COMMENTARIES

Innateness, learning and the development of object representation

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This is a curious debate. On one side, Baillargeon argues that theories couched in the terms of the nativist–empiricist dialogue are wellsprings of empirical hypotheses and findings. She illustrates this point by describing a series of elegant experiments investigating infants' developing knowledge about the relationships between objects and their supports, containers or occluders. In a particularly exciting turn, she reports new object-learning experiments in which she both observes and manipulates the growth of infants' knowledge of these relationships. On the other side, Smith argues that theories couched in the terms of the nativist–empiricist dialogue are dead ends. She illustrates this point by describing a series of elegant experiments investigating young children's developing knowledge about the relationships between objects and the words and expressions people use to describe them. In a particularly exciting turn, she reports new word-learning experiments in which she both observes and manipulates the growth of children's knowledge of these relationships. Why do such closely parallel research programs lead their authors to such apparently different conclusions?

I see three sources to this debate. First, Baillargeon and Smith disagree over the terms of the nativist–empiricist dialogue, and particularly over the nature of nativist claims. Second, they disagree about the role science can play in illuminating human knowledge. Finally, they disagree about the questions that studies of cognitive development should ask and the questions that should be postponed. I consider each of these differences in turn.

Baillargeon and Smith give different meanings to the term 'innate'. For Baillargeon, as for generations of contributors to the nativist–empiricist dialogue, 'innate' means *not learned*. For much of Smith's paper, in contrast, 'innate' means *preformed*. The vast difference between these meanings can be seen through Smith's chosen example of fingers and toes: body parts are not preformed in the fertilized egg, but neither are they learned by the developing child as she observes other people's body parts or encounters situations that require them. In the tradition of the nativist–empiricist dialogue, body parts pass the two central tests for innateness: their structure emerges in advance of their function, and their structure is not subsequently shaped by the organism's encounters with the external objects and situations to which it applies. When Smith claims that knowledge develops as do fingers and toes, therefore, the most radical nativist would not disagree.

Although earlier contributors to the nativist–empiricist dialogue were careful to maintain the distinction between nativism and preformationism, a number of recent discussions have tended to conflate these claims (e.g. Thelen & Smith, 1993; Elman *et al*., 1996[1]; Quartz & Sejnowski, 1997; see Spelke & Newport, 1998, for discussion). Because nativists are not preformationists, this conflation produces arguments directed against no real adversary, and it sidesteps the core questions at issue concerning the origins and development of knowledge: To what degree is our knowledge shaped by encounters with the things known? How variable is the knowledge of people who live in different circumstances? To what degree can we influence the conceptions of our children, by changing the environments in which they grow? The research described by Baillargeon and by Smith testifies to the continued fruitfulness of these questions, and to the vitality of the centuries-old dialogue within which the questions have been articulated and refined.

[1] Elman *et al.* (1996) carefully distinguish these different senses of innateness in their initial discussion. They conflate them in their subsequent arguments, however, by taking evidence against preformationism as evidence for learning.

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The second difference between Baillargeon and Smith concerns the role of science in addressing questions about the origins and growth of knowledge. For Smith, the question whether or not a given aspect of knowledge is innate is not open to empirical study. Whatever the evidence turns out to be, 'The concept of an innate knowledge structure is just not alignable with ... the goal of a mechanistic understanding of cognitive development.' Evidence is not relevant because a belief in innate knowledge, like a belief in angels, lies outside the domain of science.

This is an odd position for a scientist to hold, even a twentieth-century psychologist engaged in a debate with eighteenth-century biologists. The history of embryology teaches us that preformationism is false, not that it is heresy. Preformationism was abandoned because investigations of the initial state and early development of organisms provided evidence against it. To collect this evidence, biologists had to be open to the possibility that preformationism might be true. If they had decided in advance that the concept of a preformed organism in the egg was 'just not alignable with the goal' of embryology, there would have been no reason to conduct any experiments.

Smith's ideological rejection of claims for innateness is illustrated by her discussion of two research programs and two principles invoked to explain their findings: her own studies of the development of the shape bias in word learning, whose findings may be explained in part by the empiricists' innate principle of temporal contiguity, and my studies of the development of object perception, whose findings may be explained in part by an innate principle of spatio-temporal continuity. Both research programs are fundamentally empirical: they aim to describe, and ultimately to explain, the development of mature knowledge. Each of the principles of contiguity and continuity, moreover, was formulated as an empirical hypothesis about possible initial structures in the mind. Based on current evidence, each principle now has some supporters and some detractors (for a skeptical discussion of the contiguity principle, see Gallistel, 1990).

Smith, however, cites no evidence for or against either the contiguity or the continuity principle. She simply concludes, without argument, that the former is good and the latter is bad: 'There are cranes and skyhooks in developmental theory ... and ... only cranes count as non-question-begging *honest science*.' Smith's explanation for children's focus on shape in word learning is a crane, she argues, because the principle of contiguity is grounded in 'processes *known to exist* and known to be capable of producing the developmental outcome'. My explanation for infants' focus on objects in perceptual parsing is a skyhook, because the principle of continuity is one of a set of '*hypothetical contrivances*, unexplained and ungrounded in any known process or mechanism' (p. 139, emphasis mine). Note that Smith does not argue that the principle of contiguity is supported by evidence, but rather that it is *known* to be true. And she does not argue that the principle of continuity should be rejected because there is evidence against it, but because it is not *honest science*. Finally, she denigrates the central currency of scientific theorizing by characterizing empirical hypotheses about the origins of knowledge as *hypothetical contrivances*. These are arguments of religious faith, of certain truth and heresy, not of scientific theories and evidence.

Baillargeon relates theories to evidence in a more productive way, but her view also bears the imprint of an ideology that favors empiricist principles. Although she states that claims that a given aspect of knowledge is innate or learned should be decided by evidence, her use of evidence bears on these claims in a skewed manner. For Baillargeon, empiricism provides the default hypotheses, and claims of innateness should be considered only as a last resort.

Baillargeon's position is illustrated by her discussion of the continuity principle. She asks what evidence 'would *compel* us as researchers to *consider the possibility*' that the continuity principle is innate (p. 121, emphasis mine). She answers that her own approach 'is to try to uncover *how* infants learn about physical events. Finding out how infants learn, we believe, should also tell us what they *can* and *cannot* learn. Consider, for example, 2.5-month-old infants' expectation that a moving object continues to exist and pursues a continuous path .... If it turns out that infants' learning mechanisms can readily acquire such an expectation, then we will know that it is learned .... On the other hand, if it turns out that such an expectation is something that infants' learning mechanisms are ill-equipped to learn, then we will be compelled to take seriously ... [the] proposal that it reflects the presence of an innate belief in continuity' (p. 121, emphasis hers).

This position, however, has two problems. First, it suffers from a logical flaw. Showing that something *could be* learned is not equivalent to showing that it *is* learned. Perhaps infants' learning mechanisms are powerful enough in principle to enable a creature to learn to grasp objects placed in its palm, for example, but in fact the 'grasping reflex' is not learned in this way, for it can be elicited in infants who have never previously encountered a graspable object. Second, Baillargeon's position sets a biased standard for research on the origins of knowledge by treating claims for development by learning as true unless proven false. Because science is not an exercise in logic or law, scientific experiments can only result in the accumulation of evidence that favors one hypothesis over another. The science of cognitive development will advance to the extent that its investigators view their field as an empirical enterprise and evaluate rival hypotheses in relation to evidence. If nativist hypotheses are deemed unworthy of consideration unless one is *compelled* to consider them, then large classes of viable, testable developmental theories are unlikely ever to be considered. It is symptomatic of the empiricist bias in our field that neither contributor to this debate shows the openness that empirical studies of cognitive development require (see Spelke, 1998, for more discussion).

The third difference between Smith and Baillargeon concerns the questions they leave unasked. Smith focuses on how children learn that a particular class of perceptible properties – those related to *shape* – guides the application of a particular class of words – count nouns. Behind her research, however, are critical further questions. For example, *shapes* are properties of *things*, and so we must ask what the things are whose shapes children analyze, and why children single out those things and not others. As the Gestalt psychologists pointed out, people see the shapes of some parts of a visual scene (particularly the shapes of objects) but not other parts of the scene (e.g. the shapes of the spaces between objects) (see Koffka, 1935, p. 208). Why, Koffka asked, do we see things and not the holes between them? In her analysis of children's word learning, Smith presumes that children also see things and not the holes between them, but nothing in her discussion suggests what cognitive mechanisms lead them to do so.

Baillargeon's research focuses squarely on the mechanisms by which developing children represent objects and physical constraints on their behavior. She proposes that infants first learn basic categories of relationships among objects such as contact, support and occlusion, and then learn the fine details of these relationships. This proposal, however, raises a critical question. As Wertheimer (1923/1958) noted, objects bear infinitely many relationships to one another, but people apprehend only some of them. What leads infants to focus on the relationships that Baillargeon studies? Consider, for example, the contact relation. Infants appear to differentiate events in which one object contacts another from events in which the objects narrowly miss one another, but they do not appear to differentiate between events in which objects miss each other narrowly versus widely. What accounts for this difference? Baillargeon may reply that contact versus no contact is a qualitative distinction whereas narrow versus wide separation is a quantitative distinction, and infants learn qualitative distinctions first. This reply, however, raises Wertheimer's question again: What cognitive mechanisms lead us to see states of contact versus no contact as qualitatively different, and states of narrow versus wide separation as qualitatively the same?

These issues are not idle musings but concrete, empirical questions concerning the mechanisms by which developing humans represent objects. The questions need to be addressed by programs of research at multiple levels within brain and cognitive science. In Mandler's (1992) apt phrase, developmental scientists cannot hope to build a baby without answering them.

I have taken issue with both contributors to this debate, but I am glad the debate is happening. It has been fashionable for some time to declare that the nativist–empiricist dialogue is obsolete and to recommend that contemporary developmental scientists move beyond 'arguments with dead people' and develop new theories (Siegler, 1993, p. 339). The chief result of these proclamations, in my view, has been that old positions have been reinvented and old arguments against them have been forgotten. It is discouraging, for example, to see a new generation of associationists devise theories as vulnerable to the Gestalt psychologists' critiques as earlier associationist theories were, or to see contemporary writers conflate distinctions that earlier thinkers had painstakingly sorted out. Nevertheless, the research by Smith and Baillargeon shows clearly that the real nativist–empiricist dialogue, concerning the nature and sources of human knowledge, is alive. If we allow ourselves to learn from its earlier contributors – from the humanists that Smith dismisses, from the scientists that she champions, and especially from the many thinkers who were both – we may advance the discussions that our ancestors began through the exciting research programs that Baillargeon, Smith and others have crafted.

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Nativism versus development: comments on Baillargeon and Smith

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Most of us working in this field have already taken a stand on the nature–nurture debate; roadside conversions on the way to Damascus are not as common as they used to be. Hence the point of this collective exercise must be to convince the next generation of students whose minds are still open regarding the oldest question in developmental psychology. My own (prejudged) opinions are offered in that light.

Baillargeon makes what sounds like a reasonable case for a middle ground between radical nativism and a *tabula rasa* view, but some very strong nativist claims are slipped in by presupposition.

First, Baillargeon contrasts her own form of nativism with a straw man, a 'no-development' variant that has no serious proponents. All organs emerge from a cluster of undifferentiated cells, and all cognitive abilities develop; no one has ever suggested otherwise. The debate is not about the presence or absence of development; it is about the role of learning versus maturation in the developmental process. If an ability is present at birth, we may conclude that learning has played a limited role in its establishment (although, as Smith points out, such abilities may be the product of multiple interacting causes, including the child's own activity and experience *in utero*). However, most of the results that Baillargeon reports are based on studies of children who are at least 3 months of age. Such children have had 90 days, approximately 900 waking hours and 54 000 minutes, of visual and auditory experience. It is now well established that truly stupid neural networks with 40 neurons can learn a great deal in 54 000 trials. Imagine what a bigger brain could do.

Second, Baillargeon points out the need for a learning theory to explain those aspects of development in which learning plays a role. However, she proposes (based on two or three studies in her laboratory) an idiosyncratic theory in which learning can only take place with exposure to contrasting events (referred to in the computational literature as 'negative evidence'). If such contrasting information is not available, then (by stipulation) learning cannot account for the result. This is empirically wrong, because there are ample demonstrations of rapid learning by infants exposed only to positive evidence (e.g. Haith's results on anticipatory learning by 3-month-olds (Haith, 1990); Saffran *et al.* on rapid auditory learning by 8-month-olds (Saffran, Aslin & Newport, 1996)). It is also logically wrong, because any organism capable of prediction ('What's coming next?') can generate its own negative evidence ('That wasn't what I expected!'). In fact, there are many reasons to believe that learning is bigger, faster, stronger and earlier than Baillargeon appreciates, in neural networks and real babies.

Finally, Baillargeon provides evidence based on a simple discriminant response (looking more at one event than another), but her terminology invites us to infer

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higher-order cognition when lower-level perceptual learning could account for much of the data. In her interpretations of preferential looking by 3-month-old infants, she uses the verbs 'believe', 'realize', 'expect', 'surprise', 'appreciate', 'reason' and 'recognize'. She rejects the idea that perceptual biases could account for her results, on grounds of parsimony (one would have to assume more perceptual biases than the number of prior structures proposed in nativist theories), but she does not appreciate (recognize, realize or believe) that perceptual learning of a relatively simple sort could account for many of these findings, instead of 'initial concepts' or other rich forms of knowledge. Because so many strong claims have slipped in by assumption and terminology, the reader is sweetly lulled into accepting what is, still, a radical form of nativism.

I am more persuaded by Smith's arguments for a dynamic, epigenetic approach to cognitive development, but I feel compelled to point out one important difference between her approach and the one my colleagues and I have espoused elsewhere (Elman *et al.*, 1996). We agree with Smith about the power of learning and the interactive nature of all development (fingers, toes and cognition). We are also persuaded by mounting evidence on cortical plasticity, demonstrating that synaptic connectivity (the neural basis of knowledge) is the product of input to the cortex (from the baby's own active body and from the world). Hence we are not 'representational nativists'. However, there is also ample evidence for endogenous, experience-independent constraints on neural architecture (e.g. the primary visual cortex in primates has twice as many neurons as any other area, a difference that is already established in the proliferative zone, during neurogenesis). There is also evidence for experience-independent variations in the timing of maturational events. As Elman *et al.* have shown, such variations in timing can determine the success or failure of learning in complex domains of grammar and logic. Quantitative variations in architecture and timing may provide the key to differences within and across species in learning, processing and motivation. One can therefore embrace a certain kind of nativist agenda without believing in innate concepts, innate knowledge, innate representations of any kind.

Having said that, I hasten to agree with Linda Smith that we would all be better off without words like 'instinct' or 'innate'. Indeed, my colleagues and I argued at great length before we settled on the title 'Rethinking innateness' for our recent book (Elman *et al.*). Our goal is the same one that Smith recommends. Let's just figure out how things work, how fingers, toes and ideas develop in the human child. We agree that the answer lies in 'interaction all the way down'. However, until a clear and explicit theory of interaction is available, history shows us that this debate simply will not go away. The nascent form of such an interactive theory can now be discerned, based on new ideas from dynamical systems, developmental neurobiology, molecular genetics, embryology and (we believe) studies of learning and representation in neural networks. Perhaps the argument will evaporate when a consensus is reached within our field regarding this new synthetic theory of development.

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Into the middle of things: from dichotomies to grounded dynamic analysis of development

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As a field, can we move beyond the entrenched positions that have pervaded the nature–nurture debate and seek out instead a fruitful middle ground that combines the arguments of Baillargeon and Smith to produce a textured account of the dynamics of development? To explain her findings, Baillargeon needs the kind of rich dynamic theoretical account that Smith prescribes; and to ground her theoretical account, Smith needs the kind of rich developmental description that Baillargeon begins to provide. The two dichotomized positions need each other to move into the middle of things, capturing development of real people acting in a complex world.

Unfortunately the debate between nativist and empiricist accounts of development remains dichotomized, as debaters state extreme positions in order to bring into focus their presumed differences. Important advances in developmental science in recent years remain unheeded in the stridency of such debates. If recent research and theory could open up the interpretive limitations that Baillargeon and Smith have backed themselves into, their otherwise solid work would naturally converge on a richer theoretical account of developmental processes than either can manage from her own less textured, dichotomized position.

We will sketch how to take the two authors' work, add to it research that deepens the understanding and significance of the work, and end with a synthesis that shows what developmental studies could be. For developmental science to move toward such synthesis, the enterprise needs to become more collective, going beyond the traps of partisanship that so typify the nativist–empiricist debate.

Baillargeon and her colleagues have done exceptionally good experimental work, providing a rich description of pathways of development of object-related skills that go far beyond the implications of her nativist theoretical framework. Her interpretation of the findings, however, tends to beg the ultimate question of how children develop activities with objects. Calling some skill or behavior innate tends to stop analysis of how it develops. Using mental state terms to describe it (the infant 'believed' or 'realized' or 'expected' something or 'showed a concept') obscures the need for an explanation at a different level from the behavior as well as a description of developmental pathways in which the behavior is embedded. We are reminded of Clark and Thornton's (1997) encapsulation of the problem as 'trading the problematic chicken for the unexplained egg' (p. 85).

Instead of focusing on her framework we will take one of Baillargeon's experiments and outline an interpretation that goes beyond claims of innateness and suggests particular developmental pathways and specific mechanisms for change. These mechanisms begin the kind of dynamic analysis of development that Smith calls for. Consider the task with an inclined ramp and a toy truck passing behind a screen, with a block placed either near the truck's track (possible event) or on the track (impossible event). The finding was that 8- and 6.5-month-old infants, and 4-month-old female infants, looked reliably longer at the impossible event.

What kind of mechanism can account for the observation that an infant looks longer when the truck appears to act in a way that trucks do not act. Certainly Smith's vague metaphor about fingers and toes will not help, but a more substantive dynamic analysis by other investigators illustrates one way to start a fruitful explanation. Mareschal, Plunkett and Harris (1995) propose an explicit connectionist model that combines a 'where' mechanism (a neural network for visual trajectory prediction) with a 'what' mechanism (a network for object identification) (see also the discussion in McLeod, Plunkett & Rolls, 1998). The mathematical model produced a developmental pathway with early success in prediction of position and later success in search through reaching. With a lower-level mechanism it thus explained one way that prediction of the truck's pathway in Baillargeon's task would precede search for a hidden object in Piaget's (1937/1954) task, which requires a combination of where and what mechanisms. This analysis fits well with the developmental changes that Baillargeon herself describes,

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and it provides a framework that supports and begins to explain those rich pathway descriptions.

Smith's analysis of the dynamic nature of development suggests such an integration of Baillargeon's developmental descriptions with change mechanisms like the what/where model. The two major themes of Smith's theoretical work provide fitting touchstones for developmental explanation (Thelen & Smith, 1998):

1. Development involves many levels interacting mutually and continuously, varying from molecules to culture.
2. Development arises from 'nested processes' that vary from fast time-frames in milliseconds to slow time-frames in years.

Of course, no one study can deal with all levels and processes simultaneously, but explaining even a couple of steps in a developmental pathway requires considering a few levels and processes simultaneously. The dynamic combination of components is what produces the changes of development, although Smith's vague metaphoric analysis in this journal does not do justice to her own dynamic framework.

To understand why a young infant looks longer at an impossible event and then at a later age searches effectively for a disappearing object, researchers need to go beyond nature–nurture and beyond vague systems metaphors to do justice to the complexity of activities and the interpenetration of levels of analysis that development embodies. Fischer and Bidell (1991) describe how relating the contributions (and constraints) from genes, environments and the person's own activity can capture the epigenetic landscape for developing skills. Development is not limited to any single factor alone, but involves a network of potential pathways arising from multiple influences that combine dynamically (Fischer & Bidell, 1998; van Geert, 1998). Effective science requires describing developmental pathways in detail, as Baillargeon has effectively done, and capturing how multiple factors shape those pathways, as Smith argues.

The what/where connectionist model is only one starting point for using a dynamic framework to explain observations of developing object-related skills and pathways. For example, a key step is to analyze how an action scheme might actually be instantiated in the visual–motor system. In recent years, the level of modeling the visual system, combining neural networks with highly detailed neurophysiology, has become sophisticated (e.g. Chabris & Kosslyn, 1998; Grossberg, in press). Glossing cognitive-neural mechanisms in terms of 'what' and 'where' pathways provides a good start, but ultimately deeper componential analyses are needed.

Some early developmental biologists believed that the human embryo begins as a tiny, perfectly formed human being and that development involves an increase in size. Developmental science has shown how little of the form and function of human anatomy is evident in the fertilized egg. In behavior likewise, developmentalists must be cautious about looking for the impressive complexity of human skills preformed in either the infant or the world. The catch-all categories of innate and learned are simply too limited to capture the rich complexity and detail of developing behavior that are now evident from many quarters of research and theory. Why should we continue to debate over concepts that, clearly, the field has developed beyond?

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Levels of learning

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At first sight there seems to be a (perhaps surprising) common ground between Baillargeon and Smith. They both indicate that knowing the mechanisms of learning can shed light on the issue of innateness.

According to Baillargeon, if infants' learning mechanisms are ill-equipped to produce expectations about the continuity of objects, then experimental indications that infants do have these expectations may constitute evidence for there being an innate belief in continuity (pp. 125–126). In her description of the infants' learning mechanism she points to the fact that it needs contrastive evidence in order to function properly. Since evidence of non-continuous objects cannot be found in our world, it is reasonable to assume an innate knowledge structure regarding the continuity of objects.

According to Smith, if one looks closely at object–name generalization in young children, it seems plausible to attribute to them knowledge about how objects are named (i.e. the shape bias). However, if one examines the mechanism that creates the shape bias in young children, one finds that there is no need to invoke innate knowledge structures or beliefs. Basically, all that is necessary is the control of selective attention to shape via associative learning, with count noun syntax as (one of) the discriminative stimuli that increase the infants' attention to shape (p. 136).

On closer examination, it turns out that the appearance of a common ground is deceptive: although both Baillargeon and Smith agree that it is valuable to investigate the learning mechanism, they differ radically with respect to the *level* at which they describe this mechanism. Baillargeon describes the infants' learning mechanism at a formal level; infants acquire concepts when they are exposed to contrastive evidence. In other words, Baillargeon describes the learning mechanism in terms of its *logical structure*, i.e. by specifying it as a process of reasoning and by indicating a necessary condition (contrastive evidence) that the mechanism needs in order to produce learning.

Smith describes the infants' learning mechanism at a lower, causal instead of formal, level. She describes it in terms of *associative processes* that, in combination with certain characteristics of the environment (i.e. the statistical structure of early noun vocabularies), result in learning.

The question that needs to be answered, then, is whether the particular choices for the preferred level of analysis can be justified. On this issue, I think Baillargeon faces a more difficult task than Smith.

Smith's preference for associative processes as the essential elements of the mechanisms of learning is unobjectionable, in my view, precisely because these processes *are* 'ordinary and mundane', as she (p. 136) puts it. Association is a very basic phenomenon, well understood both psychologically and neurophysiologically, and thus provides a solid explanatory foundation. This is not to say, of course, that the choice for this level is sufficient by itself to decide the debate. On the contrary, much empirical work needs to be done in order to fully understand the details of the learning mechanisms studied. Yet the level at which Smith wants to investigate these details is a basic and therefore justified one.

Baillargeon's choice for a higher, more formal, level of description of the learning mechanism lacks, in my view, an equally 'rock-solid' foundation. Taking a formal perspective of learning as a process of reasoning on the basis of evidence is surely part of a long and revered tradition in cognitive science. But although such a description is often possible, it is entirely unclear, or at least in need of substantial *further* justification, whether such a description succeeds in catching the contours of the actually operating causal mechanisms that underlie development. One of the main reasons for debates such as the current one, I think, is a growing doubt that the concepts used at the formal level of description (e.g. belief, knowledge, reasoning, contrastive evidence) genuinely map onto the actual processes occurring in the learning infant. Indeed, such a doubt is evident even in Baillargeon's

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own writing. Although she is willing to claim that 3.5-month-old infants are able to reason about the height of an object relative to that of an occluder (p. 118) and to defend the existence of an innate notion (belief, knowledge structure) of continuity, she does indicate some discomfort with this terminology. As she says: 'let me emphasize again that this notion cannot be tantamount to a full-fledged understanding of continuity (...) continuity provides infants with no more than a scaffold – albeit a very important scaffold – to guide their knowledge acquisition' (p. 126). Likewise, I venture, one could emphasize that a 3.5-month-old infant does not engage in 'full-fledged reasoning' either.

But then why *speak* of beliefs, reasoning and the need for contrastive evidence? And why claim that such high level concepts are necessary to provide a proper understanding of the basic mechanisms of infant learning? I think the main temptation to do so stems from *starting* with a conception of 'mechanism' in terms of its logical structure and function. Similarly, it is the acceptance of a formal level of analysis that makes the suggestions about innate knowledge structures that aid reasoning seem fairly straightforward; these structures are to fill the gaps of the missing premises. From my perspective, then, Baillargeon's nativist conclusion is a consequence of her choice for a formal level of description of the learning mechanism. But it is precisely this choice that is being questioned in the current debate.

The traditional predilection for a formal level of analysis has been unchallenged for a long time, but of late alternatives with the same empirical rigour have become available. And while the empirical debate is far from settled as of yet, analyses of the kind provided by Smith look more promising to me because they are located at a basic level, while those preferring analyses at the higher formal level simply have more explaining to do.

Some thoughts about claims for innate knowledge and infant physical reasoning

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I have the pleasure of commenting on two forcefully argued and thought-provoking papers on topics that are at the top of the stack for developmental psychologists. Linda Smith offered an elegant portrayal of the epigenetic position, elaborated by compelling examples from a systematic program of research on language development. Renée Baillargeon confronted critics of the modern precocist view with a dazzling array of ingenious experiments that grapple with the difficult issue of representation.

Of course, I have complaints. First, the debaters did not really address one another's issues. To distill, Smith focused on nativist accounts of early cognitive development and Baillargeon on representational processes. This is not the fault of the debaters, because their published papers closely followed their verbal presentations, and they had inadequate opportunity to read one another's presentations while preparing their own. But realize that, for the most part, the participants attacked different issues and used different examples, making it hard to cast one's vote.

Instead of trying to force artificial clashes between our two worthy combatants, I will comment on the issues that they raised individually. At the same time, realize that I am no impartial referee, having critiqued the positions of Spelke and Baillargeon quite recently (Haith, 1998; Haith & Benson, 1998).

Smith's initial arguments against the nativist position, while stated precisely and powerfully, are aimed at the old nativism. Her closing discussion acknowledges

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the more complex possibilities in newer nativist and precocist positions that lean on the experience–expectant nature of many phenomena or the skills that are present at birth (probably better labeled as congenitally organized (Kessen & Mandler, 1961) than innate), and that use nativist concepts to refer to certain primitives of cognitive development as a shorthand to enable further developmental analysis. Her presentation helped to clarify the subtleties of an easily caricatured polarity.

While I favor Smith's epigenetic view, my perspective on what has happened to our field over the past three decades is that the anti-empiricist arguments have helped to move us along, and many of these arguments are nativist in character. We are much more inclined to acknowledge the biological directedness of early infancy. So, this is not a fruitless argument; it has pushed people to acknowledge that experience plays only a fractional role in early intellectual development and also to elaborate the ontogeny of so-called innate endowments. Where I have difficulty is with the notion of innate *knowledge* structures as opposed to innate (using that word to mean congenitally available) structures or processes that make the development of that knowledge likely. Part of the confusion lies in the word 'knowledge'. For example, Spelke uses depth perception as one of her two best guesses for a form of knowledge that is innate (1998, p. 193). But as Butterworth (1996) has noted, perception is not the same thing as knowing. Infants show evidence of depth perception long before they can reach or crawl, but this does not imply that they have knowledge of (know the meaning of) depth in terms, for example, of their own safety. It is much later that infants show fear of falling over an edge specifying depth (Campos, Bertenthal & Kermoian, 1992). Thus, it seems no more appropriate to talk about depth perception as 'knowledge' than color perception as knowledge.

I am reminded of the hullabaloo that surrounded the early findings of phoneme discrimination in young infants (Eimas, Siqueland, Jusczyk & Vigorito, 1971). The ready interpretation was that infants came into the world with knowledge of basic language sound components that gives them a leg up on the unique language life of *homo sapiens*. Then we found that chinchillas make the same phoneme discriminations as infants (Kuhl & Miller, 1978) and came to the more sensible conclusion that language evolved to take advantage of the discriminative properties of the mammalian ear, a perfectly reasonable strategy for evolution. The 'knowledge' is not there; the structure for acquiring that knowledge rapidly certainly is.

I took the position two decades ago (Haith, 1980) that, aside from lack of evidence for it, prepackaged knowledge doesn't make evolutionary sense; with others, I have believed that what is congenitally available is acquisition processes that exploit experience. These acquisition processes include visual scanning, looking and listening (as well as associative abilities), so that knowledge is the result of infants' application of their tools to objects and events. Agreed, the term 'learning', as psychologists use it, does not quite capture these nonassociative processes. Regardless, the evidence fits better with an infant who is endowed with acquisition tools that have been shaped through evolution to readily acquire knowledge of particular forms when they are encountered rather than the knowledge itself. As Baillargeon suggests, some information is acquired more readily than other information, presumably because of the structure of these tools. The evidence for innate ideas is not strong. Even for the 'idea' of continuity, the strongest claim made for innate ideas, Baillargeon finds mixed evidence at best. It seems unlikely that nature would be so cruel as to endow an infant with an innate idea that is so erratically applicable.

If I get to pick from Smith's multiple-choice array for the use of the word innate, I'll take the gene analogy. Mendel and his intellectual descendants did well enough by conceptualizing the gene as an entity, even though they had not the foggiest idea about how DNA was structured or even that DNA subentities produced proteins. And the fundamental progress was not undone by the subsequent deeper understanding of what a gene is. Likewise, investigators may be able to learn a great deal about the development of infant knowledge by being able to characterize knowledge entities as uniform or modular very early in life without knowing how they reached that state. Smith would like to search backward and understand the epigenesis of those entities, while Baillargeon and Spelke would prefer to document their presence and understand how they form the basis for what follows. There is room for both strategies.

What I have had to say about the issue of representation has been stated in less restricted space elsewhere (Haith, 1993, 1998; Haith & Benson, 1998). So, I'll summarize. There is no more difficult term in our field to understand because of the number of levels at which it is used. At the most basic level, 'representation' means energy transfer, as when used by neuropsychologists who talk about the visual input being represented in over 30 areas of the brain. With this meaning, even the fetus 'has' representations, because transducers convert physical energy of the world to neural pulses, which, in turn, undergo coding on the way to various sites in the brain. If fetuses do it, we should not be surprised that young infants do also. This is not a felicitous interpretation, because this term implies a recoding of information in the brain. People who use the term most likely mean a different kind of recoding, but we unfortunately have no specific guidance about what this meaning entails, because there is no functional information about how the 'representation' can be used. As Gallistel (1990, p. 24) put it, 'The experimenter must know not only what the mapping is but also what use is made of it. Until both are known, the character of the representation cannot be ascertained.' Another frequent use of the term refers to memory, a lingering effect of input after the causal stimulus disappears. But the implication of Baillargeon's use of the term is more adventurous, implying that there is both a recoding and dynamic use of information about events going on in the head that permit the infant to reason, realize, expect, be surprised and so on. Thus, the issue is not whether representation occurs in the infant's head but exactly what the properties of that representation are.

This is the nub of the argument. Those of us on the perceptual side have no argument with the claim for representation of some kind; we feel, however, that the representation in the experiments reported (i.e. what is going on during an occlusion period of a few seconds) is closely tied to the physical events and that the findings can be accounted for by known perceptual phenomena based on hundreds of habituation studies. It is important to realize that all investigators get from these looking paradigms is evidence for discrimination – longer looking at one object or event than another. The basis for the discrimination is left to interpretation. Concepts such as reasoning, realizing, expecting and being surprised are gratuitous mentalistic speculations about what is going on in the infant's head.

The easiest way to think about these experiments is to ask what would happen if we were to remove the occluder and let infants see the whole unfolding of the display sequence. If infants were to look longer at anomalous than non-anomalous events, we would not be surprised, because we know that infants look at events that are novel, whether based on their habituation or on their other-world experience. We would feel no need to infer such constructs as expectation, surprise, reasoning and the like. Babies are simply interested in odd or novel events. Now, when we introduce the occluder into the experiments and we find that infants again look longer at odd events, what must we conclude about infants' representations, especially when most of these experiments interrupt infants' observations by only a few seconds? (There are a few experiments that use longer delays but they typically involve older infants, and even there, the results can often be accounted for by consequences of an initial orientation bias produced by the experimental manipulation.) Must the baby create a mental structure and reason about it, or is it sensible to assume that she does pretty much what she does when she can see the whole event? There is evidence, in neurophysiological recordings with primates, that many of the neurons that are active when an object is present remain active for several seconds after it disappears, at least as long as the occlusion periods of most experiments (Ungerleider, 1995). It may be wrong to call this an afterimage or sensory storage, but it seems equally wrong to talk about complex recoding in this situation. We do not have a good word for what is going on at the behavioral/psychological level; perhaps the best we can do is simply say that the information remains in a degraded form that is fairly isomorphic to the original. If so, complex recoding has not occurred, and we can use known principles from looking and habituation paradigms to account for the results.

On a somewhat separate point, I find the conceptual terms that are used to describe the cognitive processes engaged in these experiments most troubling. There is no anchoring of these concepts, no independent evidence for infants expecting anything before it happens, no evidence for surprise, and no evidence for reasoning. All we know for sure is that infants look longer at one object or event than another. As one example, the infants in these experiments may expect nothing at all before the event terminates; at that point, the infant may detect a mismatch with similar remembered events, resulting in longer looking (Haith, Wentworth & Canfield, 1993). There is no forecasting here, just an after-the-fact realization that something happened that seems irregular. The gratuitous invocation of expectation and surprise and other mentalistic terms for these situations makes for conceptual confusion in theorizing about cognitive development.

What cannot be questioned about Baillargeon's program of work are the rich discoveries of what infants find odd about violations of very interesting dimensions of the physical relations among objects and events. At a bare minimum, these findings tell us that infants are sensitive to aspects of events that theorists have not typically considered. And the work on developmental changes in this sensitivity and how training affects sensitivity will contribute to an eventual characterization of how infants reason about physical events.

We have here strikingly different approaches to the science of our field. In spite of the divergent research strategies that investigators adopt and in spite of the polar positions that they confidently and competently defend, we still make progress. Debates aside, we must be doing something right.

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