? *Infant Behavior & Development*, *21*, 167–179.

Johnston, T. D. (1987). The persistence of dichotomies in the study of behavioral development. *Developmental Review*, *7*, 149–182.

Keller, E. F. (2000). *The century of the gene*. Cambridge, MA: Harvard University Press.

Keller, E. F. (2010). *The mirage of a space between nature and nurture*. Durham, NC: Duke University Press Books.

Lehrman, D. S. (1953). A critique of Konrad Lorenz's theory of instinctive behavior. *Quarterly Review of Biology*, *28*, 337–363.

Lewkowicz, D. J. (2011). The biological implausibility of the nature–nurture dichotomy and what it means for the study of infancy. *Infancy*, *16*, 331–367.

Lickliter, R., & Berry, T. D. (1990). The phylogeny fallacy: Developmental psychology's misapplication of evolutionary theory. *Developmental Review*, *10*, 348–364.

Locke, J. (1690). *An essay concerning human understanding*. Oxford: Clarendon Press.

Martin, A., Haxby, J. V., Lalonde, F. M., Wiggs, C. L., & Ungerleider, L. G. (1995). Discrete cortical regions associated with knowledge of color and knowledge of action. *Science*, *270*, 102–105.

Martin, A., Wiggs, C. L., Ungerleider, L. G., & Haxby, J. V. (1996). Neural correlates of category-specific knowledge. *Nature*, *379*, 649–652.

Meltzoff, A. N., & Moore, M. K. (1977). Imitation of facial and manual gestures by human neonates. *Science*, *198*, 75–78.

Moore, D. S. (2001). *The dependent gene: The fallacy of nature vs. nurture*. New York: Henry Holt.

Moore, D. S. (2009). Probing predispositions: The pragmatism of a process perspective. *Child Development Perspectives*, *3*, 91–93.

Moore, D. S., & Cocas, L. A. (2006). Perception precedes computation: Can familiarity preferences explain apparent calculation by human babies? *Developmental Psychology*, *42*, 666–678.

Moss, L. (2003). *What genes can't do*. Cambridge, MA: The MIT Press.

Noble, D. (2006). *The music of life*. Oxford: Oxford University Press.

Oyama, S., Griffiths, P. E., & Gray, R. D. (2001). *Cycles of contingency: Developmental systems and evolution*. Cambridge, MA: MIT Press.

Penfield, W. (1975). *The mystery of the mind: A critical study of consciousness and the human brain*. Princeton, NJ: Princeton University Press.

Penfield, W., & Jasper, H. (1954). *Epilepsy and the functional anatomy of the human brain*. Boston: Little, Brown.

Reznick, J. S. (2000). Interpreting infant conceptual categorization. *Journal of Cognition and Development*, *1*, 63–66.

Robert, J. S. (2004). *Embryology, epigenesis and evolution: Taking development seriously*. New York: Cambridge University Press.

Schöner, G., & Thelen, E. (2006). Using dynamic field theory to rethink infant habituation. *Psychological Review*, *113*, 273–299.

Sommerville, J. A., Woodward, A. L., & Needham, A. (2005). Action experience alters 3-month-old infants' perception of others' actions. *Cognition*, *96*, B1–B11.

Soska, K. C., Adolph, K. A., & Johnson, S. P. (2010). Systems in development: Motor skill acquisition facilitates three-dimensional object completion. *Developmental Psychology*, *46*, 129–138.

Spelke, E. S., & Kinzler, K. D. (2007). Core knowledge. *Developmental Science*, *10*, 89–96.

Spelke, E. S., & Newport, E. L. (1998). Nativism, empiricism and the development of knowledge. In W. Damon, & R. M. Lerner (Eds.), *Handbook of child psychology. Theoretical models of human development* (pp. 275–340). New York: Wiley.

Spencer, J. P., Blumberg, M. S., McMurray, B., Robinson, S. R., Samuelson, L. K., & Tomblin, J. B. (2009). Short arms and talking eggs: Why we should no longer abide the nativist–empiricist debate. *Child Development Perspectives*, *3*, 79–87.

Thelen, E., & Smith, L. B. (1994). *A dynamic systems approach to the development of cognition and action*. Cambridge, MA: MIT Press.

Ungerleider, L. F. (1995). Functional brain imaging studies of cortical mechanisms for memory. *Science*, *270*, 769–775.

Wynn, K. (1992). Addition and subtraction by human infants. *Nature*, *358*, 749–750.