INVITED ARTICLE

WHO PUT THE COG IN INFANT COGNITION? Is Rich Interpretation Too Costly?

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The basic topic of my address today concerns how much of cognition is in the head of the infant and how much in the mind of the theoretician. My general stance is that we are being treated to an interpretive flavor of infant behavior that is much too rich.

The balance of research in infancy has shifted over the past 15 years from the sensory and perceptual domains toward cognitive processes. This trend has created a connectedness between infants and adults that did not exist before. If infants can think, then perhaps their thinking is similar to our own mental activity. But we know that infants are different from adults and older children. The questions we ask and the research methods we use are unique to infants (Horowitz, 1992). Linguistic incompetence is key. Language is a core medium of cognitive functioning in older children and adults, and it is an essential tool for probing cognitive activity. But when it comes to language, babies just don't get it.

This misalignment—between characterizing the mental processes of infants as more adultlike than we once thought while depending on methods that are idiosyncratic to infants, causes problems and tensions in the field. We can all agree that infants are on the road to thinking like adults and older children, but how do we defend against excess adultocentrism—as Fischer and Biddell (1991) have put it—from inadequate methods?

The suggestions and arguments I make today come from many sources—a recent chapter by Haith and Benson (1998) and writings of Bogartz, Shinskey and Speaker (1997), Butterworth (1996), Cohen (1995), Cohen, Gilbert and Brown (1996), Fischer and Biddell

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INFANT BEHAVIOR & DEVELOPMENT 21 (2), 1998, pp. 167–179 Copyright © 1998 Ablex Publishing Corporation (1991), Mandler (1997), and Munakata, McClelland, Johnson, and Siegler (1997). There is a controversy going on. I borrow from these people liberally.

Our side charges that the other side has committed several misdemeanors, if not outright psychological felonies. Let's get them all out on the table. These consist of:

- Claims that certain types of knowledge are innate;
- Assertions that young infants know things about objects, events, and people far earlier than seems reasonable;
- Uses of indications for the earliest fragments of a concept as evidence for virtual mastery of the concept;
- Preemption of developmental analyses;
- Over-interpretation of findings as evidence for high-level cognitive operations (e.g., representation, reasoning, belief, expectations, surprise) in the absence of adequate definitions or anchoring observations or procedures;
- Employment of a minimalist perceptual (discrimination) paradigm that taps only a limited aspect of knowledge (certainly not functional knowledge) to infer cognitive operations; and
- Just being downright provocative.

These charges are sometimes unfairly melded even though they are separable; for example, claims of innateness do not necessarily imply very early emergence, and some claims of precocity come with experiential interpretations. Additionally, some claims for very early competencies accompany fairly elaborate attempts at developmental study. It is not easy to keep these distinctions clear.

A WORKING EXAMPLE OF THE PROBLEM

I'll pick on research in my own lab to get us started, which may be the only way I can get

some of my own research in here. This is a hypothetical case of rich interpretation.

The basic phenomenon that we demonstrated in our early work on the development of future orientation is that infants at 3 months of age and younger form expectations for predictable events very quickly (Haith, Hazan & Goodman, 1988; Robinson, McCarty & Haith, 1988). We let babies watch a video screen while we record their eye movements. In the simplest situation, little attractive pictures appear in alternation on the left and right side for 700 ms each, and a 1000 ms time interval of blank screen intervenes between the offset and onset of successive pictures. Later, we examine the videotape in stop-frame mode to determine whether infants made anticipatory fixations to the alternate location during the blank-screen interval and, if not, we record their fixation reaction time to the picture. In comparison to a random picture-sequence condition, infants make more visual anticipations and have faster reaction times during the predictable sequence, and these differences show up very quickly, in less than a minute of exposure to the sequence. We have interpreted these findings of visual anticipation and response facilitation as evidence for infant formation of expectations for forthcoming events.

Canfield and I (Canfield & Haith, 1991) went further to demonstrate that this phenomenon is not simply a short-circuiting of a visual pendulum effect, nor an entrainment phenomenon. We showed babies series in which the picture sequences were not symmetric, for example, repeating episodes of two pictures on the left and one on the right or three pictures on the left and one on the right. Infants at 3 months of age provided evidence that they could master the L-L-R sequences, again through anticipatory fixations and facilitated reaction times.

A rich interpretation of these data might go something like this. Because infants behaved appropriately in the absence of input (during the interpicture interval), they must have *represented* the forthcoming picture to themselves as well as the rule that governed the sequence

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(e.g., L-R or L-L-R). Further, because in the L-L-R sequence, they tended not to shift fixation locations after one picture on the left but did so after one picture on the right, and also tended to shift fixation location after two pictures on the left, they must have been *counting* the number of pictures on each side, and thus, they are also capable of *symbolic representation*.

Further work in a dissertation by Arehart (1995) demonstrated that when an infant has learned a L-R sequence, her visual performance is disrupted when there is a shift to a L-L-R sequence. A rich interpretation might proffer that infants, reasoning from exposure to the L-R sequence, believed that the pattern would continue, inferred that after each picture the next picture would appear on the opposite side, and were surprised when it did not, as evidenced by their disrupted tracking. We might go on to claim that at least the basic phenomenon reflects an innate skill since it occurs so early, at least as early as six weeks of age (Robinson, McCarty & Haith, 1988). Finally, we could argue that we have evidence of adaptive evolution, inasmuch as expectations for future spatial locations are important in survival both for predators and for prey. This story may seem a bit stretched but, in fact, it is not an outrageous example of the kind of interpretation that has been put on data collected with infants.

What is wrong with this picture?

1. There is no anchoring of the higherlevel cognitive constructs. For expectations, the anchors are clear enough through evidence of forecasting (visual anticipation) that is acted upon. But we have no indicators of belief, inference, reasoning, counting, symbolic representation, or surprise. We do have evidence that some kind of pattern was detected, certainly in the Canfield studies, and we have evidence of behavioral disruption. But, disruption could result from a mismatch between a programmed action and an expected consequence and not involve reasoning, inference, belief, or surprise.

- 2. Most of these constructs have meaning at later ages, specific meaning in terms of observables and measurables-surprise (e.g., HR acceleration, changes in muscle tone, facial gestures, verbalizations), reasoning (e.g., narratives and protocols, where we know that alternatives are considered), representation (e.g., in language or pretend play, or deferred imitation, for which some action is involved), inference (e.g., in language protocols). The use of the same terms in infancy and adulthood implies that there is a connection. In the absence of ways to tie these constructs down with young infants how can we possibly affirm or disconfirm claims of such a connection?
- 3. As much as I've told you, you might believe that infants' expectations are the same as those of adults'. I've put no constraints on the concept for infants; we have said nothing about how expectations probably grow in terms of their temporal and spatial extent, their richness of content or their dependence on the developing control of the environment by the viewer. (See Haith, Benson, Roberts, & Pennington, 1994). I'm guilty of presenting expectation formation as a dichotomous skill.
- 4. Does categorizing expectation formation as evolutionarily adaptive or as innate add much to our understanding? We can create a nice story, but we are doing little more than naming, assuming as Butterworth suggested, what we want to explain (Butterworth, 1996). Also, we have calculated that 3.5 montholds have had 800 hours of waking time, that is 48,000 minutes and almost 3 million seconds, during which they have made 3 to 6 million eye movements, plenty of opportunity to benefit from visual experience (Haith, Hazan, & Goodman, 1988). The key issues are how does the skill unfold, what experiences are necessary to fos-

ter it, and what is its developmental course? Again, there is no stop rule for speculation. What data could disconfirm the claims of nativist origins or the evolutionary course of knowledge of these constructs?

THE LOOKING PARADIGM

Now let's get the spotlight off of my lab, so we can pick on someone else. Since virtually all of the claims for early cognitive processes hinge on how long an infant looks at one display rather than another, it is sensible to examine the looking paradigm.

One difficulty is that people developed this paradigm to address sensory and perceptual questions, not questions of high-level cognitive processing. Many factors affect looking, including variations in the perceptual dimensions of objects and people, familiarity, novelty, recency, predictability, and the time lapse between stimulus exposures. Of course, a paradigm that is created for one purpose may be adapted for another, but investigators who pursue high-level cognitive constructs must play the default game. That is, one must fend off every possible perceptual interpretation of differences to entertain default cognitive interpretations. Surely, there are alternative interpretations for any experiment, but the use of perceptual paradigms tends to favor wellestablished perceptual explanations. Even when an immediate perceptual explanation is not obvious, there is the danger that one will come along.

For example, Bogartz, Shinskey, and Speaker (1997) analyzed experiments by Baillargeon and Graber (1987) and Baillargeon and DeVos (1991) that they originally interpreted as demonstrating that infants *represented* a tall toy rabbit as it moved behind a screen, *assumed* that it should continue to move, *expected* it to appear in a high window in the screen before it emerged from the far side, and were *surprised* that it did not. Infants looked longer when the episode involved a tall

rabbit than a small one, and infants even as young as 3.5 months of age were held to possess object permanence and be able to reason about the height and trajectory of hidden objects. Bogartz et al. identified several perceptual variables that remained the same or changed between the habituation and test periods, including the zone of infant tracking and the amount of visible movement in the habituation and test displays, and developed a multivariate perceptual model of the episode that they then tested. Their elegant design used all conditions for both test and habituation episodes, rather than the single test condition that is customary, so they could test for the effect of various perceptual contrasts. The resulting data closely fit their model that emphasizes perceptual salience, change and novelty with no evidence that the impossible condition affected infant looking. Bogartz et al. discussed similar analyses of several other experiments although they have not been tested. To quote their conclusion:

> In considering one study after another purporting to establish young infant knowledge of the properties of occluded objects or portions of objects, we have found repeatedly that the same simple ideas of motion capturing attention, tracking and scanning of objects, and longer looking to displays dependent upon their relationship to previous displays, has great explanatory value ...(1997, p. 420).

These conclusions have been challenged by Aguiar and Baillargeon (1997), so this particular story is not over.

Likewise, Cohen and his colleagues (Cohen, 1995; Cohen, Gilbert & Brown, 1996) carried out several attempts to replicate studies of solidity and continuity reported by Spelke and her colleagues (Spelke, Breinlinger, Macomber, & Jacobson, 1992) as support for the core principles hypothesis but with added comparison groups. Several of the original studies found that infants looked longer at a display when one object appeared to pass through another. Cohen et al. found no support for longer looking at impossible intrusions of one object through another at 4 months of age but some tendency for 8-mo olds to look at an episode when a ball moved through a wall whether the event was consistent or inconsistent, that is, whether or not the wall had a big hole in it. Only at 12 months of age did infants behave according to the core principle predictions, far later than the 2.5 months of age reported earlier. There may be something that is perceptually attractive about one object being partly occluded and disoccluded by another as happens when an object moves through a wall or a gap in a wall.

A second issue is that in many infant looking studies, the habituation sequences create complications because one doesn't know whether infants compare the test experience with what happened during familiarization, or with their life experience, or a mix of the two, or whether experience is irrelevant. An infant comes to the lab with a lot of history, and it is unclear how that history interacts with the familiarization experience. For example, outside the lab, infants do not typically see people drop things that fail to fall to the nearest surface. Differential looking activity to two episodes is ambiguous with respect to what the infant uses as a base of comparison.

A third issue is that looking paradigms were initially designed to answer yes/no questions-such as can infants discriminate shapes, colors, motions, and depth-that were oriented to such skills, as color vision and form and pattern discrimination, rather than to higher-level cognitive processes. For cognitive experiments, the question typically is whether babies recover more under one condition than another, a dichotomized inquiry. Unfortunately, our methods influence our conceptions, and the use of the looking paradigm for cognitive inquiry has encouraged dichotomous answers to cognitive questions also. Can infants do arithmetic? Do infants perceive causality? Do infants appreciate continuity, cohesion, and inertia in object motion? Can infants do superordinate categorization? We need to remember that dichotomous data may not

reflect the complexity and continuity of the underlying processes.

Finally, the looking paradigm was aimed at discovering *competencies*, not the processes that underlie those competencies. It is one thing to ask if an infant reacts, for example, to a violation of gravity. It is another to invoke high-level constructs to explain what that reaction reflects. After all, we only obtain evidence in these experiments that babies look at one thing longer than another. Without anchors on the behavioral side that permit us to distinguish among alternative mental constructs, we create confusion for people who want to create a developmental story of how these processes develop.

AREAS OF INQUIRY

The foundation for the debate rests on several pillars, including the object concept, the understanding of causality, and infant arithmetic, categorization, and physical reasoning. I can only talk about a subset of these here to illustrate my points.

Causality

The work on causality underlies one of the core concepts that Spelke and colleagues have claimed are innate (Spelke, 1991, 1994; Spelke et al., 1992), based on the work of Ball (1973) and Leslie (Leslie, 1984: Leslie & Keeble, 1987). Leslie showed infants films of two bricks, one red and one green. The green one sat in the middle of the screen, and the red one moved from the left side of the screen toward the middle until it contacted the green brick, at which point the red brick stopped and the green brick moved along the same path. Adults say that the red brick caused the green brick to move; they experience a transfer of force from the red to the green brick. Leslie familiarized infants to this sequence or to sequences that had the same components but differed in spatial contact or timing. For one sequence, the red brick stopped 66 mm short of contact, but the green brick began to move immediately. For another, the red brick contacted the green brick, but the green brick did not begin to move until .6 s later. Adults do not see these latter two sequences as causal. Infants looked longer during test when the familiarization or test included the causal episode than when both involved different noncausal episodes. Leslie proposed from these and other experiments that infants possess a modular structure that permits them to experience causality directly, without tuition. This module emerges around 7 months of age.

The causality work illustrates the ambiguity inherent to the looking paradigm. Perceptual interpretations of the findings are possible that could undermine the notion that infants have a core innate belief about how contacting objects behave. Only in the causal sequence does one have simultaneity of three factors: red block stops, red block contacts green block; and green block moves. As movie producers and orchestra leaders know, simultaneity of perceptual inputs is a powerful factor. Babies know this as well; the simultaneity in the peeka-boo game of hands moving away from the face, the face appearing, and the pronouncement of "peek-a-boo" is key to the resulting peals of laughter. Here is a prediction. Place a vellow block a half-inch above the contact point of the red and green blocks, and when the red and green blocks contact, immediately move only the yellow block upward. I predict that one will find equivalent effects for infant looking as for the causality sequence because simultaneity is present in both. Even if the prediction is wrong, the claim that young infants experience causality is a strong one that should have a firmer base of support than looking reactions to billiard-ball analogies alone. For example, one would expect functional consequences of a true causality experience such as a baby reaching out to intercept and catch the green brick before the red brick sends it on its way. The causality work shares a limitation with other research domains for which extraordinary claims are made of early infant competency: there is virtually no evidence that the

putative cognitive skill has functional or action consequences. Rather, inferences are drawn from looking behavior alone.

Oakes and Cohen (Cohen & Oakes, 1993; Oakes & Cohen, 1990, 1994) have argued that the claim of modular status for the appreciation of causality is dichotomous and certainly non-developmental. In several studies, they have demonstrated that various factors affect infants' responding to so-called causality episodes depending on age-including the simplicity of the objects involved and whether they move on the same trajectory, are occluded during contact, or the agents and recipients of the actions vary. Even if one accepts differential looking as sufficient to make the case for a causality experience, the evidence indicates that it is anything but an all-or-none affair. Rather, it appears to be much more fragmented and developmentally based than is implied by a nativist modularity conception.

Physical Reasoning

Now, I want to turn to studies of physical reasoning, using a well-known and very clever study by Baillargeon (1987) as an example. Infants became familiar with a screen whose top rotated toward and away from them through a full 180° extent. Following familiarization, the experimenter laid the screen flat with the top toward the infant and placed a box behind the pivot plane of the screen. In the possible condition, the top of the screen then rotated away from the infant, first occluding the box, and then reaching a position a few seconds later where it contacted the box and stopped. In the "impossible" condition, the screen continued to rotate a full 180°, as for familiarization, until it lay flat with the top facing away from the infant, invading the space that the box had occupied. Infants looked significantly longer at the impossible than the possible event.

These data were interpreted as demonstrating that the infant represented the box during occlusion, reasoned that two things can not be in the same place at the same time, consistent with the continuity core principle, and manifested their surprise by longer looking. Among several alternative interpretations, Munakata et al. (1997) suggested that infants predicted how far the drawbridge would go, which was violated when it continued to move, an alternative free of infant representation. I favor a different alternative. First, realize that only around 2.5 s elapses from the time that the top of the screen fully occludes the box and a "violation" begins to occur. Is it possible that infants have some lingering sensory activation, in a way, still "seeing" the barrier box as the drawbridge swings backwards? Let's rid ourselves of the occlusion part of this experiment so that we can think about it more clearly. Say the infant looks at this episode at an angle from the side, so that she can actually see the box behind the drawbridge as it moves toward the box and magically moves through it. We are not surprised at all if infants look at that episode longer than when the drawbridge stops on contact. Why? Because infants often see one moving object contact another but never see one solid object go through another. In fact, Baillargeon and her co-workers have been quite eloquent in espousing real-world experiences as the basis for their findings (Baillargeon, Kotovsky, & Needham, 1995, pp. 88-89). The pivotal issues arise because of the occlusion manipulation, so we need to do replication thought experiments with no occluder to keep our thinking straight. Almost without fail we can account for infants' longer looking at an inconsistent or impossible event, in these nooccluder thought experiments, in terms of well-established perceptual principles-novelty, familiarity, salience, and discrepancy.

So what is special about occlusion episodes? This is a central issue because it is almost always for occlusion events that people talk about representation, physical reasoning, and the like. I believe that there is nothing about the typical occlusion event that requires us to use different principles. The time factor in occlusion events is usually not emphasized, but it typically lasts for no longer than 3 s. So, what kind of representation are we talking about?

I digress, but no concept causes more problems in discussions of infant cognition than that of representation. What do we want to imply by this term? In infant research, the term often refers only to a residual that exists beyond the duration of the external input. More generally, we can consider a representation as something that stands for something else, as when a form of energy undergoes a transformation. Neuropsychologists use the term representation to refer to the coding of information in neural networks. For example, they tell us that visual information is "represented" in over 30 different areas of the brain. If that is how we are using the term, then even the fetus "represents" events because, for example, pressure on the leg is transformed into electrical pulses, and different mappings occur at synapses at several junctures along the way to the brain. Infant researchers must mean more than that. If we mean that infants can represent 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, then we are talking about something else, something that begins to sound like a symbolic representation. Is this what is going on in the 3 s or less that most experiments occlude an event or that something should happen to an occluded event? Let me give you an absurd example to think about: When infants blink, all external input is eliminated for 50 to 100 ms, yet no one believes that infants process the environment anew each time the lids reopen or must represent the environment to themselves during the blink. Clearly cognition does not necessarily begin when an object disappears, so where is the temporal breakpoint? The question boils down to: When are we dealing with lingering sensory information following disappearance, and when are we dealing with self-generated images and their manipulation?

Evidence from the adult literature tells us that adults can keep literal visual information

alive for several seconds following disappearance (Baddeley, 1990; Phillips & Baddeley, 1971). There is also 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. For example, when macaque monkeys perform a matchingto-sample task with a delay between presentation of the sample and the match, many of the same neurons fire in the inferotemporal cortex that fired when the stimulus was actually present. The activity lasts, on average, for around 16-18 s (Fuster & Jervey, 1981; Miyashita & Chang, 1988; for a review of this and related work, see Ungerleider, 1995).

In reviewing this literature, Ungerleider concluded:

Many studies have found cells whose response to the initial cue is maintained at some level throughout the delay period. Thus, the memory of the cue appears to endure by maintaining the activity of cells that represent the cue. (1995, p. 774).

The point for us is that there is substantial evidence that the kind of representational activity that investigators invoke in many infant studies, involving delays of only a few seconds, could be very closely tied to sensory/ perceptual activity when the object or event is present. Thus, the same factors that operate in the presence of stimuli and events—namely, familiarity, novelty, and discrepancy—could be operative during the short delays that these experiments employ.

Is there any evidence that infants use lingering sensory information in these experiments? I believe so. In an elegant set of studies, Baillargeon (1992) set a second "reminder" box next to the occlusion box, which remained visible as the drawbridge rotated. The reminder box enhanced infants' performance, presumably by helping infants secure the trace of the hidden box. But more interestingly, the surface features of this reminder box mattered. The reminder box assisted 4.5-mo olds' performance only when its decoration was highly

similar to the target box. Performance of 6.5mo olds benefited from the presence of either a highly similar or moderately similar decoration, but not from a very dissimilar decoration. This seems like a very sensory-based visual representation. I agree with Baillargeon (1991) when she says: "... infants keep in their mind's eye an image of the [occluded] box ..." But, if so, why do we need principles of physical reasoning to account for the results? If the "representation" during occlusion is a close analogue of the original, if the baby is using degraded sensory information rather than a re-computation and transformation of that information, why would we use different principles to account for behavior when the sensory information is simply upgraded by an actual view of the impossible event (as in our non-occluder thought experiments)?

Numbers

Let's consider another well-known and clever experiment on infant arithmetic.

Wynn (1992) argued from data on looking experiments that 5 mo-old infants could do arithmetic. In a 1 + 1 condition, infants first saw a single doll on a stage, that a screen then occluded. A hand appeared that held a second doll, which moved to disappear behind the screen, and then the hand emerged empty. An adult would assume that the second doll was left behind the screen. When the screen dropped to reveal either one doll or two, infants looked longer when there was only one doll, presumably surprised by a violated expectancy. A consistent result obtained in a 2-1 condition that started with two visible dolls that a screen then occluded. Here, the disembodied hand first appeared empty, moved behind the screen, and emerged with one doll, giving an adult the impression of only a single doll remaining behind the screen. When the screen was removed, infants looked longer when there were two dolls than one.

Some fairly strong claims have been made on the basis of these data that, if true, have farreaching implications. The data have been taken to indicate ".. that infants possess true numerical concepts and suggest that humans are innately endowed with arithmetical abilities." (Wynn, 1992, p. 749.)

Once again, we do a replication thought experiment without an occluder. A single doll stands on a stage (the 1 + 1 condition), and then a hand appears with a second doll, deposits it next to the first doll, and then the hand disappears. Poof! The second doll suddenly vanishes. Of course, infants should look longer when the second doll vanishes than when nothing happens, because visible vaporization is unusual. Or, two dolls stand on a stage (the 2 -1 condition), a hand appears and removes one doll. Shortly after, a second doll suddenly materializes on stage. Again, infants look longer at the materializing event than a nonmaterializing event. For neither condition would we appeal to representation or arithmetic skills; rather, we would say that infants look more at events that are weird.

How must we alter our thinking when a screen hides the placement of the second doll on the stage or its removal? Assume that when the screen hides one doll, infants still have some decaying perceptual information about that doll. The hand appears with a second doll and disappears behind the screen, and infants have lingering information about the doll in motion, presumably halted by the first surface, the stage. When the hand emerges empty, the lingering sensory information about the two dolls continues, in a degraded form. Then the screen disappears and only one doll appears instead of two, mismatching the content of infants' decaying information. Infants look longer at a mismatch. A similar scenario occurs for the 2-1 condition. Bogartz et al., (1997) offered a similar possibility for occlusion studies of object knowledge.

Of course, neither Bogartz et al. nor I believe that infants have Superperson-like Xray vision, but the thought experiment is illuminating. Infants' "representations" could be very literal and sensory-based. We need not appeal to numerical competence, innate endowment of arithmetical abilities, or ability to reason about numerical quantities to understand the addition or subtraction outcomes.

Uller and Huntley-Fenner (1995) and Wynn (1995) obtained results that fit our interpretation. Wynn found that infants performed better in a subtraction (3-1) than in an addition condition (2 + 1). In the subtraction condition, the infant initially sees the whole set together, so the infant can operate on an already established sensory tableau. Uller and Huntley-Fenner (1995) carried out an addition study (1 + 1)in which they varied infants' opportunity to see the initial object in place. In an object-first condition, they showed infants a single object on stage before occluding it and then added another object. In a screen-first condition, they started with the occluding screen in position and then added one object behind the left side of the screen and another behind the right side. The original Wynn finding replicated only for the object-first condition. That is, infants did a lot better when they began by actually seeing the object in place. There is clearly an effect of varying infants' opportunity to build a strong visual image of the objects.

Again, this seems like literal sensory information that we are dealing with, not with flexible symbolic representations, numeric or otherwise.

THE NEED FOR GRADED CONCEPTS

This discussion about representation helps to make the point about the confused state of affairs of our concepts and the problem in using evidence of fragments for a concept as evidence for the whole. Now that we recognize that even fetuses "do it," the need for a fullscale developmental model of representation that incorporates the notion of partial accomplishments is obvious. We can not get by with a single term whose meaning spans the full distance from energy transformations in the CNS to mental manipulations of symbols. The question is, what is different about infants' mental contents at different stages of development? We need a full developmental model with lots of steps along the way.

I and others have pointed out elsewhere that part of the problem is that we have no comfortable way to think about or to model these partial accomplishments (Benson & Haith, 1995; Haith, 1990, 1993; also see Munakata et al., 1997). The idea is that, without a model or theory of partial achievement, we are driven to think in dichotomous terms. Consider color vision in infants, for which we do not possess a model of partial skill. We ask whether an infant discriminates colors and, if so, we grant the infant color vision. But, no one really thinks that 3-month-old infants distinguish the shades of color as well as a normal vision adult. The problem is that we have no mental model of what some, but incomplete, color vision might be like. Now contrast this state of affairs with visual acuity. Many of us have blurred vision or have seen out-of-focus images, so we have a mental model of what some, but not perfect, vision is like. We even have a quantitative indicator (Snellen acuity) to reflect imperfection. Accordingly, we would never assume that a young infant who can see can also see perfectly. Unfortunately, we do not have similar models for such topics as categorization or causality or many aspects of physical reasoning. As a result, we tend to think dichotomously. Although there are graded characterizations for the development of representations, post infancy (e.g., Fischer, 1980), much of the discussion in infant work is over whether infants have them or not, typically to challenge Piagetian theory. But, until we hear more about these representations, I'm not sure. I don't think that Piaget would have a major problem with "representations" in early infancy that consist of degraded sensory information.

CONCLUSIONS

1. Are rich interpretations too costly? It depends. Traditional arguments against nativist and precocist approaches are

that they are too deterministic and shut down attempts at process explanations and developmental analyses. I see no problems here. The nativist and precocist positions have actually been provocative and generative, encouraging many to seek out alternative explanations to prove them wrong. The studies in these areas have been unusually creative and ingenious, and the interpretations have been clear and bold, even courageous. They have forced many of us to examine what we mean by such concepts as reasoning and representation, and they have provided fodder for entertaining debates.

My concern is that there may be a cost outside the field of infancy. When outside researchers work with the same cognitive concepts that we invoke in rich interpretation, confusion arises. If infants can reason and make inferences, why can't three-year olds? If surprise is clearly defined by observables for children and adults, how does a developaccommodate mental theorist undocumented reports of surprise in infants that are not based on observables? If infants know simple arithmetic, why don't three-year olds? In brief, rich interpretation can contribute to theoretical muddle.

There are also potential repercussions in the lay literature. Increasingly, the idea has become fashionable that our infants are little scientists. Brand new parents call me, and I am sure they call many of you, to ask when they should begin with the flash cards. Apart from the specter of these infant scientists threatening our job security, I fear that if these characterizations are overblown, as more qualified renditions appear, they will either be ignored because they are insufficiently sensational, or the lay public will nod their heads in affirmation of their preconceptions about social science research. I

worry about this cost, especially because infancy research occupies such a commanding platform.

2. Infants do appear to see nonsensible events as odd, and this information is valuable. For example, research on physical reasoning has made an important contribution by uncovering several physical events about which infants must have information; otherwise, they would not respond to aberrations. This "seeing as odd" could constitute a seed from which genuine understanding of a concept emerges as the child tries to account for why something does look strange; for example, seeing one object go through another may prompt inquiry into why, a step to true understanding.

Still, don't forget an important point that Butterworth (1996) made recently: Perception and knowing are not the same thing. A person can regard an event as odd without knowing why. Isaac Newton would have been surprised to see an apple fly upward from a tree during his youth, but it took many more years for him to formulate the principle of gravity. We need to be clearer about what we mean by the term "knowledge."

- We need a kitbag of new terms to talk 3. about infant constructs, or we need to fractionate the terms we are using to enable developmental analysis. Researchers must convey what they mean to imply by terms as well as what they do not intend. We have a fair collection of infant competencies, but we need to know about the functional implications of these competencies as well as incompetencies. Efforts should be made to anchor the use of terms and their gradations in observations.
- 4. So who put the cog in infant cognition? I suspect again that one's answer will depend on perspective. The suitability of cognitive constructs for the infant at any particular age may be similar to the

embryologist's dilemma when asked to decide when a cell cluster is a brain, or an eye, or a heart. Certainly these names are applicable by the end of the first trimester of gestation, certainly not when the germ layers are first established; the brackets are there, but the zones of demarcation are fuzzy. In fact, the issue dissolves for the embryologist, because she can lay out the whole sequence. Unfortunately, we don't even have the luxury of knowing with certainty what the primordial structures are for the constructs and competencies we entertain.

For now, I'll answer that it is the psychologist who has put the cog in infant cognition, at least in the early months. I look forward to firm evidence of when the infants" cognitive gears really begin to mesh.

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