BEHAVIORAL AND BRAIN SCIENCES (2001) **24,** 1–86
*Printed in the United States of America*

# The dynamics of embodiment: A field theory of infant perseverative reaching

**Esther Thelen**
*Department of Psychology and Program in Cognitive Science, Indiana University, Bloomington, IN 47405*
**thelene@indiana.edu** **www.php.indiana.edu/~gormleyf**

**Gregor Schöner**
*Centre de Recherche en Neurosciences Cognitives, C.N.R.S., Marseille, Cedex 20, France*
**gregor@lnf.cnrs-mrs.fr**

**Christian Scheier**
*Department of Biology, California Institute of Technology, Pasadena, CA 91125*
**scheier@neuro.caltech.edu**

**Linda B. Smith**
*Department of Psychology and Program in Cognitive Science, Indiana University, Bloomington, IN 47405*
**smith4@indiana.edu**

**Abstract:** The overall goal of this target article is to demonstrate a mechanism for an embodied cognition. The particular vehicle is a much-studied, but still widely debated phenomenon seen in 7–12 month-old-infants. In Piaget's classic "A-not-B error," infants who have successfully uncovered a toy at location "A" continue to reach to that location even after they watch the toy hidden in a nearby location "B." Here, we question the traditional explanations of the error as an indicator of infants' concepts of objects or other static mental structures. Instead, we demonstrate that the A-not-B error and its previously puzzling contextual variations can be understood by the coupled dynamics of the ordinary processes of goal-directed actions: looking, planning, reaching, and remembering. We offer a formal dynamic theory and model based on cognitive embodiment that both simulates the known A-not-B effects and offers novel predictions that match new experimental results. The demonstration supports an embodied view by casting the mental events involved in perception, planning, deciding, and remembering in the same analogic dynamic language as that used to describe bodily movement, so that they may be continuously meshed. We maintain that this mesh is a pre-eminently cognitive act of "knowing" not only in infancy but also in everyday activities throughout the life span.

**Keywords:** cognitive development; dynamical systems theory; embodied cognition; infant development; motor control; motor planning; perception and action

It is far too little recognized how entirely the intellect is built up of practical interests.
William James, 1897, p. 36

## Introduction

A century after William James created a psychology based on the primacy of experience, the ideas of embodiment are again entering the debate in the mind sciences. To say that cognition is embodied means that it arises from bodily interactions with the world. From this point of view, cognition depends on the kinds of experiences that come from having a body with particular perceptual and motor capabilities that are inseparably linked and that together form the matrix within which reasoning, memory, emotion, language, and all other aspects of mental life are meshed. The contemporary notion of an embodied cognition stands in contrast to the prevailing cognitivist stance which sees the mind as a device to manipulate symbols, and is thus concerned with the formal rules and processes by which the symbols appropriately represent the real world. There is now converging interest in embodiment from scholars in philosophy, cognitive science, psychology, linguistics, robotics, and neuroscience (Almássy et al. 1998; Ballard et al. 1997; Brooks 1991; Chiel & Beer 1997; Clark 1997; Damasio 1994; Edelman 1987; Fogel 1993; Gibson 1969; Glenberg 1997; Harnad 1990; Johnson 1987; Lakoff 1987; Lakoff & Johnson 1980; Merleau-Ponty 1963; Newton 1996; Pfeiffer

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https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

& Scheier 1999; Sheets-Johnstone 1990; Talmy 1988; Thelen 1995; Thelen & Smith 1994; Varela et al. 1991).

In this target article, we contribute to this multidisciplinary effort by focusing in considerable detail on a particular, and controversial, phenomenon seen in human infants, the so-called, "A-not-B error." We present theory, a model, simulations, and experiments that recast this phenomenon in embodied terms, using assumptions and the formal language of dynamic systems. We believe this offers an attractive starting point for an embodied cognition for several reasons:

1. We cast the mental events involved in perception, planning, deciding, and remembering in the analogic language of dynamics. This situates cognition within the same continuous, time-based, and nonlinear processes as those involved in bodily movement, and in the large-scale processes in the nervous system (Freeman & Skarda 1985; Kelso 1995; Koch & Davis 1994; Port & van Gelder 1995; Singer 1990; Turvey 1990; van Gelder 1998). Finding a common language for behavior, body, and brain is a first step for banishing the specter of dualism once and for all.

2. Because perception, action, decision, execution, and memory are cast in compatible task dynamics, the processes can be continuously meshed together. This changes the information-processing flow from the traditional input-transduction-output stream to one of time-based and often shifting patterns of cooperative and competitive interactions. The advantage is the ability to capture the subtle contextual and temporal influences that are the hallmarks of real life behavior in the world.

3. We address specifically the developmental origins of cognition. Since Piaget (1952; 1954), it has been widely acknowledged that all forms of human thought must somehow arise from the purely sensorimotor activities of infants. But it is also generally assumed that the goal of development is to rise above the "mere sensorimotor" into symbolic and conceptual modes of functioning. The task of the developmental researcher, in this view, has been to unearth the "real" cognitive competence of the child unfettered by performance deficits from immature perception, attention, or motor skills. This division between what children really "know" and what they can demonstrate they know has been a persistent theme in developmental psychology (Gelman 1979; Spelke 1990). We argue here that these discontinuities are untenable. Our message is: if we can understand this particular infant task and its myriad contextual variations in terms of coupled dynamic processes, then the same kind of analysis can be applied to any task at any age. If we can show that "knowing" cannot be separated from perceiving, acting, and remembering, then these processes are always linked. There is no time and no task when such dynamics cease and some other mode of processing kicks in. Body and world remain ceaselessly melded together.

The burden of this larger agenda rests on our dissection and reinterpretation of a classic infant perseverative reaching phenomenon, the well-known "A-not-B" error (Piaget 1954). The dynamic field model formalizes a new approach to this error first suggested in conceptual form in Thelen and Smith (1994) and subsequently extended and supported by a series of experiments described in Smith et al. (1999b). The model is an adaptation of Erlhagen and Schöner's (2001) dynamic neural field theory of motor programming (Schöner et al. 1997), and thus offers a bridge between the more general processes of motor planning and execution and the developmentally specific effects revealed by the A-not-B task. The model offers a powerful and parsimonious, yet biologically plausible, account of the many contextual influences on A-not-B tasks that have puzzled developmental psychologists for two generations. More generally, it demonstrates the elegance and usefulness of dynamic systems principles and language for understanding the intertwined processes of perceiving, deciding, acting, and remembering, and their changes over time.

The article proceeds as follows: First, we describe the A-not-B error in its canonical form and the variations that constitute the data to-be-explained. We show how previous explanations each capture some of the phenomenon but fail to account for all the known effects. Next, we lay out the broad outlines of the new approach put forth in Thelen and Smith (1994) and Smith et al. (1999b) and the empirical

Esther Thelen has a Ph.D. in Biological Sciences from the University of Missouri and has been a Professor of Psychology and Cognitive Science at Indiana University-Bloomington since 1985. Her research has centered on the acquisition of motor skills in infants, the relation of movement to cognition, and developmental theory. She is the author of over 85 papers and, with Linda Smith, the book *A dynamical systems approach to the development of cognition and action.* She recently served as the President of the International Society for Infant Studies.

Gregor Schöner is Directeur de Recherche at the Centre de Recherche en Neurosciences Cognitives of the French Centre National de la Recherche Scientifique. Originally trained as a theoretical physicist, he has developed ideas from dynamical systems theory for such diverse fields as perception, movement, cortical neurophysiology, and autonomous robotics, with a recent interest in cognition. Over the last 15 years he published over 85 papers in experimental psychology, neuroscience, and engineering. He has lectured on dynamical systems ideas to many different audiences, with the goal of contributing to interdisciplinary interchange.

Christian Scheier is a postdoctoral fellow at the California Institute of Technology. He is the author of over 40 publications in the areas of time series analysis, autonomous agents, crossmodal psychophysics, and infant development. He has recently published, together with Rolf Pfeifer, a book on embodied cognitive science with MIT Press. He did his Ph.D. on category learning in autonomous agents at the Artificial Intelligence Laboratory at the University of Zurich.

Linda Smith is Chancellor's Professor of Psychology and Cognitive Science at Indiana University-Bloomington. She received her B.S. degree in 1973 from the University of Wisconsin-Madison and her Ph.D. in psychology from the University of Pennsylvania in 1977. She joined the faculty at Indiana University in 1977. Her research is directed to understanding developmental processes especially as it applies to cognition and word learning. She has published over 80 research articles and is co-author with Esther Thelen of *A dynamical systems approach to the development of cognition and action.* Her research is supported by grants from the National Institutes of Child Health and Development and the National Institute of Mental Health.


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

support for that approach. Then we introduce the major assumptions of the dynamic field model and discuss why the model is so well-suited to explaining the A-not-B error. This is followed by a description of the model, and a series of simulations that capture the main A-not-B effect as well as the known contextual variations. We evaluate the strengths and shortcomings of this model in relation to other explanations. Finally, we offer some speculations about the model's more general usefulness for integrating multiple, time-based processes of human cognition and action.

## 1. The A-not-B error

The A-not-B error was first described by Piaget (1954) in the context of his life-long quest for the developmental origins of knowledge. Piaget was particularly concerned with the question of when infants understand the properties of objects, and especially that objects continue to exist even when they cannot be seen or acted upon directly. Through a series of clever hiding games he played with his own children, Piaget discovered that such object knowledge arises gradually and rather late in infancy. Before 7 or 8 months-of-age, infants refuse to search for a toy hidden under a cover, as though the toy simply ceased to exist. After about 12 months, they search robustly, even after the toy is hidden in several places in succession. But between 7–12 months, infants display a peculiar kind of "partial knowledge" where they search at one location, but cannot switch their search if the toy is then switched to a second or subsequent hiding place. Piaget labeled this behavior as "Stage IV" in his series of stages in the development of adult-like object permanence: Infants act as if the toy had lasting existence only where it first disappeared.

In the decades since Piaget's first descriptions, the hiding task has been repeated countless times, in laboratories all over the world, and with myriad variations (see reviews by Acredolo 1985; Bremner 1985; Diamond 1990a; 1990b; Harris 1987; Markovitch & Zelazo 1999; Munakata 1998; Wellman et al. 1986, among others.). The classic, canonical version goes like this: An investigator hides a small, attractive toy under one of two identical hiding places, usually cloth covers or hiding wells with lids, and allows the infant to search and recover the toy. After a number of hidings and recoveries from the first location, "A," the investigator hides the toy in the second location "B" in full sight of the baby. If there is a few second delay between the hiding event and when the infant is permitted to search, 8- to-10-month-old infants reliably make the "A-not-B" error, that is, they reach back to the original location "A," even though they saw the toy hidden at "B."

It is both surprising and endearing to see infants so determined to make a mistake. But the reason that the A-not-B error has intrigued developmentalists for nearly 50 years is not just the phenomenon itself, but the questions raised by the many variations of the task studied over that time. Here is the puzzle: while the A-not-B error is entirely robust in the canonical form we described above, even seemingly small alterations in the task conditions can disrupt it. Nearly every aspect of the event matters: the visual properties of the hiding locations, including the distinctiveness, distance, number, and transparency of the covers (e.g., Bremner 1978b; Butterworth 1977; Butterworth et al. 1982; Horobin & Acredolo 1986; Sophian 1985), the delay between hiding and search (e.g., Diamond 1985; Gratch et al. 1974; Harris 1973), whether search involves reaching or just looking (e.g., Hofstadter & Reznick 1996) whether there are landmarks in the environment (Acredolo 1979), whether infants search for objects, food treats, or people (Diamond 1997), whether the task is done at home or in the laboratory (Acredolo 1979), whether the infant or the hiding places have been moved (e.g., Bremner 1978a), and the infants' amount of crawling experience (e.g., Bertenthal & Campos 1990; Horobin & Acredolo 1986). Such diverse context effects pose a serious challenge to Piaget's original interpretation. If the A-not-B error is a true measure of the status of infants' representations of objects, how can it be that what they know depends on so many seemingly irrelevant factors? How can it be that infants have a more mature object concept at home than in the laboratory or when the object is a cookie rather than a small toy?

### 1.1. Some explanations

The contemporary consensus is that Piaget's account is incorrect, but opinions differ on why the classic explanation is insufficient. One group of developmentalists argue that Piaget was asking the right question, but that he simply chose the wrong behavioral task to answer it. (Baillargeon & DeVos 1991; Baillargeon & Graber 1988; Bertenthal 1996; Diamond 1990b; Munakata et al. 1997). These theorists focus on the striking decalage between what infants seem to know about hidden objects when they manually search for them compared to when they just watch hiding events. Experiments using visual violation-of-expectancy measures have shown that many months before infants routinely make the A-not-B error when reaching for hidden objects, they seem to expect that objects will be retrieved from the place they were just hidden when no reaches are involved. They demonstrate this knowledge by looking longer at events where an object is plucked from a place other than the one where the baby watched it last disappear (e.g., Ahmed & Ruffman 1998; Baillargeon & Graber 1988). Reaching is the problem, some argue, because it requires a "stronger" object representation than looking (Munakata et al. 1997), or because it involves additional means-ends performance demands (Baillargeon & Graber 1988), or because the "knowing" system is unable to control the "acting" system (Ahmed & Ruffman 1998; Bertenthal 1996). Infants, Diamond (1990b, p. 662) maintains, "really know where the [object] is even when they reach back to where they last found it."

One foundational assumption behind these dual-process (knowing vs. acting) accounts is that there lives, in the baby's head, a creature that is smarter than the body it inhabits; that there is a sharp partition between the mental events that precede the decision to act and the action itself. In this scenario, an object concept develops that is disembodied, timeless, and modular, and that may or may not actually motivate behavior. Moreover, there is the second assumption that because looking is motorically less complex than reaching, it will have a privileged access into the object knowledge module; it is a better measure of infants' "real" object concept. Bertenthal (1996) takes this further to suggest that knowing and acting are two dissociable systems, with different anatomical bases. One system represents objects, develops early, and is tapped by the visual expectancy studies. The A-not-B error comes from a second, "perception-action" system and has nothing to do with ob-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

ject representation. But if the A-not-B error is indeed *not* about the object concept at all, the intriguing question remains. What are the mechanisms that account, in the same baby, for accurate performance under some circumstances and perseverative reaching under other conditions?

One way to view the A-not-B error is simply as a reach to the wrong location. Indeed, a second group of theorists explain the A-not-B error as a manifestation of infants' immature abilities to direct their movements in space. Here the basic assumption is that young infants tend to represent space egocentrically, that is, based on their own bodies, rather than the objects' true positions in space, or an allocentric representation. For instance, infants who are trained at A and then moved around the table 180 degrees reach correctly to B at the B cue, which is still the A position from the baby's perspective (Bremner 1978a; 1978b; Bremner & Bryant 1977). Indeed, studies show that conditions that provide clues in the environment that help disambiguate the two identical targets, A and B, tend to decrease perseverative reaching. These include making the hiding covers more distinctive, adding landmarks in the room, or testing infants in more familiar environments (see Acredolo 1985; Wellman et al. 1986). In addition, there is a strong association between infants' experience in self-locomotion and correct responses (Acredolo 1985; Bell & Fox 1992; Bertenthal & Campos 1990; Kermoian & Campos 1988). Self-locomotion is believed to increase infants' visual attention to spatial locations and thereby their ability to code them allocentrically. The spatial hypothesis itself is not sufficient, however, because it neither accounts for the delay effect (why would babies' spatial coding change from allocentric to egocentric in the three-second delay?) nor for the looking-reaching decalage (allocentric when looking and egocentric when reaching?).

Another way to view the A-not-B task without invoking object permanence is as behavior requiring memory for a location and a motor response. In a series of influential papers, Diamond (1985; 1988; 1990a; 1990b) invoked these two processes to address two of the important context effects: the necessary delay between hiding and retrieval, and the ability of older infants to tolerate longer delays. Diamond proposed that two processes combine: the error results from infants' poor memories for the hiding place *and* their inability to inhibit strong motor responses. Having once reached to A, infants must inhibit this prepotent response in order to shift to the B place. The delay is important because in infants, both the memory for the hiding location and the ability to inhibit responses are short-lived. Thus, over the few second delay between hiding and retrieval both the memory of the hiding place *and* the ability to inhibit decline. At the B trial, this leads to a B reach with a short delay and a return to the old, prepotent A response with longer delays. With age, the persistence of these processes increases and infants do not err at short delays. The source of the developmental effect, according to Diamond, is maturational change in the dorsolateral prefrontal cortex, a brain area identified by lesion studies to be involved in both memory and response inhibition (Diamond & Goldman-Rakic 1989). This account, while powerful, is incomplete. It does not offer a principled explanation of the spatial location or context effects described above, nor why locomotion would hasten the decline of perseveration. And it cannot account for the looking-reaching decalage.

Thus, each of these accounts captures some truths about infant perseverative reaching, but none has a full explanation of both the canonical error, *and* the richly-documented effects of context which are part and parcel of the same phenomenon. In our theory, we incorporate aspects of many of the explanations of our predecessors. First, we agree with some of our colleagues that the A-not-B error is not about an object concept per se. Smith et al. (1999b) performed a critical experiment: infants were tested in the canonical task with one difference, there were no hidden objects at all. When simply cued to location A or B by waving the lids covering the wells, infants made the A-not-B error just as robustly as they did when objects were actually hidden and recovered. But we deeply disagree with the widely held assumptions that knowing and acting are modular and dissociable. Indeed the cornerstone of our dynamic model is that "knowing" *is* perceiving, moving, and remembering as they evolve over time, and that the error can be understood simply and completely in terms of these coupled processes.

Second, we further agree that the A-not-B error is about moving to a location in space. But it is *also* about remembering a cued location, and being unable to inhibit a previous response. What we will demonstrate with the model, however, is that there is no need to posit such individual and separate mechanisms such as egocentric or allocentric coding or memory or response inhibition deficits or incomplete object knowledge. Infants indeed *sometimes act* as though their responses are egocentric or as though they lack memory or inhibition. But they act that way because of the coupled interactions of the very same dynamic processes that make them appear to *sometimes "know"* where the object was hidden. Because all of the processes contributing to the behavior are coupled, continuous, and based in time, we can account in one model for both the error itself and for the decline in perserverative responding in different situations and at different ages.

## 2. A dynamic systems approach

The starting point of the dynamic model is with new assumptions. The A-not-B error is not about what infants *have* and *don't have* as enduring concepts, traits, or deficits, but what they *are doing* and *have done.* What they do is reach repeatedly to one location and then return to the original location when the goal has changed. From a dynamic perspective, this perseveration is emergent from the real-time dynamics of visually elicited reaching, the memory dynamics of repeating the same action several times in succession, and the intrinsic dynamics of these processes in infants. According to our view, the error arises from the same multiple processes that produce goal-directed reaching *at any age* and we indeed create the error using a model originally formulated to simulate the general process of motor planning for reaching. The age and context effects arise naturally from the parameters of the model, which in turn, we derive from a fine analysis of the actual task.

### 2.1. A task analysis

Thus, to begin, we describe the error task in the most simple behavioral terms. Then, we identify the factors in the task itself or in the baby that are known to affect the behavioral outcome, that is, the tendency to reach to A when cued at B. We report here the details of the canonical task


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

used in Smith et al. (1999b). There were two versions of this task, which differed only in whether a toy was actually hidden or whether there was no hidden object and the infant was cued simply with the cover to the hiding well (lids only). If this account seems burdened with details, it is because these details matter profoundly, as we will further document below.

For both the hidden toy and lids-only versions, the infant sat on a parent's lap at a small table facing the experimenter, surrounded by neutral and unmarked walls or screens. Here are the events that transpired in a typical A-side hiding trial. The infants first saw the hiding box, which was 30 cm by 23 cm by 5.5 cm. It was painted brown and contained two circular hiding wells, each with a radius of 4.5 cm and depth of 4.5 cm. The centers of the wells were 12.5 cm apart. The wells were each covered by a circular wooden lid with a small round handle in the middle, all painted the same brown color as the box. The box and the covers were in view throughout the whole procedure, and thus constituted a continuous visual *task input,* as depicted in Figure 1. The notable characteristic of the task input was its lack of visual specificity. The two lids were indistinguishable from each other and blended into the background of the box. No familiar or distinct environmental landmarks were evident. Once the two lids were in place, there was little else to demarcate them except their relative spatial position.

The trial began with the box well out of the infants' reach. The experimenter first called *attention* to the toy by waving it or tapping it on the box, always in the vicinity of the A side, and often calling the child's name. (All experiments were counterbalanced: half the infants had the A side on their right and half had the A side on their left.) When the infant was clearly looking at the toy, the experimenter hid the toy under the lid. This visual (and auditory) cue we call the *specific input,* a transitory indication of which lid is specified as the goal (Fig. 1). (In the lids-only condition, no toy was hidden and only the lid to the well was waved and tapped.) We then imposed a short *delay* of 3 sec for 8-month-olds and 5 sec for 10-month-olds. During the delay,

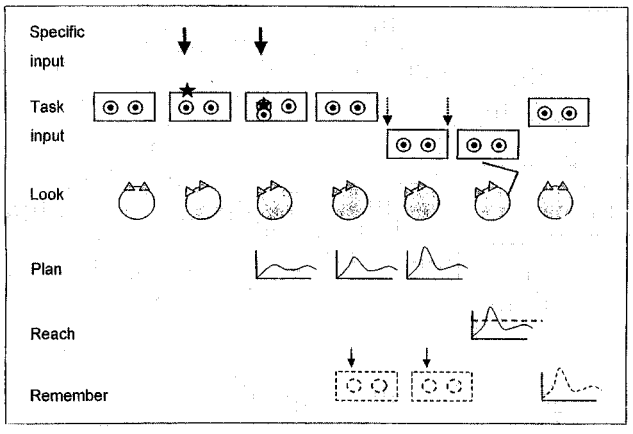



Figure 1. A task analysis of the canonical A-not-B error, depicting a typical A-side hiding event. The box and hiding wells constitute the continually present visual task input. The specific input comes from the transient cue of hiding the toy in the A well. A delay is imposed between hiding and allowing the infant to reach. During these events, the infant looks at the events, remembers the cued location and undertakes a mental planning process leading to activation of reach parameters, followed by reaching itself. Finally, the infant remembers the parameters of the current reach.

the infants most often *looked* at the cued location. Infrequently, they also glanced at the experimenter. During this delay, infants needed to *remember* the cued location in the face of the ambiguous task input. After the delay, the experimenter pushed the box into the infants' reach.

After infants saw the specific location cue, they needed to decide whether to reach to the A or to the B side, and *plan* the appropriate movement parameters to actually activate the muscles of their arms to go to the right or to the left. This planning was done largely in the absence of the well-specified goal. We have conceived this planning in Figure 1 as mental activation functions that precede the reach and are initially equally likely at A or B, but gradually shift to the A side. (We shall further justify this depiction below.) The *reach* itself was initiated once activation reached a certain threshold, sufficient to activate the muscles to move the arm in the specified direction. The infants continued to *look* at the A target throughout the planning and reaching.

Finally, the infants grasped the target toy or lid and shifted their glances back to the experimenter, who also withdrew the test box. A critical assumption on our part at this juncture is that the just executed reaching act was *remembered* for some unknown time after the reaching act was completed. Again, we will elaborate further on this memory process in a later section.

The A trials were *repeated* several times before infants were asked to switch to B for two trials. The events for the B trials were identical to those at the A side except that the experimenter hid the toy in or cued the previously unspecified location. Under these canonical conditions in both hiding and lids-only conditions, 70–80% of infants reached back to A on the first B trial, and an equal proportion continued to perseverate on the second B trial. This rate is significantly different from chance responding, that is, from an equal probability to go for either target after the B cue.

### 2.2. Events and processes in the A-not-B task

A foundational assumption of our theory and model is that both the error and correct reaching emerge from the coupled dynamics of looking, planning, reaching, and remembering within the particular context of the task: repeating a novel and confusing reaching action. Having described these events and processes in a canonical A-not-B trial, we now further analyze their contributions to the infants' behavior. It is important to note here that although we address these contributions one at a time, the point of our dynamic analysis is that they *are continuously coupled and interactive.* Everything counts! These detailed context and infant effects *are the data upon which the model is based.* We need not invoke any new constructs or traits.

**2.2.1. Task input.** As we mentioned earlier, the notable characteristic of the test scene was that the hiding box and wells were poorly specified as targets. Once the toy was hidden or the lid placed on the box, infants could easily confuse the targets, requiring them to decide on a reaching direction in the absence of well-marked locational cues. Indeed, there is ample evidence in the literature that perseverative responding depends on target ambiguity and that therefore, errors are reduced by manipulations that make the targets more distinct from one another (see Wellman et al. 1986). For instance, the error is decreased when the two hiding locations are visually very different, for ex-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

ample, when one cover is blue and the other cover is white (Butterworth et al. 1982). There is less clear evidence on the importance of the spatial separation of the targets, but for the most part, only relatively small differences in this distance have been manipulated (Sophian 1985; Wellman et al. 1986). However, in the study with the largest target separation, only 6 of 56 9-month-old infants made the A-not-B error, far less than would be expected in the canonical situation (Horobin & Acredolo 1986). Using multiple, rather than just two, hiding locations both reduces error and increases correct responding, meaning that infants return to the just cued location more than at just chance levels (Horobin & Acredolo 1986; Wellman et al. 1986). The effect of multiple targets as part of the task input may be to add spatial landmarks to the scene, giving the infant other relational cues for reach direction once the specific input is completed. Indeed, the addition of the familiar landmarks of infants' homes dramatically reduced egocentric responding (Acredolo 1979).

In addition to the A-not-B task being visually confusing for babies, it is also entirely novel. Infants have been grabbing objects for four or five months before they typically do the A-not-B test, but during this time, they have been reaching for well-specified, single, often colorful and highly distinct toys. Before this, no one has asked them to repeatedly choose between identical, closely spaced, boring items such as the covers to the hiding locations. Indeed, the task is so confusing and novel for infants that they have to be trained to do it. All A-not-B studies involve some, often unspecified, number of *training trials.* For example, in the canonical hidden object task used by Smith et al. (1999b) the training consisted of four trials in which infants were gradually led from a familiar task to the novel A-not-B test. To do this, the experimenter first placed the toy alone at the edge of the box at the A location and slid the box forward toward the infant, while verbally encouraging the infant to reach for the toy. This task was familiar and infants reached reliably. On the second training trial (A2), the toy was placed inside the well but left uncovered and on the third trial (A3), the toy was partially hidden in the well. In the last training trial (A4) the toy was completely hidden. In the lids-only version, the A-side lid was progressively moved back over the four trials from the front edge of the box to a position in line with the B side lid. The training was followed by two test trials at A with the toy completely hidden or with the lids lined up equidistant from the edge of the box and then the two B trials where the cue was switched, as described above.

Because the task input is novel and confusing, Smith et al. (1999b) predicted that without the training events to help disambiguate the A and B locations, infants would not be strongly disposed to reach to A *even on the first A trial.* They tested this by eliminating the four pre-training trials and commencing with only the two A test trials. With no training, only 35% of the infants reached correctly to A on the first A trial, compared to over 75% of infants who had been conventionally trained. Moreover, without training, a sizeable proportion of the infants did not reach at all, but just stared at the hiding wells, as though they could not figure out what to do, and they often refused to reach on the remaining trials as well.

In previous studies of A-not-B, training was justified as necessary to teach babies to reach for a hidden or unfamiliar object, and it does accomplish this. But this training, when done at the A location, also accustoms infants to repeatedly reach to A. In an effort to demonstrate these two effects, Smith et al. (1999b) trained infants for the typical A-not-B task, not at the two-welled test box, but at a different box with only a single, center well. They reasoned that, given the confusion of the targets and the fewer reaches to A, training at this single "C" location should render infants less likely to consistently reach to A on both A and B trials. Indeed, infants trained at the single well were *more* likely to reach to A on the A trials than babies with no pretraining. Simple practice with the hiding and recovery events in one location made the two-well condition less novel. But the "train at C" infants were also *less* likely to reliably reach to A on both the A and B trials than those babies given the standard training regime. Stable behavior at A and perseveration back to A depended on previously reaching to the A side. (We will discuss the effects of repetition further below.) Thus, the training trials, thought to be merely "warm-up" by previous investigators, are also critical contributors to the dynamics that produce the A-not-B error, both in helping disambiguate the confusing targets – affecting A-side behavior – *and* building up a tendency to stay at A when cued at B – affecting the appearance of the error.

Thus, target ambiguity alone is not the single cause of perseveration. But coupled with the cue, the delay, and the particular dynamics of infant reaching and remembering: *The relative ambiguity of the task input is a critical parameter in the model.*

**2.2.2. Specific input.** Given the largely tonic and confusing nature of the task input, infants receive transient clues to where to reach by the actions of the experimenter who waves and/or taps the toy or target lid. This activity invariably directed infants' visual attention to the target location, and they continued to look at that location throughout the hiding and reaching event (Smith et al. 1999b). The finding that waving the lid to the hiding well was as effective in producing perseverative reaching as was actually hiding a toy demonstrates that it is not hiding per se that is the critical stimulus. However, this does not mean that the nature of the specific input is unimportant. In our dynamic view, it is one of the critical events.

Recall that the specific input serves to demarcate the target location and then it disappears; the infant must remember it when it is no longer evident. Given that the specific input serves both to capture visual attention and to provide a spatial target in memory, it follows that some specific inputs may be more effective in either or both of these purposes than others. Some objects and events may be more interesting for babies and some may be more memorable than others. Specific inputs with more "punch," therefore, should increase correct responding, both on A and on B trials.

We found some support for this assumption in the literature. For instance, inspired by Smith et al.'s (1999b) demonstration of perseverative responding with no hidden objects, Munakata (1997) tried the following variant: she presented infants with lids only on the A trials, but on the B trials, either hid a toy at B or cued just the lid. In the lids-only condition, infants reached perseveratively at A, replicating Smith et al. (1999b). The error was decreased, however, in the condition where the toy was shown for the first time at B. Munakata (1997) interpreted this as infants representing lids-hiding-toys differently from lids with no toys.


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

Our interpretation of these interesting results is simpler: the novel and visually interesting toy captured visual attention more strongly than the lid alone and pulled infants away from their habitual response. This interpretation is further supported by Munakata's (1997) second experiment where, first, toys were hidden on A and then infants were cued with either toys or just lids on B. In this manipulation, infants made the error in both conditions. Of course, because both the lid and the toy were used on the A trials, neither the lid alone nor lid-plus-toy provided a sufficiently strong specific input to counteract the repeated reaches to A.

Our assumption that the strength of the specific cue makes a difference is further confirmed by experiments reported recently by Diamond (1997). Diamond coded infants' levels of interest in the hidden toys as they were hidden and then uncovered. Infants made significantly fewer A-not-B errors when the toy was different from that used on previous trials and when the infants' interest in that toy was high. More remarkable, when Diamond substituted pieces of cookie for hidden toys, all infants reached correctly, even at delays in which they had previously erred when toys were hidden.

Furthermore, when the "punch" of the specific input is enhanced by an event that increases infants' visual attention in one direction, it should be more potent in influencing reaching direction. Likewise, when the specific cue is diminished by competing claims on infants' visual attention, the power of that cue for directing the subsequent reach should be weakened. This is exactly what Smith et al. (1999b) found. In one experiment, these investigators manipulated the direction of infants' gaze after the A trials by simply tapping on a little blue rod placed either on the far right or the far left side of the testing arena. After four training trials, half the infants had their visual attention directed to the just trained A side and half to the upcoming B side. Reliably more infants in the A-side-tap condition reached to A on the A trials than those in the B-side-tap condition, and although there were no further taps, these infants also reached more to A on the B trials. In a second experiment, the researchers provided the pulls to visual attention before the B trials. In this case, infants given the additional cue to A all stayed at A, committing the error. Conversely, and in line with predictions, infants seeing the tap in the direction of B reached *correctly* on the B trials.

The critical feature, then, of the contribution of the specific cue is not whether it is a toy or a lid or a cookie or just an event to look at, but the power of the stimulus for capturing infants' attention and/or for remaining in memory when it is no longer in view. When the specific input is strong it will have a powerful influence; when it is weak, it may be swamped by the other system dynamics.

Thus: *The relative strength of the specific input is a second critical element in the model.*

**2.2.3. Delay.** Infants do not perseverate at any age if they are allowed to reach immediately after the object is hidden. The error emerges in the delay between the cessation of the specific input and when infants are permitted to act by sliding the box forward (Fig. 1). In addition, as they get older, infants require increasingly longer gaps before they perseverate. Eight-month-olds make the error with a 3 sec delay, but 10-month-olds require 5 sec to perseverate. The delay effect is robust (e.g., Diamond 1985; Harris 1973; Wellman et al. 1986) and is especially hard to reconcile with a strictly Piagetian interpretation of infants' stage-specific object knowledge since there is no reason why infants would know less after a longer delay, when they presumably have more time to process the situation.

What sorts of mechanisms can account for this apparent switch from right-to-wrong in 3 seconds? One class of explanations invokes a shift of level of processing within the delay period. For instance, Gratch et al. (1974) proposed that at no delay, infants' actions are guided by a motor memory of the most recent event, but at the longer delay, the concept of the hidden object kicks in, so infants return to the habitual hiding location. Harris (1973; 1987) agreed that the no-delay performance was dominated by a simple motor response, but also, at longer delays, infants had problems with object identity in that they believed the object hidden at B was not the same one that was previously hidden at A. In contrast, Wellman et al. (1986) saw the delay as revealing a conflict between two mechanisms for search. At short delays, infants relied on more immature, direct-search strategies: go where the object last disappeared. Longer delays, however, allowed activation of the conflicting, albeit more mature, strategy of an inferred search based on the movement of the object to its hiding position, leading to sometimes correct and sometimes errorful actions. Competing short and longer-term memories are also at the heart of the recent connectionist model of Munakata (1998).

As mentioned earlier, these accounts have several troubling aspects. Strategy-based theories are hard to reconcile with the age and delay interactions. If retrieving a correct strategy takes time, as infants get older and as they are given more time to retrieve a correct strategy (longer delay), they should be increasingly correct, but the reverse is true. Even more problematic, in our view, is the sharp distinction between knowledge (more mature, more conceptual) and action (more immature, less planful), especially in the face of compelling evidence that this "knowledge" comes and goes with each variant of the task.

Nonetheless, the heart of the explanation of the A-not-B task, we believe, lies in what happens during this delay when infants are faced with the ambiguous task input and yet must decide whether to go to A or to B based on a cue that is no longer present. Our model is similar to Diamond's well-known theory or to the recent connectionist model suggested by Munakata (1998), in postulating interacting dynamic processes that lead to one behavioral outcome at short delays and the probability of another outcome as time passes. But, specifically and uniquely, we place the locus of these dynamics in the motor planning process, envisioned as a continuous dynamic field that evolves under the influence of several input parameters, and whose behavior can be sampled during the delay.

In sum: *What happens during the delay is also critical as it is a window on the natural dynamics of the contributing processes.*

**2.2.4. Reaching.** Our focus on the A-not-B error as centered in the motor plan for reaching is well-supported by the developmental evidence. As we mentioned earlier, one of the most serious challenges for Piaget's original explanation was the discovery of the apparent decalage between infants' understanding of objects when reaching for them compared to when they were questioned by looking measures alone. Thus, even several months before they demon-



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strate reliable reaching errors, infants are not apparently confused about the last location of a hidden object when just looking. Likewise, 8–12 month-old infants tolerate a longer delay before erring in a visual violation-of-expectancy search task than when manually searching (Ahmed & Ruffman 1998). Moreover, when infants only watch the hiding events at A and B, they more often look correctly at B than they reach to B in the conventional search task (Hofstadter & Reznick 1996). Thus, at the very same age, infants show that they "know" more about hidden objects in the visual modality than with manual action.

The apparent confusion of what infants "really know" in the wake of such conflicting stories is only an issue when the foundational assumption is that infants "really know" something in the absence of the processes that demonstrate, in the moment, what they do in light of what they have just done. As argued in Smith et al. (1999b), the A-not-B error is about the behavior of reaching in the context of a particular perceptual scene, specific task dynamics, and dynamics of reaching and remembering intrinsic to infants at particular stages of development. Perseveration or the lack thereof in *looking* tasks has its own contributing dynamics, which *may or may not* produce the same behavioral outcome (and clearly does not). Neither reaching nor looking is a better measure of the infant mind; both are very revealing windows into the complex coupled dynamics that produce goal-directed behavior. But whether infants do better at looking than reaching is somewhat of a side issue for our present goal, which is to elucidate the dynamics of the reaching A-not-B error. Once these dynamics are understood, a similar analysis can be applied to looking tasks and comparisons made.

At the heart of our model, therefore, is the act of *reaching*, which requires that infants see and remember a target location as a goal, that they plan the appropriate movement parameters for a trajectory in space and time that will transport their hands to the desired object, and that they activate muscles that will carry out their intended movement. These processes are perceptual *and* they are motor *and* they are cognitive. Indeed, the model demonstrates very clearly the impossibility of making clear distinctions between these processes as they evolve in coupled and parallel fashion over the time of each trial and the time of the whole experiment.

In particular, we focus on the evolution of the reaching plan between the time the target is cued and infants actually move their arms forward to pick up the toy or lid. This plan, we contend, is the locus of both the error and correct responding because it integrates the perceptual input of both the tonic task conditions, the cuing of the location with toy or lid, the infants' visual attention to that cue, and the memory of previous actions in the same situation. The combined dynamics of these processes constitute the infants' *decisions* whether to move toward the A side or toward the B side. All of these are expressed in movement parameters because the ultimate behavior is an action requiring a correct movement. The fact that the visual, memory, and motor processes are coupled and continuous means that changes in any of their parameters can (and often does) affect whether infants reach correctly or perseveratively. Again, we will justify this feature of the model in more detail in the next section.

Thus, *the error emerges in the context of the specific behavior of reaching.*

**2.2.5 Remembering.** In an earlier section, we presented evidence that infants reliably produced the A-not-B error only after a number of training trials which both served to disambiguate the targets and to establish a repeated pattern of reaching to the A side. Indeed, in every version of the task reported, experimenters elicit the error only after several reaches to the A location. Nonetheless, in their comprehensive meta-analysis, Wellman et al. (1986) found no overall effect of the number of A reaches on the commission of the error. Does repetition matter? This is a critical question for understanding the task dynamics, and we believe the answer is unequivocally yes – they matter profoundly.

In a number of recent studies, Smith, Thelen, and their associates have shown conclusively that commission of location errors with the B cue is strictly a function of the number of prior reaches to A. *The strongest influence on where infants will reach on any trial is where they have just reached.* This is true whether the target objects are hidden toys or just the lids.

As we described earlier, perseverative errors decreased when infants were not trained at all or trained at a neutral location. Both of these manipulations simply reduced the number of times infants have reached to A (Smith et al. 1999b). Furthermore, infants tended to perseverate even when no specific cue was offered. In a recent experiment, Smith et al. (1999b) gave groups of infants six trials in the lids-only task. One group was cued on one side before they reached at all and then allowed to reach spontaneously. These babies went to the cued side on their first reach and tended to stay there. The other groups were allowed to reach spontaneously on the first trial and then they were cued once, either after 1, 3, or 5 reaches. The cue was always opposite of infants' preferred side; a "B" trial to infants' personal "A" choices. The infants who were perturbed by a B trial after reaching five times to their A choices stayed at the A side. Without any hidden objects or even location cues, infants built a habit to go to A strong enough to counteract the new pull on visual attention. Infants perturbed after one or three reaches, in contrast, were more likely to switch to B when cued, and to switch sides more spontaneously thereafter.

In addition, in several studies where other parameters of the A-not-B task were manipulated, including aspects of the specific and task input as well as the dynamics of the reaching arm, the Smith et al. group always found a strong effect of the number of trials to A on whether infants stayed at A or switched. This effect could be detected because, as noted by previous researchers, infants' tendencies to go to A or B are never absolute, but probabilistic. That is, on any given cue to A, even the very first one, there is a chance that, spontaneously, infants will go to B. Even after several trials to A, when the probability to stick with A is high, a few infants will go to B.

Diedrich et al. (2001) captured this effect of spontaneous switches by a new measure of the growing effect of previous reaches – the relative memory strength to A. The index is predicated on the simple assumption that each reach in one direction creates a memory trace that increases the likelihood of subsequent reaches in that direction. Thus, each time an infant reaches to A or to B, the memory of that target increases. By subtracting the memory of B from the memory of A, we can express the probability the infant will return to A. The maximum memory strength for any direction is 1 (8/8), meaning that the infant will have


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

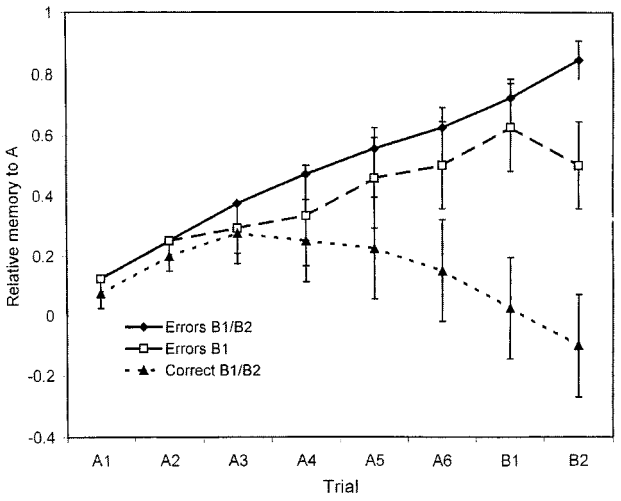



Figure 2. The effects of repeated reaching on the A-not-B error. The measure " Relative memory strength to A" (explained in text) for a canonical no-hidden object task in 9-month-old infants. Shown for infants who made the error on both B trials (n = 9), on only the first B trial (n = 3) and who were correct on both B trials (n = 5). Correct B trial responding was preceded by spontaneous reaches to B even when A was cued.

reached to that side in, say, all 8 of 8 trials. Likewise, the minimum memory strength is 0/8, when the baby never reached to that direction. Figure 2 shows the evolving memory strength to A in a canonical two-target, no-hidden object task. The infants in this study were divided into those that perseverated on both B trials and those that got at least one B trial correct. Note that, from the earliest trials, the infants who were more likely correct at B were those who, spontaneously, had reached previously to B even when cued to A, while those who strongly perseverated had stuck with A throughout.

How are these results reconciled with the conclusions of Wellman et al. (1986) that "number of A trials was consistently unrelated to performance" (p. 31)? As mentioned previously, in all A-not-B studies, experimenters train infants to do the hidden object task, which may involve some number of reaches to the A side. These training trials were thought to be prelude to the task, and were not included in the actual count of repetitions to A. Infants, thus, may have actually reached to the A location three or four times before the first official A trial is counted. Furthermore, in some versions of the task, infants are cued to the A side until they reach a criterion of two or three successive "correct" reaches to A and then switched to B (e.g., Bell & Fox 1992; Diamond 1985). "Correct" in these tasks means not just touching the cover, but lifting the cover and touching the hidden toy. In this version of the procedure, therefore, the actual number of reaches to A before the switch is not reported, and is unknown. An infant may make several "incorrect" reaches to the A side, or several "correct" reaches but not in succession. Without counting training and/or actual movements in the A direction, the full range of parametric effects for repetition was therefore not obtained, and indeed cannot be ascertained from the information provided in the published reports (Diedrich et al. 2001). When these parameters are fully explored as in Smith et al. (1999b), the effect of repeating movements is overwhelmingly strong. Indeed, a recently published new meta-analysis of A-not-B reaches the identical conclusion: that perseveration is a function of the number of A-side reaches (Markovitch & Zelazo 1999).

That the direction of reaching depends critically on where infants just previously reached means that the A-not-B task is a memory task on two time scales. First, it requires that infants remember the location of the specified target in its absence, as discussed above. Second, there must be memory dynamics between the time when the reach is executed in one trial and the sight of the target and decision to reach on the succeeding trial. We suggest that this time between retrieval trials was also of critical importance because infants *remember* the actions they just performed during the time between recovering the toy and the next specific cue. Thus, when the second cycle of cuing, deciding and reaching commences, it is initiated within the time span of the memory of the previous cycle. The third cycle builds upon the memory of the first two and so on, so that when the baby is finally cued to B the memory traces from the first six reaches to the A side may be very strong. This memory is a motor memory whose content can be examined, at least in part. Critical to the model, therefore, is that the memory of one action is in the same space of movement parameters as the plan for the subsequent actions and can thus influence the evolution of the next movement. Again, we substantiate these claims in a later section.

*Infants make perseverative location errors because the motor memory of one reach persists and influences subsequent reaches.*

**2.2.6. Development.** In the canonical form of the task, only infants between 7 and 12 months of age consistently make the error. Before this age span, infants do not search at all for hidden objects, and after a year, infants search successfully where they saw the object last disappear, even after it has been displaced several times. Thus, in traditional interpretations, perseveration is seen as a distinct stage in cognitive development: that of incomplete object representation.

It is incorrect, however, to assume that perseverative reaching responses are unique to a particular stage in infancy. Try moving an article in your kitchen from a long-established location and start cooking dinner! Indeed, adults with no perceptual or motor impairments can be trained into perseverative or biased responding within an experimental session in the laboratory (e.g., Ghilardi et al. 1995). Especially relevant is a recent study where Smith et al. (1999a) elicited strong perseverative reaching responses in 2-year-olds, at an age at which the Piagetian object concept should be strongly established. These authors asked toddlers to retrieve a toy hidden in a long narrow sandbox , a task that differed from the classic A-not-B exercise primarily in the less well-delineated targets. (Once the toy was hidden in the sand, no covers to the hiding places helped mark the possible locations.) After several recoveries from the A side, toddlers continued to search in the vicinity of the A location even after the toy was hidden on the other side.

We contend here that the processes that create perseverative responding in infancy are not special, but are the very same processes that lead to correct responding at this age, and also to correct and perseverative responding in individuals at any age. Nonetheless, it is incumbent upon us



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

to explain which aspects of the processes can account for the developmental effects: why location errors occur at particular ages and under certain conditions and not at others.

We believe that all of the above-mentioned contexts and parameters contribute in coupled and perhaps nonlinear ways, and that they all may have age- and experience-specific dynamics. In short, the age differences reside in the particulars of the environment and timing demands of the tasks. Consider for instance, the problem infants face in distinguishing the two identical lids. There are good developmental reasons for infants to have difficulties at say, 9 months, and to improve their abilities in this regard by 12 months, but still to be confused at 24 months when the task becomes more ambiguous. In the last part of the first year, infants have limited experience with spatial localization, especially before they have moved themselves around. Understandably, self-locomotion may focus infants' attention on where things are in the environment because these objects and places become relevant in ways they were not when babies are still transported by others (Horobin & Acredolo 1986). Increasing visual attention alone may change the impact of the various manipulations of the task. In addition, infants may learn to pay attention to the relationships of objects in the environment to each other, and thus form a better conception of right side versus left side when faced with an otherwise ambiguous visual scene. These experiential effects are co-evolving with infants' increasing skill at reaching. As infants move about by walking, cruising, and crawling, they reach for different items, at different levels and locations, and from a variety of postures. Thus, their action capabilities expand and become more flexible. At the same time, we may suppose, these interrelated perception-action experiences also impact memory processes, as babies now have reasons to remember where things once were when they are presently out of sight. However, and what is critical here, is that these changes are gradual and not all-or-none: given good reaching skills and adept use of landmarks, toddlers and even adults may still become confused when the target locations are transient or not well-marked or when the delay between target cue and go signal for reaching is extended.

Given these multiply-determined, dynamic, and cascading effects, the power of the model is to offer entry points for actually probing how the process parameters may change over developmental time. As a starting assumption, we focus on one of the likely sites for developmental change, the mental processes involved in the motor planning and decision to reach to either the A or B side. We will spend considerable time in the next sections justifying why it is appropriate to express the evolution of the A-not-B error dynamics in terms of a motor field, and therefore, how developmental events may impact upon the behavior of that field under certain task conditions.

Thus, and finally, *age-related changes in the likelihood of perseverative reaching may result from parameter changes in multiple contributing processes; one candidate is a change in the properties of the integrative motor planning process.*

## 3. A dynamic field model: Overview and rationale

Previous accounts have assigned the cause of the A-not-B error to infants' deficits in object knowledge, spatial localization, memory, or inhibition. In contrast, we center our attention on reaching, and in particular, the processes that lead to a directional reach to A or B. The challenge is to explain, therefore, in terms of the normal processes involved in reaching, behavioral phenomena that *look* like infants really do have problems with object permanence, *and* that they cannot escape their body-centered understanding of space, *and* that there is something lacking in their memories or inhibitory mechanisms. Thus, our formulation is justified as far as we can show that the well-documented context- and age-effects described above can be manifest in the domain of the action of reaching.

The model describes the mental events that constitute the decision to reach to A or B as activations in a dynamic field expressed as directional movement parameters (Erlhagen & Schöner 2001; Schöner et al. 1997). The field, which has nonlinear properties, evolves continuously under the influence of input dynamics from three sources, also expressed in compatible directional parameters. These include the specifications of the task environment, which establishes the decision field, the specific cue to reach to A or B, which is transient and must be remembered, and, after the first reach, a memory dynamics which biases the field for the subsequent reach. Both the properties of the field itself and the input dynamics can be assigned parameters that are derived from data, and we simulate, using such parameters, the robust effects documented in the literature. We also use the model to generate novel predictions.

Note here three critical aspects of the model. First, although we maintain that the model is biologically plausible, and discuss this further below, it is an entirely abstract model, and not a neuroanatomically specific account of looking, reaching, and remembering. In its present form, the model also does not incorporate specific bodily parameters such as muscle anatomy, segment masses, and centers of gravity, or joint configurations. Rather, the model captures the abstract, collective dynamics of multiple processes, which are likely happening in parallel in many integrated sites in the brain and the body. Second, and related, the concrete mathematical functional form of the model is not unique (see for instance, Grossberg 1980; Wilson & Cowan 1973). What is important here are the assumptions of blended, continuous performance and how closely these assumptions both match the previous experimental results and generate good, testable new experiments. That this precise formulation works, validates our assumptions but does not preclude other theoretical instantiations using similar assumptions. Third, we put forth this model as only the first step toward a fully embodied account by casting the mental events that precede the movement into the same dynamic language appropriate for movement itself: continuous and time-based. The important step of integrating the actual movement dynamics with the motor plan dynamics remains to be done. It is a difficult challenge, given the complex nature of the coordination and control of limb movements (see Bullock & Grossberg 1988; Erlhagen & Schöner 2001; Houk et al. 1995; Jordan 1990; Kopecz & Schöner 1995, for neural network accounts of the motor control problems).

### 3.1. Integration in a motor planning field

Reaching in infants, like reaching in adults, begins when individuals see objects they want. We know from a vast liter-


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ature that 150–300 msec elapse between the visual fixation of the target or some other "go" signal and the actual movement of the arm and hand toward it. During this time, conventionally called the reaction time, persons are processing the visual stimuli, effecting some sort of transformation from visual space to body-space, and establishing the specific movement parameters for the execution of a movement that will attain the goal. After this "motor program" is set, the traditional explanation goes, then the reach is triggered and the actual movement occurs. In terms of the cartoon of Figure 1, the series of events can be imagined as the diagonal row of icons from left to right: look, plan, reach.

The first part of the process involving attention, vision, and planning are the mental or cognitive events usually studied by psychologists. The prevailing theoretical frame of reference has been that of information processing: the vocabulary revolves around notions of programs, codes, and representations, also processes such as feature extraction, response choice, and serial stages. These mental constructs are accessed through manipulating the attentional demands of the task, the nature of the stimuli or the memory load, and by looking at reaction times and error rates. Investigators cared about mental events; the movement itself was of little or no concern. In contrast, movement scientists have been more focused on the actual control and execution of the reach itself. In this case, they focused on kinematic variables such as trajectories and velocities, and the biomechanical and neuromuscular contributions to the transport of a physical entity – a real limb – in space and time. They might vary the load on the limb, speed and accuracy requirements, work space demands, postures, and so on, and measure actual movements, forces, and muscle patterns. People plan in order to move; yet, in the traditional formulations, there was no common currency between the activities of the mind and those of the limb (for discussions see Allport 1987; Georgopolous 1986; Hommel 1996; Keele 1981; Poulton 1981; Prinz 1997; Rosenbaum et al. 1995; Schöner 1995).

Despite this historical dualism, we often recognize that the processes of action planning and execution cannot, in reality, be so neatly parceled and expressed in such incommensurate dimensions. Empirical advances over the last decade or so have gone far in breaking down the strict serial processing assumptions of "first preparation, then execution," in favor of much more parallel, mutually influential mechanisms. There is evidence, for instance, that the visual input is important not just at the start of the process, but is continuously and intimately influential throughout the planning and execution of the reach trajectory. And that adults are proficient at producing on-line corrections to movements, indicating that the programming process is an ongoing dynamic, not rigidly fixed by the initial target specifications (see, for instance, Goodale et al. 1986; Prablanc & Martin1992). Additionally, perception itself is not isolated from action: the very act of perceiving is enmeshed with actions that accompany it (Muessler & Hommel 1997). Indeed, we believe that it is just this amalgam of processes that gives rise to infant perseveration.

Because we take the unusual stance that the A-not-B error arises from a motor planning process that is part of a dynamic perception-action loop, we take some time at this point to substantiate this fundamental assumption. Specifically, we give evidence for (1) actions are planned in movement parameter space; (2) the plans are continuous and graded in nature; (3) plans evolve under continuous perceptual influence of both task and cue; and (4) the system has history.

**3.1.1. Actions are planned in movement parameters.** The most compelling and direct evidence that what has been traditionally called "cognition" can be effected in movement parameters comes from the pioneering work of Apostolos Georgopoulos and his colleagues (for reviews, see Georgopoulos 1986; 1990; 1991; 1995). In their now classic experiments, these investigators trained monkeys to reach for different targets in space while they recorded simultaneously from many neurons in the motor cortex. They discovered populations of neurons that together code for the direction of movement. This code is a product of an ensemble of neurons, each of which is only broadly directionally tuned. Together, however, they provide a unique population vector that points in the direction of the movement to the target. There are several aspects of these findings that are especially relevant for our model. First, the code is body-centered: the population vector points in the direction of the target despite the monkey starting from various positions. Visual and body information are in the same parameters. Second, the vector emerges gradually and continuously in the planning period between cue and reach. As illustrated in Figure 3 (top panel), in the approximately 150 msec between the presentation of the target and the actual start of the movement (indicated by the velocity vectors of the movement) neuronal activity gradually builds and predicts the upcoming movement. Third, the vectors mirror the direction of the velocity of path of the hand, suggesting that the population vector carries information about the instantaneous velocity of the hand. Again, this is strong evidence of the compatibility and continuity of the planning/action code. Fourth, the population vector predicts the direction of reaching during a delay. In a good analogue of the infant A-not-B task, researchers presented the target briefly, then instructed the monkey not to move until a cue was given 450–750 msec later.

Figure 3 (middle panel) shows the vectors in the direction of the cued signal held in memory during the delay, again as a continuous and graded signal. Indeed, when the light was not turned off during the delay, requiring no memory, the vector length is somewhat decreased (Smyrnis et al. 1992). Finally, specific task-related cognitive manipulations learned by the monkeys can be detected in the evolution of the directional vectors. In one such study, Georgopolous et al. trained a monkey to sometimes move a handle 90 degrees and counterclockwise from a reference direction and sometimes move directly to it. During the reaction time – before actual movement – this directional rotation could be detected in the population vectors (Fig. 3, bottom panel), indicating that the animal was performing a mental rotation from the accustomed to the novel motor direction. Again, this rotation was gradual and continuous and involved activation of cells tuned not just to the stimulus and rotated direction, but those in the directions passed through during the rotation. This, according to Georgopoulos, "provided for the first time a direct visualization of a dynamic cognitive process" (1992, p. 514). Similarly, in a recent paper, Kettner et al. (1996) showed that in monkeys trained to move to two locations in a sequence, activation of both sets of directionally specific neurons – to the first and then the succeeding movement – could be de-



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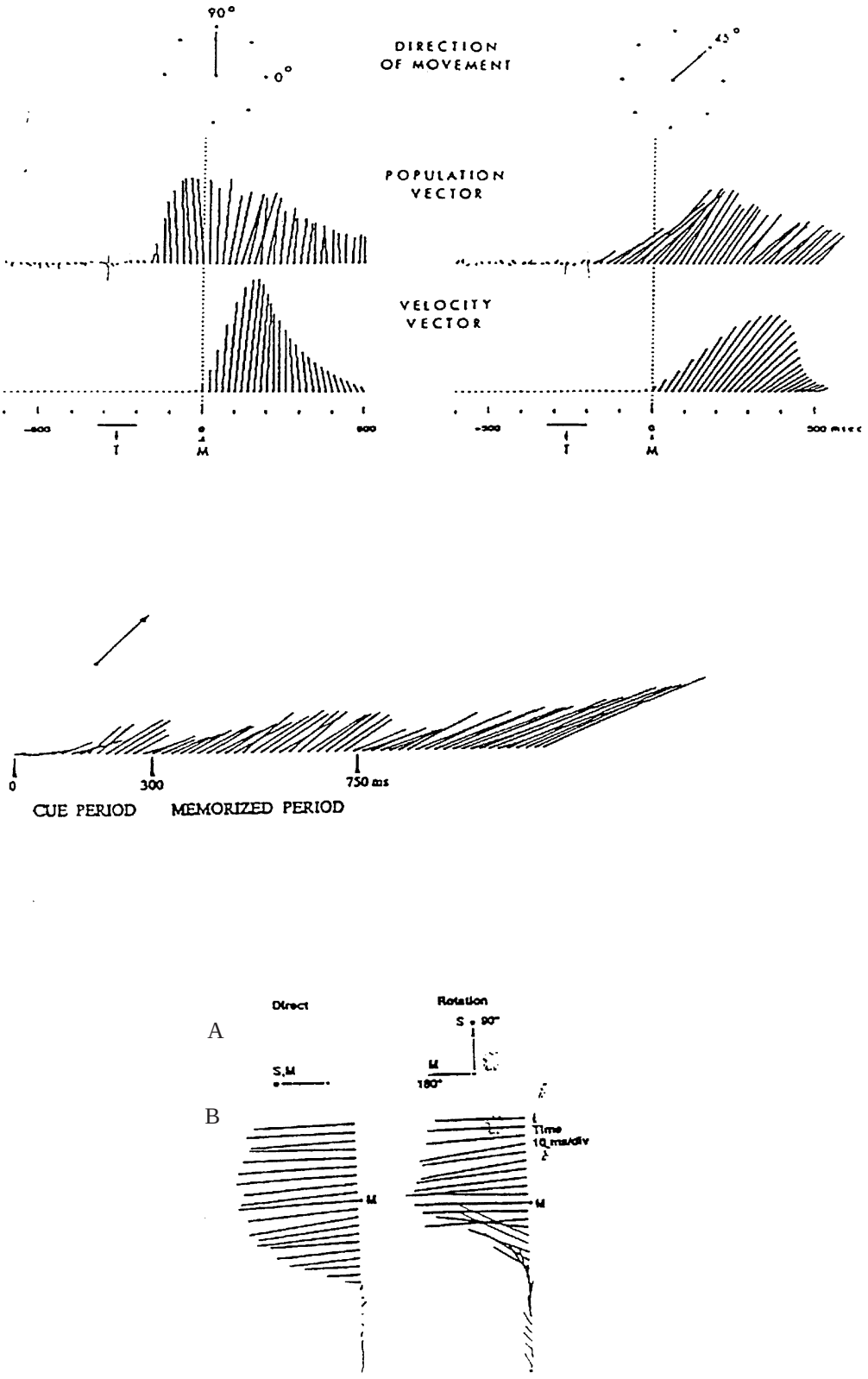



Figure 3. Top panel: Cortical population vectors point in the direction of the movement before the movement begins. Middle panel: Population vectors in the direction of the cued signal are held in memory during the delay. Bottom panel: Rotational vector detected during the reaction time when animal was performing a mental rotation. **A:** Rotation task showing direction of stimulus (S) and movement (M). **B:** Neuronal population vectors calculated from the onset of S until the onset of M. (Georgopoulos 1995).


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tected in motor and premotor cortex, again before the movement actually began.

The A-not-B error is an error of reach direction. Directional vector coding has been described in several areas of the brain, including primary motor cortex (Georgopoulos et al. 1988), dorsal premotor cortex (Caminiti et al. 1991), areas 2 and 5 of the parietal cortex (Kalaska et al. 1983), and in the cerebellum (Fortier et al. 1989). Moreover, in several of these areas, the neural response dynamics are congruent with the visual-motor integration we propose in the model, as we discuss further in a later section (Pellegrino & Wise 1993). Thus, it is tempting to conceptualize activation in the model to be direction-specific. However, there is also considerable debate among motor physiologists over which movement parameters are actually encoded in the brain. For instance, researchers have discovered neurons in motor, premotor, and parietal cortex that are specific to a body-centered code, and change with the postures of the arm, before, during, and after the movement (e.g., Caminiti et al. 1991; Lacquaniti et al. 1995; Scott & Kalaska 1997; Scott et al. 1997). It is possible and likely that the CNS uses multiple frames of reference. The precise coordinates of the movement parameters are not critical for our model, however, as the activation in parameter space is purely abstract, and could therefore be topographic in respect to body segments as easily as metric in respect to extrinsic coordinates.

**3.1.2. The plans are continuous and graded in nature.** The analogue character of motor planning is clearly manifest in the direct recordings from monkey cortex. But there is similarly compelling evidence of the continuous and graded nature of this process from behavioral studies of adult humans done by Claude Ghez and his collaborators (Hening et al. 1988a; 1988b). Their task, like the one used in the monkey studies, is similar to the infant A-not-B in that the target was ambiguous and that there was delay between preparation and execution. Participants were asked to match forces generated by isometric muscle contractions to one of three target amplitudes shown on a computer screen (think of contracting your biceps with your arm held still – a little, some more, and a lot). The experimenters instructed the participants to respond on the last of four successive, equally-spaced tones. The manipulation was that the target specification flashed on the screen at varying intervals between the third and fourth tone (Fig. 4). When the interval between target and go signal was long, individuals had a long time to prepare their response. When the interval was short, participants had to respond presumably before the planning process was complete. Indeed, what the Ghez group found was that the distribution of responses varied systematically with the time available for preparation. Figure 4 also illustrates these results. At short intervals and without knowledge of the upcoming target, individuals responded with a middle, "default" response. Gradually, as the time between target and response signal increased, these force amplitudes shifted toward the target values. (In contrast, when participants knew well ahead what their target would be, this interval did not matter.) By this clever experiment compelling participants to move before they were "really ready," these researchers sampled the response preparation time and showed that the planning gradually evolves – it is not an all-or-nothing trigger for response initiation. Equally remarkably, and also relevant, is that the

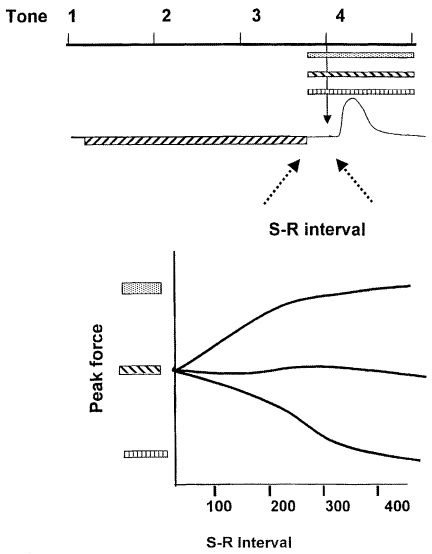



Figure 4. The effect of response preparation time on target accuracy. Top panel: participants were given variable times between the appearance of the target for an isometric force amplitude (three levels of force, indicated by the filled bars in the top panel) and the "go" signal indicated by the fourth of 4 tones spaced 500 msec apart (S-R interval). The diagonally striped bar is the center or default value, which is what the participants prepare ahead of the specific target cue. The effect is shown on the bottom panel. When the S-R interval is short, participants respond with the middle default response. Target accuracy increased continuously with increased planning time. (Redrawn from Henning et al. 1988a).

default response, the middle amplitude, was prepared in advance, presumably specified from prior trials in the task.

Together, the Ghez and Georgopoulos studies point to planning for action as specified in movement parameters and as having real dynamics; that is, a time course of activation and decay. The dynamics can be viewed directly by neural recording or indirectly through behavioral manipulations. We contend that the A-not-B error is, much like the Ghez studies, a window on these planning dynamics, with the ambiguous targets and delays providing the necessary manipulations.

**3.1.3. Plans evolve under continuous perceptual specification of task and cue.** Having established the likelihood that the plan for action is expressed in the same dynamic variables that control action – that planning and reaching are meshed and continuous – we now add the third parallel channel, looking. There is a large and growing body of evidence that visual behavior is intimately linked to every aspect of the reaching task, and therefore, that we are justified in assimilating the visual dynamics into the reaching field. Looking affects reaching and reaching affects looking.

Consider the following adult experiments which link direction of eye movements with motor responses. Fisk and Goodale (1985) asked participants to reach with right and left hands to targets that were both ipsilateral and contralateral to the hand used. Latency to reach onset was shorter when participants moved to targets on the same side as the reaching hand and also when using their dominant hand. This effect was precisely echoed in their eye movements. Although eyes moved toward the targets 50 msec sooner than hands did, eye movements latencies were



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also decreased when the *hand* was reaching to the ipsilateral side and/or was the preferred hand. Now there are reasons to expect that neuromuscular and biomechanical factors would make it easier to prepare movements on the same side or with the accustomed limb. Eye movements to the right or left, however, if independently specified, should be equally facilitated; there should be no biases in the system. But in the dynamics of this task, there are. One conclusion is likely:

> The relationship between ocular and manual latencies suggests that production of these two motor responses is far from independent and that programming within the two systems must be integrated (at least temporally) at some level of the central nervous system. (Fisk & Goodale 1985, p. 170)

A second study of looking and reaching closely parallels the A-not-B task: the target is only briefly presented and must be remembered and the task situation provides ambiguous cues. This experiment, conducted by Enright (1995), demonstrates that momentary changes in gaze affect reaching accuracy. Participants initially fixated a central target. After a few seconds, a peripheral target lit up and then was turned off. The central target remained on for another 2 sec – the delay – and when it went off, participants had to reach toward the remembered target. Three different conditions were used. In one, individuals kept their gaze on the central focus and pointed in the dark. In the second, they were instructed to look at the missing target immediately after the fixation light went off, and in the third, to shift their gaze to the target during the time it was lit and to keep it there. Participants had more accurate aiming when they shifted their eyes toward the target, whether the target was visible or not. It is not surprising that people would be better at aiming when they immediately shifted their gaze and held it at the target. But the difference between those who kept their gaze at midline and those who shifted just as they reached suggested that it was the eye orientation *during the pointing process* itself (even though the target was memorized and not visible in either case) that had the impact on the movement outcome. This must mean, according to the author, that information about the direction of the gaze and not the visual information per se, combines with the spatial specification of the upcoming movement which, in this case, is stored in memory. Where one looks matters in how one reaches.

These experiments showed the ongoing influence of looking at the specific targets on reaching, but other studies also show that even seemingly irrelevant distractors in the visual field perturb reaching performance, and most important, that the disturbance is action-centered. These experiments are pertinent because the A-not-B task can be seen as one where the target (the cued location) is made ambiguous by distractors in the field. Tipper et al. (1992) presented adults with a $3 \times 3$ matrix of buttons on a sloped board. Buttons in the middle row, when lit by a red light, were the targets and those in the front and back rows, when flashed yellow, acted as distractors in some trials. Latency to reach was consistently longer when distractors were present, but asymmetrically so. Distractors interfered more with the reach plan when they were below and in front of the targets, and thus acted as visual perturbations to the path of the hand, than when they were above and behind them, out of the hand path. In addition, and consistent with the Fisk and Goodale studies described above, right-handed participants were slower when the distractor was on the right side, again in the path of hand. The visual pull for attention was again manifest in motor dimensions.

Another particularly clear demonstration of the influence of the entire visual task environment, in interaction with the target dynamics, comes from a study by Jackson et al. (1995). These authors asked participants to reach for a wooden block, sometimes in the presence of a wooden dowel placed either midline or peripheral to the target. They found a significant distortion of both reach and grasp kinematics when the distractors were in the visual field, but only in the condition when participants closed their eyes. There was an effect of flanker position as well, whereby distractors at midline affected only those reaches that crossed midline and those at the periphery distorted reaches to the ipsilateral side. The effect of vision is especially relevant: when the targets were continually in view, the authors suggested, participants had time to attentively select the correct target and prepare motor response. When eyes were closed, however, the target location had to be remembered for about 2 seconds. As in the A-not-B task, during this gap, memory of the distractors must have influenced the memory of the specified target location – in reach kinematic parameters.

One further line of evidence of the reach-look synergy comes from studies where limb proprioception is manipulated as well as vision of limb and target. For example, when participants were instructed to move to either visible or memorized targets without seeing their limbs, the directional biases in target accuracy depended on how far the hand starting position was from the middle of the body (Desmurget et al. 1995; Ghez et al. 1995; Ghilardi et al. 1995; Prablanc et al. 1979; Rossetti et al. 1994, among others). When both the hand and the target were visible, no such biases appeared. Presumably, given the relatively less practiced task of reaching from either one side or the other, people needed to update their hand-to-target motor plan with additional visual information about the position of their limbs.

**3.1.4. The system has history.** The A-not-B task is about looking and reaching, but it is also about remembering on two time scales. The target position must be remembered during the reach. But also critical to our model assumptions is that the memory of one just-completed reach is retained to bias subsequent movements. As documented in the previous section, this memory likely includes the entire task set-up, which "pre-shapes" and biases the field in the same movement parameters as the reach plan (see also Mitz et al. 1991; Spencer & Hund, submitted). But we also propose that the motor memory of the just-completed movement is also retained and integrated into the next plan.There is evidence for this assumption in the adult literature. For example, in the Ghilardi et al. (1995) study described earlier, participants showed biased directional errors when reaching from unusual starting positions such as the far right or left of their bodies. With extensive training in one area of this novel work space, say to the right or to the left, these biases were eliminated. However, the repeated movements in the trained area then skewed performance in the untrained areas toward the trained position, even creating systematic errors in midline where none were found before the experimental training. Directional accuracy, therefore, was a direct consequence of repeated moving in that task. When people are accustomed to reaching from a middle


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position, they make errors in other areas of the workspace. This visual-motor map must be highly dynamic however, as it could be changed through altered experience. The system retains a memory of previous movements that incorporates the feel of the arm in relation to the target and uses the memory to plan future responses.

Equally dramatic evidence of these movement field memories was provided by studies of Shadmehr and Mussi-Ivaldi (1994) (see also Lackner & DiZio 1994), who created novel reaching conditions by subjecting participants to an artificial force field. At first, participants could not make straight reaches to a target; when they tried their habitual reach parameters, the novel forces created curved trajectories. With extensive training, however, participants learned to adjust their arm dynamics to the new environment, and performed straight reaches again. When the force field was then unexpectedly turned off, the learned adaptation remained, and participants now produced curved movements, as though they were still compensating for the unusual field. These "after-effects" again demonstrated that, even in adults, repetition of an action changes subsequent movements, indicating that the system retains a memory for the movement parameters from trial to trial.

### 3.2. Motor plans in infants

Are the fundamental dynamics that produce these coupled interactions of looking, reaching, and remembering in adults the same as those that lead to perseverative reaching in infants? Although direct behavioral evidence is sparse, a few recent experiments increase our confidence that model assumptions chosen from adult studies are also good for infants.

We discuss first the issue of looking and reaching, and especially that the direction of the gaze is mutually coupled to the direction of movement. This was directly tested by Smith et al. (1999b) using the canonical A-not-B hiding, but adding a simple manipulation of the direction of visual attention, as we described earlier. Infants whose visual attention was pulled in the direction of their original movement training stuck with their ongoing motor habit; conversely, infants whose glances were in the opposite direction of their movements were more likely to switch to the new target. In infants, as well as adults, goal-directed reaching is coupled to the direction of visual attention.

Second, infants code reach direction in a manner that incorporates both postural and trajectory information, as suggested by the neurophysiological evidence in primates mentioned previously. Furthermore, these parameters are held in memory and influence subsequent reaches. For instance, in an another experiment, Smith et al. (1999b) reasoned that if the memory built up of repeated reaches to A is based specifically on the position of the hand and arm in relation to the target, then shifts of posture that disturb the remembered hand-target trace should also disrupt the perseverative pull to A, much like a glance in the B direction competes with the activity pulling the baby to A. To test this idea, the researchers gave infants the standard hidden object A-not-B task. For the training trials and A-side test trials infants sat on a parent's lap, as is customary. However, between trials A2 and B1, parents stood their infants up so that the baby had to reach down to uncover the toy. Control group infants were distracted visually with a colorful, noisy toy shown to them at midline. Infants who saw the centered visual distraction perseverated when cued at B. However, when their posture was shifted, infants' tendencies to return to A were dramatically reduced. Indeed, infants who reached from a standing position tended to reach *correctly* to B, a level of performance not seen in any previous manipulation. (In other experiments, even when perseveration is reduced, infants are not normally correct, but at chance levels of going to either A or B.) Body memory, therefore, was whole-body memory, incorporating the trace of the reach from a specific arm-to-body position to a specific location. Disrupting that memory through a bodily perturbation was especially powerful in interrupting the influence of previous reaches.

Recently, Diedrich et al. (2000) provided equally compelling support to our assumption that the A-not-B error is generated from motor memories. These investigators, for the first time, actually tracked the path of infants' hands while they engaged in a no-hidden-objects version of the task, using computerized motion analysis equipment. At nine months – the age of the participants – infants are not yet fully skilled reachers, as evidenced in their hand trajectories. Although they go quickly and rather accurately to the target lid, their hands trace a somewhat bumpy course, speeding up and slowing down several times (von Hofsten 1991; Thelen et al. 1996). Each reach of each infant, therefore, has a distinctive speed signature, which normally varies from reach to reach. However, when infants reached repeatedly to the A side in the two-target task, Diedrich et al. (2000) discovered a remarkable result. In those infants who perseverated to A on both B trials, and presumably had built up a strong motor memory of the direction of their reaches, their trajectories converged in form. That is, the speed bumps became increasingly alike, as evidenced by increasingly strong pairwise correlations. This is illustrated in Figure 5 which is an example from a single infant reaching to a single target "C" (left panel) and in an A-not-B lids-only task where the baby perseverated on both B trials (right panel). Note that although the repetition at the single lid produced some trajectory resemblance in C3 and C4, there was a strong tendency for trajectory convergence in the

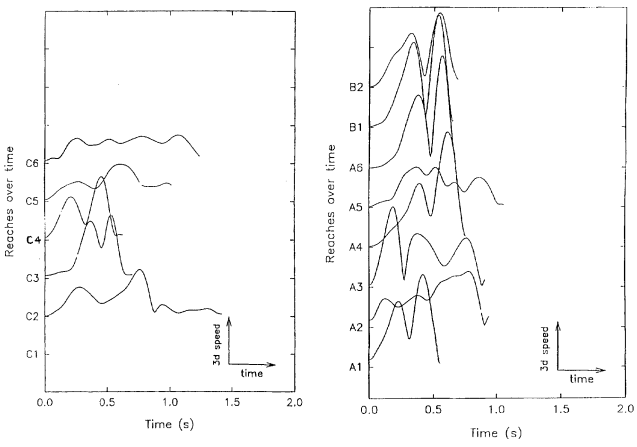



Figure 5. Speed profiles of an individual nine-month-infant in two reaching tasks. Successive trials are shown on the Y axis. Left panel: hand speeds while reaching to a single, centered target 6 times. Right panel: hand speeds while reaching in a no-hidden object A-not-B task where the infant reached to A on both B trials, although the kinematic data from trial B1 was missing. (Diedrich et al. 2000).



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two-lid task. For the group of infants, convergence was less strong in infants who spontaneously reached occasionally to B during the A trials, who reached correctly to B, and in infants reaching to only one target. It is unlikely that several reaches will have exactly the same time-space signature repeatedly in the absence of some memory of the previous reach. We can conclude that, in the conditions that produce perseverative reaching – repetition in the face of the novel and confusing two targets – both the direction and the pattern of changes of forces producing hand accelerations are held in memory from one trial to the next. And this memory is a powerful influence on the movement parameters generated for the next reach. In sum, there are good behavioral and neurophysiological reasons to seat the reach decision in a movement parameter field that integrates the visual characteristics of the task and the memory of the previous actions. Also critical is our assumption of a graded and continual field where these integrated dynamics evolve.

## 4. The dynamic field model

The model presented herein is an extension and modification of a dynamic field theory of motor programming originally formulated by Erlhagen and Schöner (2001) that is based on mathematical models formulated by Amari (1977), Grossberg (1980), and Wilson and Cowan (1973) (see Grossberg 1988 for reviews). Erlhagen and Schöner developed the model to formulate the processes of movement planning in dynamic language that ultimately may be reconciled with the dynamics of movement execution. Kopecz and Schöner (1995) and Schöner et al. (1997) have applied a similar model to the planning of eye movement saccades. Readers are referred to these papers for additional technical details concerning the models.

### 4.1. The movement planning field

We begin by describing the dynamics of the movement planning field, the site of the integration of visual input and motor memory, and the generation of the decision to reach to A or B. As we have argued earlier, this field must be able to generate and maintain specific activation states denoting the directional parameters of the reach *in a continuously evolving manner* that simulates the gradual specification of motor plans seen in experiments. Thus, the dimensions of the field in this case are the movement parameters appropriate to planning and executing a reach in a specific direction to the right or to the left. The field represents the relative activation states of those parameters. At this point, we conceptualize this field only in abstract terms as a site where visual input and memory are integrated into movement parameters supporting movement amplitude, direction, or time. Later in the discussion, we will speculate further as to possible neuroanatomical areas where such a field might evolve.

Although the model dynamics result in a dichotomous choice – A or B – it is important to emphasize again that the behavioral dimensions supporting the choice are continuous directional parameters where A and B are locations on this continuum. Unlike classic symbolic models where either A or B constitute the universe of choices, the field model allows metric specification of particular parameters from a continuum of possible actions (see Lewin 1946, for an early version of choice behavior on a continuum). The activation field then assigns an activation variable to each site on the dimension. The specification of the movement is thus a function of the amount of activation at particular values representing, in this case, direction. Thus, the field literally has a shape that reflects different possible movement states: a sharp local peak indicates a well-specified motor act. Activations that are more graded and distributed imply that the movement parameters are less-well specified, resulting in more random responding or less accurate actions toward the targets.

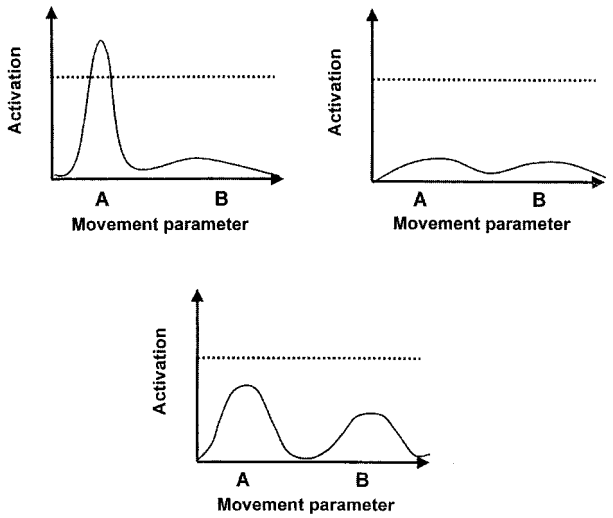



Figure 6. Activation in continuous movement parameter fields at values corresponding the to A or B direction. Top left: activation passes threshold (dotted line) for a reach to A. Top right: subthreshold activation leads to no reach at all. Bottom: Graded, and asymmetrical, but still subthreshold activation at both sites.

In Figure 6, we depict the dynamic field in terms of direction to the A or B side as a continuous space spanning the infants' visual and reaching field and locating the targets at A or B. A reach to A requires strong, above-threshold activation at the A target in movement parameters: infants need to activate whatever combination of muscles are needed to get their hand to the cued target. In the upper left panel we depict the specification of a reach to A. In contrast, without any cues to A or B and when both are visible, infants may have equally relatively low activation at both locations, and may not reach at all. The upper right panel illustrates this condition. Graded information about the target choices is reflected in the activation space, but not in sufficient strength to trigger a movement. The bottom panel represents another possibility where the distributed activation is asymmetric, favoring the A site, but where the activation has still not reached threshold for movement generation.

**4.1.1. Dynamics.** Given our extended justification for choosing the parameters of the field, we begin by defining the movement parameter, $x$, and its dynamic field $u(x)$ as representing those motor values the baby can continually specify to move in the direction anywhere from right to left. Our foundational assumption is that this dynamic field changes continuously with time, thus $u(x,t)$. The state of the field depends, however, not just on the dimension $x$, but also with respect to its own level of activation, $u$. This means


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that $u(x,t)$ itself has continual dynamics, where the next state depends on the previous one. The level of activation, therefore, cannot jump, but must build up gradually instead.

This build-up depends on the nature of the field and the inputs to the system, in this case the information about the *task* structure itself, the *specific cue* to A or B, and after the first reach, the *memory* of previous reaches. These inputs, as we stated earlier, are expressed in the dimensions of the field: $S(x,t)$, here, again, in a reach direction specifying A or B. (We explain the contributions to $S(x,t)$ in detail in the next section.) So the most simple case where the inputs are added together to the field can be expressed mathematically as:

$$\tau \dot{u}(x,t) = -u(x,t) + S(x,t) \quad (1)$$

where we have also added a linear decay term, $-u(x,t)$ which together with the constant $\tau$ defines the time scale over which the field gradually builds up or decays.

**4.1.2. Time scale.** Because the field is dynamic, its time scale of activation is critical. We illustrate the operation of $\tau$, the time scale parameter, with a simple case where each site on the field evolves independently, that is, without influencing its neighboring sites. Under this condition, the activation at each site relaxes over time to the level of the input, and the stationary solution, $u(x) = S(x)$, directly reflects the input. However, when the input changes, the activation in the field does not change with the input instantaneously, but has a certain inertia. Specifically, when the input changes in a step-like manner, say from $S(x) = 0$ to $S(x) = S_0(x)$ , then the field changes exponentially according to:

$$u(x,t) = \Delta u(x) exp\left(-\frac{t}{\tau}\right) + S_0(x) \quad (2)$$

where $\Delta\, u(x)$ is the initial deviation of $u(x,0)$ from $S_0(x)$. Thus, $\tau$ is the amount of time during which the distance between the current activation level and the input level is reduced to $1/e$, or about 37% of its initial value. Because $\tau$ expresses a percentage of change, the temporal evolution of a site proceeds independently of its current activation, although its activation level itself is strictly a function of its previous activation. This is a characteristic of any dynamic system close to its stationary state. Thus, without cooperativity, the field evolves over time to assume the shape of the specific input and then it decays.

**4.1.3. Cooperativity.** We used the simple case of site independence to illustrate the temporal properties of the field. This limited case is unrealistic, however, because if all the sites were independent, the field would only reflect the exact parameters of the inputs and could not reach a decision in the face of several competing inputs. A mechanism for integrating graded information is needed: the sites must be coupled so that a single decision can evolve (Amari & Arbib 1977). To produce such a self-sustaining peak from multiple inputs, the field is endowed with interactions so that sites that are close together are mutually excitatory, whereas more distant sites are inhibitory. We refer to these interactions within the field itself that can enhance (or inhibit) activations as *cooperativity*. (Note that, strictly speaking, these interactions contain both excitatory and inhibitory activations. Typically only the former are called cooperative connections, while the inhibitory interactions are usually considered competitive. We adopt the term cooperativity to stand for the combined effects of the interactions as both mutually produce (or inhibit) self-sustaining peaks.)

Again, cooperativity, $g_{\text{intra-field}}$, like the other contributions, is a function of the state of the field itself and so may be expressed as an added term to Equation 1:

$$\tau \dot{u}(x,t) = -u(x,t) + S(x,t) + g_{\,intra\text{-}field}[u(x');x'] \quad (3)$$

More specifically, $g_{\text{intra-field}}$ is composed of two functions: an interaction kernel, and a threshold function. The interaction kernel $w\,(x - x')$ allows the model to generate self-sustaining solutions by the balance of local and global excitation and inhibition. The interactions may arise from any point $x'$ in the field. An additional assumption is that these cooperative interactions are homogeneous within the field, so that no point is privileged over others and the interaction thus depends only on the difference, $x - x'$, the distance between the sites. The interaction kernel has the form:

$$w(x - x') = -w_i + w_e exp\left[-\frac{(x - x')^2}{2\sigma_w^2}\right] \quad (4)$$

where $w_i > 0$ and $w_e > 0$ are the strengths of the inhibitory and excitatory components and $\sigma_w > 0$ is the size of the excitatory region, which establishes the size of the localized activation patterns. This is illustrated in the top panel of Figure 7.

Not all sites in the motor parameter field can contribute to the interaction at all times, however, as this would lead to a single, inflexible solution. As is true in real neural systems, only those sites that are activated communicate to other sites. Thus, a threshold function, $f(u)$, allows only certain levels of activation to enter into the interaction:

$$f(u) = \frac{1}{1 + exp[-\beta u]} \quad (5)$$

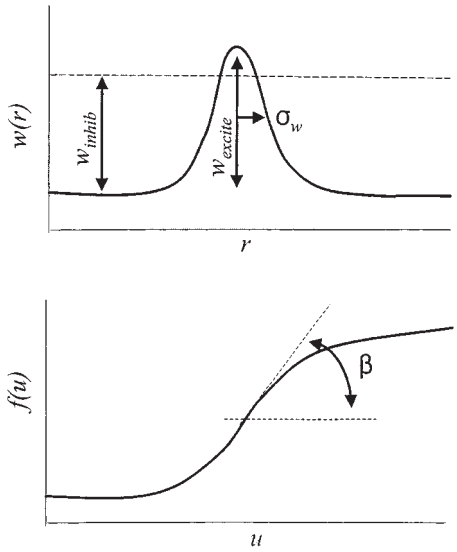



Figure 7. Cooperative interactions within the dynamic motor field. Top panel: Interaction kernel $w(r)$ consists of a local excitatory zone of width $\sigma_w$ and strength $w_{excite}$ and a global inhibitory contribution of strength $w_{inhibit}$. Bottom panel: The contribution of any location in the neural field to the cooperative interaction is determined by a threshold function $f(u)$ to the neural activation. The slope, $\beta$, of this function determines the degree to which subthreshold values of activation contribute to the interaction.



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where β is the slope of the sigmoid function, set to a zero-to-one range, where the system becomes activated, depicted in the bottom panel of Figure 7. (For a discussion of the different types of nonlinearity involved, see Grossberg 1973.)

Thus, $g_{intra\text{-}field}$ is the product of the interaction kernel and the threshold function which is then integrated over all sites of the field:

$$g_{intra\text{-}field}[u] = \int w(x - x')f(u(x'))dx' \quad (6)$$

When the cooperative functions are added to the other contributions to the field, as sketched in Equation 3, the dynamics take the form of Equation 7, reflecting the continual evolution of the motor field with a particular time scale, cooperativity, and inertia, in the presence of sensory inputs.

$$\tau\dot{u}(x,t) = -u(x,t) + S(x,t) + \int w(x - x')f(u(x'))dx' \quad (7)$$

We now add two additional parameters to the equation. The first, $h$, sets a resting level to the field. Recall that we are able to set a threshold for entering into the interaction kernel. At any given threshold, however, the number of sites that actually participate depend upon the resting level of the field. If this level, $h$, is small, only sites with strong input contribute to the interaction because the threshold is relatively greater. In contrast, if the sites are already somewhat active, and the threshold is effectively lower, the interaction is much less localized and more widely distributed.

This resting level has profound implications for the resulting field dynamics. When $h$ is low and only strong inputs predominate, the system is driven largely by inputs and less by the local interactions. The field behaves more like the one shown in Figure 7 where the output reflects the input. When $h$ is large, however, many sites contribute to the interaction and the localized excitation becomes amplified by the many excitatory connections of its neighbors and the corresponding surrounding inhibition. In this regime, excitation can become self-sustained even without continual input, and the field can express a decision in the face of multiple inputs (Amari 1977). This is the critical mechanism for the integration of perception and memory into a decision field as it allows inputs of different relative types, strengths, and degrees of specificity to contribute to an integrated motor outcome.

Finally, we add a term, $q\xi(x,t)$, for Gaussian noise, giving the field dynamics the overall form:

$$\tau\dot{u}(x,t) = -u(x,t) + S(x,t) + \int w(x - x')f(u(x'))dx' + h + q\xi(x,t) \quad (8)$$

As in any dynamic system that exhibits multiple states, these fields are sensitive to noise near instabilities, and thus noise is justified in the equation. In reality as well, infants' behavior is noisy. The A-not-B decision is always a probabilistic one; sometimes infants spontaneously reach to either A or B whatever the input or history of the system.

As we show in the simulations, below, these intrafield dynamics capture both the age and the delay effects in the A-not-B error, as well as their interactions. In principle, one might imagine developmental changes in any of these interaction parameters: in $h$, the ability of the field to generate a localized solution; in the strengths of $w_i$, $w_e$, *and* $\sigma_w$ which determine the relative basins of inhibition surrounding peaks of excitation, and/or in the threshold function. For the purposes of our simulations, however, we will assume that the main developmental effect lies in $h$, the ability of the field to enter the cooperative, self-sustaining regime.

### 4.2. Inputs to the field

With these intrinsic properties, the action decision evolves in the motor planning field under the specification of three sources of input:

$$S(x,t) = S_{task}(x,t) + S_{specific}(x,t) + S_{memory}(x,t) \quad (9)$$

The first two, $S_{task}$ and $S_{specific}$, are the parameters of the persisting task environment and of the cueing event that are both present in the very first reach to A. The third, $S_{memory}$, is the contribution to the current motor decision from the system's history, and thus effectively enters into the model after the first reach. Critical to this formulation is that the inputs to the decision field are expressed in movement parameters so that they may be mutually coupled. The assumption here is that the inputs, like the movement parameters, are not discretely specified, but are identified as locations on a continuous field.

**4.2.1. Task input.** People move within an immediate spatial environment which usually remains stationary. This environment provides persistent visual (or tactile or auditory) input that specifies the task space – what objects and surfaces delineate the continuous targets and supports for action. These are the features of the world which constitute the behavioral alternatives within the intentions of the actor: the possibilities to move in one direction or another (or forcefully, or gently, etc.). The task layout is thus prespecified and independent of immediate signals to act. In experimental situations, the task parameters typically do not change during the performance of a single trial, although they will often be varied for different experimental contexts. Without additional specific attentional cues or memory traces, the strength and symmetry of the task input determine the contours of the decision field. When specific attentional or memory inputs are added, they combine with the existing and tonic activation patterns set by the task environment.

Consider the infant reaching task with the two possible target locations, A and B, always in view. In Figure 8, we illustrate the task input (before any additional cues are provided) as activations in motor parameter space. The first way that the task input can be characterized is by the target locations along the decision field. These are represented as probability distributions in the field centered on two locations, $x_A$, $x_B$. In the upper left graph of Figure 8, we represent the activation distributions around two similar targets that are well-separated and provide two clearly specified goals. The upper right panel, in contrast, shows these distributions when A and B are close together, their distributions overlap, and there is a greater probability that the baby would, by chance, reach to either A or B. Indeed, target confusion as evidenced by A-not-B errors is reduced when the covers are relatively farther apart (Acredolo 1985).

The second way in which the task environment can be parameterized is by the distinctiveness of the targets, and hence, their relative attractiveness. Imagine as an extreme case, a hungry infant faced with two targets, a cookie and a familiar toy. Without any additional cueing, the baby would likely spontaneously reach for the cookie and would persist


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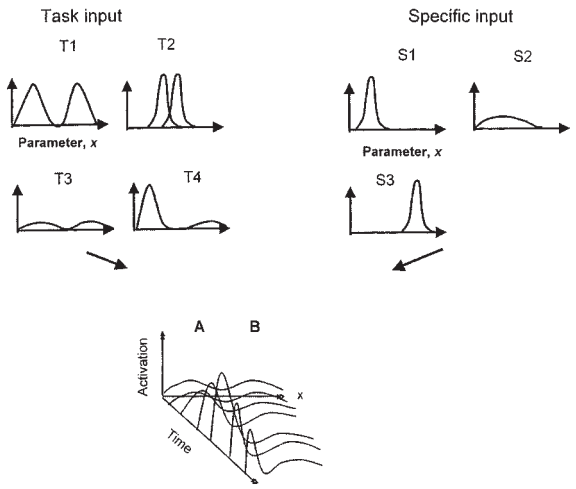



Figure 8. Inputs to the decision field at the first reach to A, showing possible configurations of the visual stimuli. *Task input.* T1: two clearly specified, separate targets; T2: two specified targets close together; T3: two boring, identical targets; T4: one attractive and one boring target. *Specific input.* S1: well-specified cue to A; S2: poorly specified cue to A, for example, a quick wave of the hand over the target; S3: well-specified cue to B. The bottom panel shows the time evolution of T3 and S1, where a strong cue to A interacts with two weakly specified targets.

in going for the cookie despite being repeatedly enticed with the toy. This is a measure, therefore, of the strength of the task environment to compel reaching to one side or another. Thus, $S_{task,0}$ is the dimension of the field expressing the tendency of the infant to go to B when A is cued or to go to A when B is cued, in other words, to "spontaneously" be pulled to one side or another. Finally, parameter $c_{A/B}$ is used to express any asymmetry in the task arrangement. When the targets are alike and placed side-by-side, $c_{A/B} = 1$.

To illustrate this, refer again to Figure 8. The bottom left panel represents the typical A-not-B situation used in Smith et al. (1999b) where the task input specifies two identical covers or targets at A and B. Since the targets are indistinguishable from each other and not highly distinct from the background, there is no strong incentive for infants to reach to either A or B or even to reach at all. The task contribution to the motor decision field is centered around the target areas, but not biased to either location, and not very strong. Next, imagine that the usual A-not-B targets are replaced by one plain brown lid and one colorful and attractive toy. As in the illustration in the lower right of Figure 8, the decision field would then be biased even before the experimenter called attention to the A or B side because of the increased possibility that infants will go for the toy rather than the lid. A third possibility – two attractive toys side-by-side – is shown in the illustration at the top left of Figure 8. Here, as in the first situation, the task environment does not offer a basis for choice of A or B, although both targets are attractive and the probability for a reach to either is high. Finally, a parameter $\sigma_{task}$ characterizes the spatial spread of the activation function, similar to that used in the field dynamics. For our simulations, this parameter is fixed.

Together, then, the specification of the task input for the A-not-B paradigm has the following Gaussian form:

$$S_{task}(x,t) = S_{task,0}(exp\left[-\frac{(x-x_A)^2}{2\sigma_{task}^2}\right] + c_{A/B}exp\left[-\frac{(x-x_B)^2}{2\sigma_{task}^2}\right]) \quad (10)$$

We chose the Gaussian because the function conveniently expresses the forms of the three parameters of the task input (location, strength, and width).

As discussed in an earlier section, because the targets are typically similar and relatively close, without other input, infants have no compelling reason to reach preferentially to A or B and their performance is at chance levels (Smith et al. 1999b). To overcome this, infants must be trained to go consistently to the A side by making that location more salient. Experimenters do this by moving the object-to-be-hidden, or the lid, forward so that it is closer to the baby, providing a visually more distinctive target.[1] This adds asymmetry to the task input field by making $c_{A/B}$ not equal to one, and thus biases the first reach – and subsequent reaches – to one side. Changing the distinctiveness of the lids, that is, making one lid a different shape or color, also biases the task field, so that once one side is cued, infants may have continual reminders of the differential targets. We simulate both the training and distinctive target effects.

Experiments have shown that other features of the task environment such as visual landmarks or background colors and surfaces can interact with the distinctiveness and placement of the targets to determine whether infants perseverate or not. As summed up by Butterworth et al. (1982), whatever the perceptual basis of this task, "it is extremely sensitive to variations in context" (p. 447). The task input field can be adjusted to simulate these variations.

**4.2.2. Specific input.** While looking at the task scene, the infant is cued to one target, A or B, and this cue also contributes in a graded way to the movement decision. Thus, the model has a second source of input, $S_{spec}$, the phasic visual cue of the experimenter waving, tapping, or otherwise calling attention to the target object. The specific input is similar to the task input as it is characterized as a location in motor parameter space, with a particular strength, and activation spread. It differs because it is a time-limited input to the field to simulate the transient visual cue which then must be held in memory. Thus, $x_{spec}$ represents in the equation the location where the attentional cue is delivered, in this case either at A or B. The strength of the input, $S_{spec,0}$, can be varied to capture the saliency of the cue. For instance, waving or hiding a bright, glittery or noisy object, a brightly colored toy, or a cookie will be more attention-grabbing than a plain colored cloth or lid, as is further illustrated in Figure 8. In addition, as in the task input, the specific input can have different values for its activation spread, $\sigma_{spec}$. The cue duration is entered into the model as a step function. Thus, before the cue is given, the specific input is zero. During the cue, the specific input is non-zero at a constant level and then it instantaneously returns to zero at the termination of the specified time. A cue of longer duration provides more input to that location in the field. The form of the specific input is:



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$$S_{spec}(x,t) = S_{spec,0} exp\left[\frac{(x - x_{spec})^2}{2\sigma^2_{spec}}\right] \quad (11)$$

during the time interval of length ΔT when such input is present and zero otherwise.

**4.2.3. Memory input.** The third source of bias to the field adds its influence and is in the form of a memory field in movement parameters retaining the shape of previous decisions to go to A or B. The memory field gets its input from the motor planning field, and thus encodes the history of all previous reaches. Because the planning field itself has integrated the two visual inputs, that of the task and specific cue, the memory also indirectly reflects the whole perceptual aspect of the repeated task. As it builds, the memory input contributes not only to the probability of a reach to A or B after cueing, but also to the likely direction of a spontaneous reach. As reported in Smith et al. (1999b), once infants have made a decision to go to one target or another in the absence of a specific cue, they are likely to stick with their choices. The memory input becomes stronger after each repeated reach to one location so that it may swamp the task and the transient cue. This is the heart of the A-not-B error.

The memory field itself has dynamics: it evolves continuously in time. These dynamics occur on two time scales, one related to its growth and one to its decay. First, the memory field grows in parallel with the motor planning field in the few seconds between cue and reach. More specifically, the planning field enters the memory field whenever the planning field is activated above a certain threshold, $u_0$. The memory field has its own time scale, specified by $\tau_{mem}$:

$$\tau_{mem}\dot{u}_{mem}(x,t) = -u_{mem}(x,t) + \Theta(u(x,t) - u_0)$$

Here, $\tau_{mem}$ governs the growth process of the memory, and $\Theta(u(x,t,) - u_0) = 1$ if $u(x,t,) > u_0$ and zero otherwise. The memory field reaches a maximum level after approximately six trials: $\tau_{mem} = 6\tau$. This is where we conventionally test for the error with a cue to B.

The second time scale is that of the decay of the memory field. In theory, we assume that the memory field decays slowly in the absence of activity in the motor plan field in the time that elapses between one reach and the cue for the succeeding trial. For the purpose of these simulations, we have assumed that this time scale of this decay is much slower than the inter-trial interval, and therefore does not enter into the model as a parameter.

$$\dot{u}_{mem}(x,t) = 0$$

We believe this to be reasonable because perseveration, and hence, the persistence of the memory, is robust despite variability in the inter-trial intervals: delays between the end of one reach and the initiation of the succeeding reach ranged from 20 to 50 seconds. This means that within the time scale of the typical A-not-B task, this decay may not be critical, although we have not empirically tested these limits.

In summary, the contribution of the memory input takes the form

$$S_{mem}(x,t) = S_{mem,0} u_{mem}(x,t)$$

where $S_{mem,0}$ is the strength of the memory input.

### 4.3. Output

The field represents a parametric movement plan, but is not linked to a model of the actual motor control of the arm (see Kopecz & Schöner 1995, for integration with motor control of eye movements). Here, we approximate such control to be a simple read-out procedure: when the movement is elicited after the delays, we assume that the location in the field with the maximal activation describes the movement that is actually performed. This approximation is reasonable as long as no other manipulations or perturbations intervene at the control level.

## 5. Simulations

Before we discuss the simulations of the model and compare them to experiments, a few comments about the model parameters may be useful. As with any model, we have constructed a mathematical abstraction – one of many possible abstractions – of complex and multi-determined behavioral events. Some aspects of the events, such as the timing of cues and trial length, can be assigned realistic parameter values. For others, such as the relative separation of the targets or the strength of the cues, parameter values are less directly mapped onto experimental factors. In principle, the basic effects are described by the model over a wide range of values of these parameters. However, the constellation of parameters is strongly constrained by the experimental results. The model is successful if two conditions are met in parameter assignment. First, the ensemble of parameters must produce the primary experimental effects when values are fixed so that all the orders of magnitude are reasonable in relation to one another. For instance, to produce the canonical error, the specific input must be sufficient to produce an A-side decision on the first reaches, but not so strong as to dominate the field interactions as the memory strength builds. Within such constraints, the precise parameter values are not critical: the qualitative effects are robust within a range of parameter values. Having determined these values on the primary result, the test of the model is to keep certain parameters fixed – the characteristics of the field, for instance – and to simulate different experimental results, as well as generate new testable predictions. Here, for example, we use the settings of the canonical effect and manipulate different values of the visual input. Together, these conditions show that the model is both internally consistent and externally valid. Note that the internal structure of the dynamic field model precludes simulation of any arbitrary input-output relationship through judicious choice of parameters. Strong theoretical assumptions of the mechanisms involved are incorporated into the equations, such as localized input with uniform width, homogeneous symmetrical interaction kernel, and superposition of inputs. Many connectionist models, in contrast, have been shown to be universal approximators under some conditions (Hornik et al. 1989).

The question addressed by the model, then, is how a decision to reach to A or B evolves under varying conditions, represented by the parameters of the model. To reach a decision, the dynamics of the inputs are thus coupled to the dynamics of the movement field. The model is integrated in time for 10 seconds, which realistically represents the time interval that begins when the infant looks at the display, the experimenter waves the target, the delay is im-


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posed, and ending with the decision to go to one target or another. Evolution in time results from the dynamic equation, the solutions of which provide values of activation at all sites in the field as a function of time. These solutions can be visualized as activation landscapes representing the relative strengths of various values of the movement parameter signifying direction.

In the simulations, we solved the dynamic equations on a digital computer using the Euler procedure, in which one time step represents 50 msec. The results do not depend on the time step, which we chose to be sufficiently small so that the numerically obtained solutions approximate the real solutions of the dynamical system. An individual simulation run of 10 seconds realistically models a single reaching act. At the end of each run, the activation in the movement field is reset to zero, readying the system for a new trial. By contrast, we continuously update the memory field to reflect the build up of a history of reaches. A sequence of such individual trials reflects the experimental paradigm (six reaches with cues at A, two reaches with cues at B). Such entire trial sequences are then repeated 500 times to obtain statistics. The outcomes vary from repetition to repetition due to the presence of stochastic forces. We store the value, $x$, at which the field has maximal activation during the sequences so that across the 500 repetitions, we can generate histograms that estimate the probabilities for moving to A or to B.

In the following series of simulations, we fixed the following parameter values of the motor field throughout: $\tau = 500$ [msec], $w_i = 1$, $w_e = 2$, $\beta = 1.5$. We changed the resting level, $h = -6$ for cooperative regime and $h = -12$ for the noncooperative regime. Moreover, in the task field $S_{task,0} = 1$ remained constant and the spatial spreads of the inputs, $\sigma_w = \sigma_{task} = \sigma_{speci} = 10$ throughout. Likewise, $S_{mem,0} = 3$ was fixed throughout. Thus, experimental effects were generated only through manipulation of $h$ and the inputs, $c_{A/B}$ and $S_{spec,0}$.

The field was supplied with the task input for 1 second, presented with the specific and memory inputs for 4 seconds, and the delay was always also 4 seconds. These values are consistent with the real task timing.

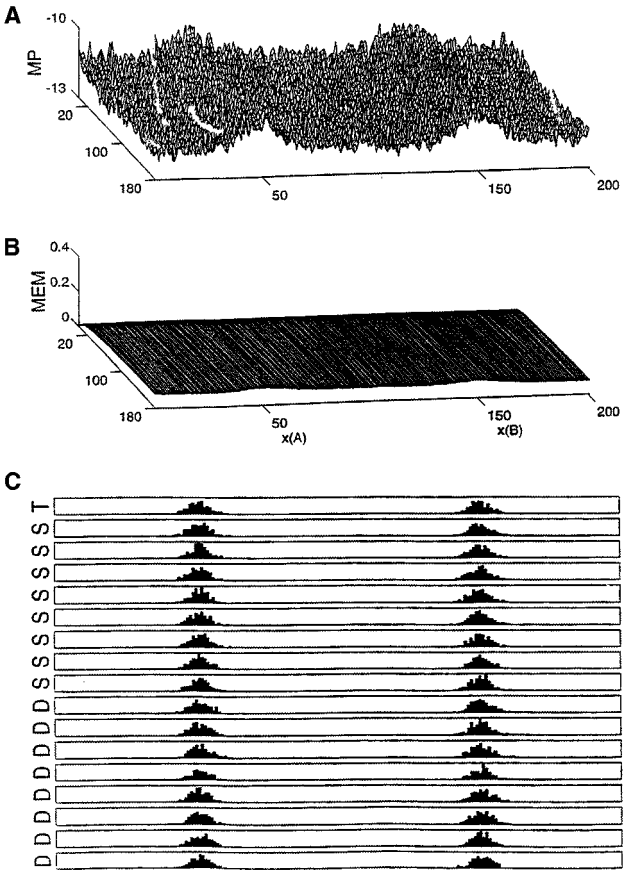



Figure 9. (A) Motor field dynamics in non-cooperative regime with task input only (no specific input) at the first reach to A (A1). Parameters: $S_{spec} = 0$, $S_{task} = 1$, $S_{mem} = 3$ (a). Motor planning field evolution. (B) Corresponding evolution of memory field. In this figure and subsequent figures, x axis denotes field location, y is time, z is activation. On the y axis a letter code indicates the input present at different moments in time: *T,* task input, *S,* specific input (none added here), and *D,* the delay where no specific input is added. (C) Histograms of decisions to A or B from an ensemble of 500 simulations per trial showing the read-out of the field as a function of time. The decision to reach to A or B is probablistic; in this case, A or B is equally likely at any point in time.

### *5.1. Results*

**5.1.1. Properties of the movement field without specific input.** The first simulation emulates the condition where the infant looks at the two identical targets, but no cues are given and there has been no training to A or to B. In these simulations, we have set the cooperativity of the field to be weak so that the local interactions cannot lead to a self-sustaining peak, as we would assume in an 8–10-month-old infant. Figure 9 shows the evolution of a typical field at the first reach to A under these conditions, while (B) illustrates the corresponding growth of the memory field, in this case, nearly flat. The bottom histogram gives the probabilities of A or B decisions from the repeated simulations shown over 17 time steps within the trial. The main influence on the field is the rather diffuse cues given by the task input; the field is noisy and converges to the weak specification of the task input. As can be seen in the histogram, the infant is equally likely to go to either A or B and because both are noisy and weak, possibly not to reach at all. Additionally, the memory dynamics generated by this initial condition are themselves weak and poorly differentiated, as can be seen in Figure 9.

The dynamic approach to the A-not-B error offered in Smith et al. (1999b) made the novel predictions that without training that highlighted the A side, reaches to A and even reaching at all would be fragile. This is formally simulated by the model as well, and has been confirmed in experiments (Smith et al. 1999b). Identical targets, especially without additional cues, are confusing for infants.

**5.1.2. Properties of the motor field at the first cued trial to A.** In the second simulation, we illustrate the effects of adding just the transient specific input on the very first trial to the A side. Note in Figure 10A under the noncooperative regime (e.g., younger infant) that this causes a rapid increase in activation to the A side and also a rapid decay as the peak is not self-sustaining. In the cooperative regime (e.g., older infant, Fig. 10C), the nonlinear properties of the field result in a more sustained above-threshold activation. This is further reflected in the evolution of the memory dynamics which are stronger at the end of the motor planning process in the cooperative versus noncooperative regime



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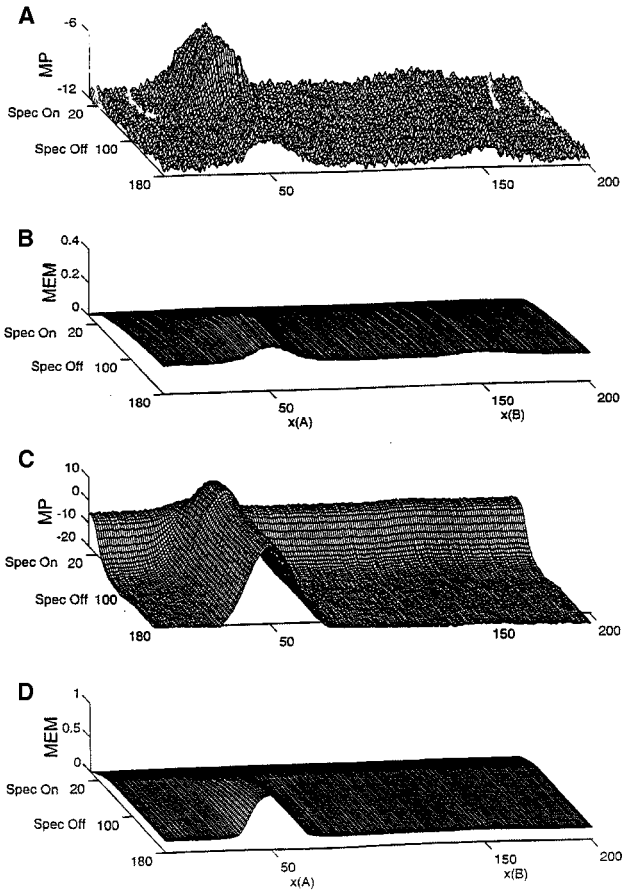



Figure 10. Motor field dynamics with task and specific input at A at A1. (A) Noncooperative regime ($h = -12$). (B) Memory field evolution for (A). (C) Cooperative regime ($h = -6$). (D) Memory field evolution for (B). Parameters: $S_{spec} = 4$, $S_{task} = 1$, $S_{mem} = 3$, $c_{A/B} = 1$.

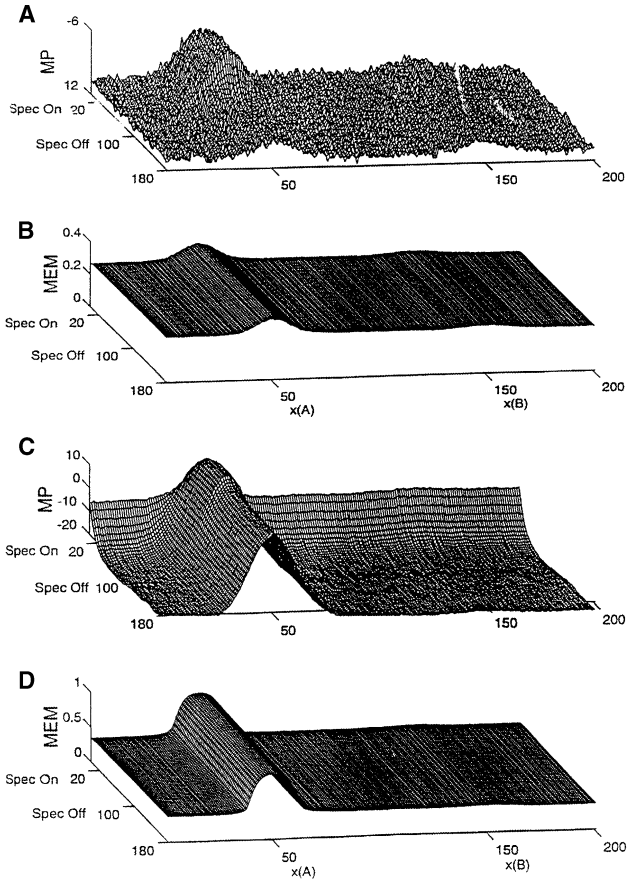



Figure 11. Motor field dynamics after 6 reaches to A. (A) Noncooperative regime. (B) Memory field evolution for (A). (C) Cooperative regime. (D) Memory field evolution for (C). Parameters: $S_{spec} = 4$, $S_{task} = 1$, $S_{mem} = 3$, $c_{A/B} = 1$.

(Fig. 10B & D). This simulation captures an important developmental effect: the ability to remember the location of the cue when it is no longer there. The novel contribution of the model is to embed that memory in movement parameter space where it is behaviorally expressed only as part of the evolution of the other contextual and field dynamics (see Rizzolatti et al. 1997).

*Novel predictions:* This simulation also predicts that for younger infants, simply cueing at A without any previous training would lead to an increase of correct responding with shorter delays. This is currently being tested: however, age and delay interactions have been amply demonstrated on the B test trials (e.g., Clearfield et al. 1999; Diamond 1985).

**5.1.3. Motor field dynamics after repeated reaches to A.** What happens when the memory of the previous reach is added to the task and specific input? In Figure 11, we report the results of a typical field evolution after six A trials, in both noncooperative (A and B) and cooperative (C and D) regimes. But to more realistically simulate the actual experimental design, we also included an effect for the training trials. Recall that in the typical A-not-B task, infants were trained to A by gradually hiding the toy, or in the lids-only condition, progressively moving the A lid back over several trials. The effect of this training was to make the A side more salient at first, and then gradually more similar to B. This asymmetry in the training trials was implemented by decreasing the relative influence of the A location in the tonic task input over the first four trials by setting parameter $c_{A/B}$ to 2(A1), 1.75(A2), 1.5(A3), and 1.25(A4). In subsequent trials, when the task field was symmetrical, $c_{A/B}$ remained at 1.

In both regimes, there is a strong tendency to reach to A and a persisting memory of that location after the field evolves. The differences are in the relaxation of the field, which is more pronounced in the noncooperative state and the strength of the memory, which is stronger in the cooperative regime. The simulations suggest that when cued at A repeatedly, both younger and older infants should be strongly attracted to that location. Why, then, the A-not-B error in younger, but not older infants?

**5.1.4. The A-not-B effect: The B trials.** We now add the critical test trial – the cue at B after repeated reaches to A. The exemplar trial simulations illustrated in Figure 12 compare the motor fields in noncooperative and cooperative regimes and show the A-not-B error (A) and correct responding (C), respectively, at the first B trial. In the simulation of the younger infant, the field evolves within the residual memory of A. The cue to B causes strong activation at first, but it cannot be sustained and decays rapidly.


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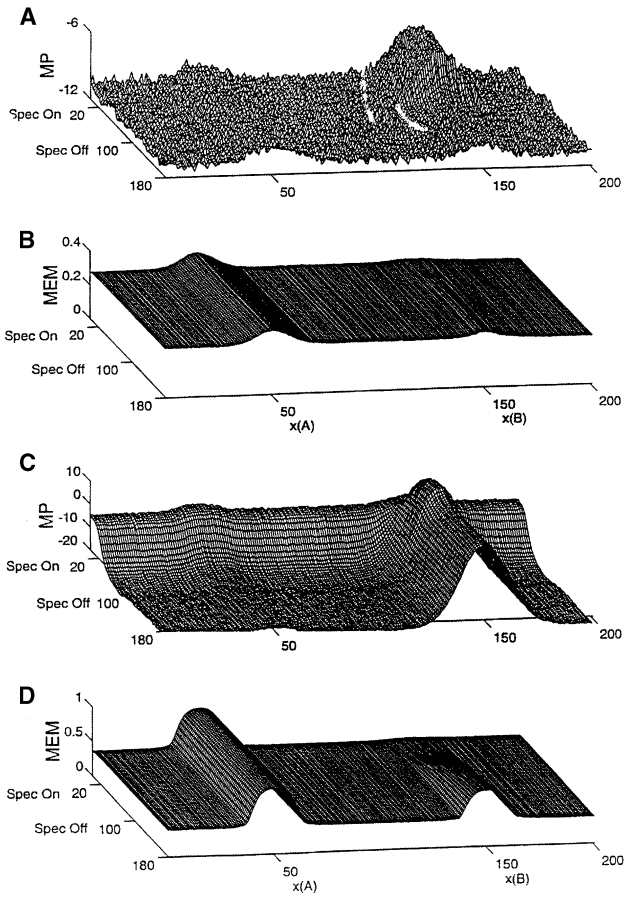



Figure 12. Motor field dynamics at trial B1. (A) Non-cooperative regime. (B) Memory field evolution for (A). (C) Cooperative regime. (D) Memory field evolution for (C). Parameters: $S_{spec} = 4$, $S_{task} = 1$, $S_{mem} = 3$, $c_{A/B} = 1$.

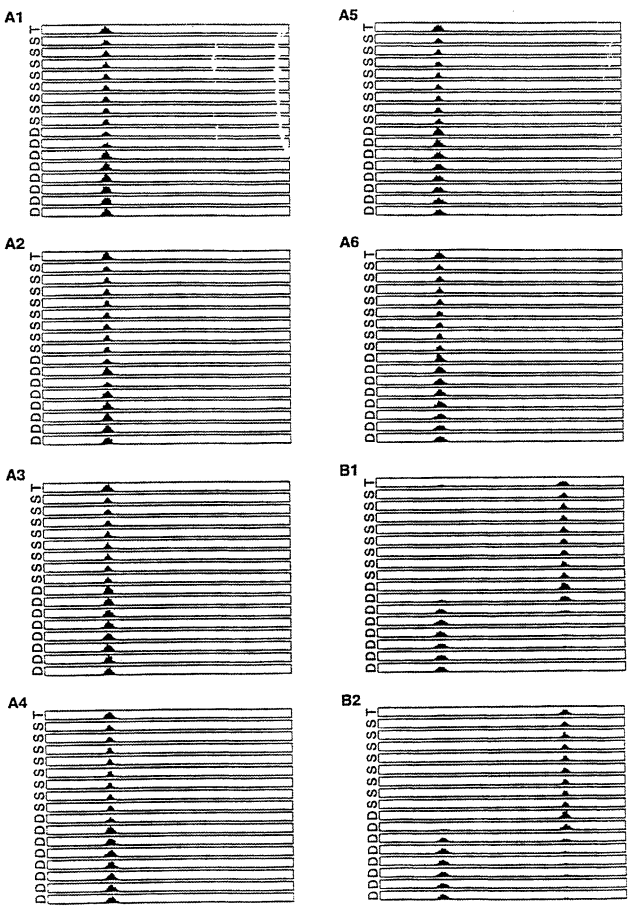



Figure 13. Distribution of decisions to A or B from 500 runs per trial in trials A1-B2. Field is in noncooperative regime; infants make A-not-B error. Note in B1 and B2 that the error emerges over the delay. At no delay, infants are correct, as shown experimentally. Other parameters same as in Figure 12.

If the resulting field evolves over the full time segment, the movement parameters for an A reach predominate. If however, the infant were allowed to reach without the delay, the B activation would swamp the memory of the A reaches. In the cooperative regime, in contrast, the ability to hold the B side activation for the full extent of the field evolution leads to a stronger tendency reach to B both with shorter and longer delays, despite the influence of the A memory.

Additional simulations predicted that even after the second cue to B, infants should still be making the A-not-B error in the noncooperative state and they should continue to be correct in the cooperative state. Indeed, in the classic version of the task, most infants perseverate to A on both the first and subsequent cues to the B side, although the number of infants making the error on both B trials is less than those erring on trial B1 (Diedrich et al. 2000; Smith et al. 1999b). That is, the memory of A persists through the second indication to reach to the B side. The simulation of the older infants, in contrast, indicates continued correct spatial choices. These differences are shown again in Figures 13 and 14 , which are the probability histograms generated by the full set of simulations.

Thus, the model captures the important and well-documented developmental and delay effects of the classic A-not-B error: the error is reduced with age (and the ability to retain the memory of the target stimulus parameters in the motor field) and with short delays. But, we can also address two related phenomena not usually considered in the traditional accounts: the effects of repeated reaching and the production of "spontaneous errors" or "incorrect" reaches to B on the A trials. This is best depicted in the relative memory strength to A measure that shows the evolving attraction for each location (Fig. 15). The simulation in the noncooperative regime closely modeled the experimental data of infants who made the error. But – and importantly – the simulation also shows that infants who are presumably older (i.e., cooperative) are correct on the B trials for a different reason than those who are younger (i.e., noncooperative). The former are largely correct on all A trials and on all B trials. The latter are reaching to B on the A trials and thus form a stronger B attraction. There is not only an increased likelihood of A-not-B error (reaching to A when B is cued) in the noncooperative regime, but also increased variation in the relative strengths to each side, suggesting more spontaneous errors (reaching to B when A is cued), as indicated in Figure 16.

Thus, two processes are working in the noncooperative state. First, the A memory builds and without the ability to sustain the B-specific input the field relaxes to the A memory, creating the error. But the same dynamics also can produce correct responding typically seen in about 20% of the infants tested in the classic task. In some infants, the de-



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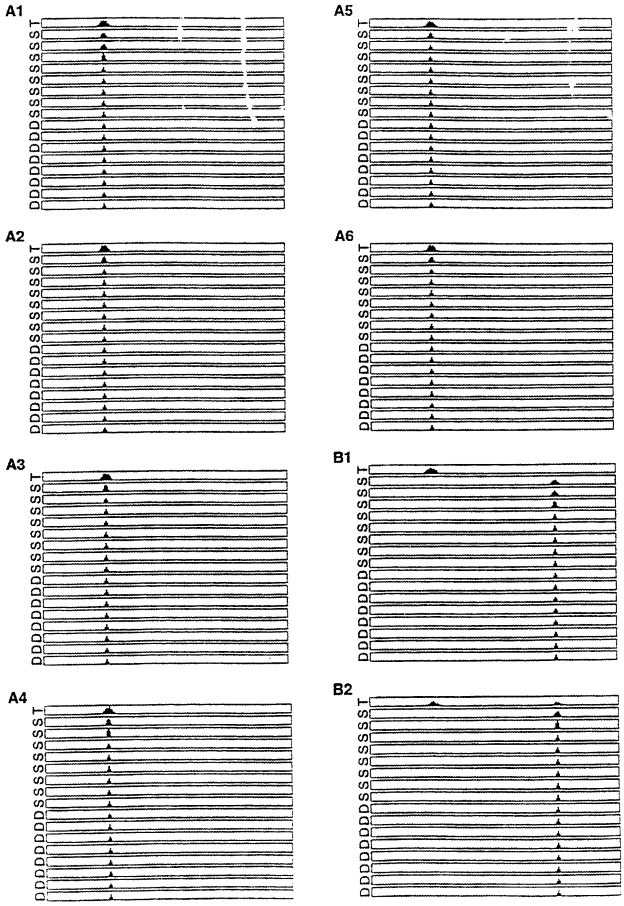



Figure 14. Distributions of decisions to A or B in Trials A1-B2. Field is in cooperative regime; infants reach correctly on B trials before and after the delay, despite the lingering memory of the A reaches. Other parameters same as in Figure 12.

caying A activation field allows for occasional noise-induced spontaneous reaches to B. Once an infant has reached to B during the A trials, the cumulated memory strength for A is reduced, generating less of pull to A and more tendency to go to B. If the A strength is sufficiently diminished by fewer A reaches, then the probability of the returning to A on the B cue is also decreased. This is precisely what was discovered in experiments (Diedrich et al. 2000; Smith et al. 1999b).

The appearance of spontaneous errors depends on noise, but also on the relative confusability of the targets. Note in Figures 15 and 16, as in Figure 2, that spontaneous errors become noticeable not in the earliest trials, but at A3 and A4, after the infant has been cued several times to A. Why would spontaneous errors arise more often after several reaches to A, when the perceptual motor habit for the target location presumably has been strengthened? Recall that in the first few trials, the task input is asymmetrical because the A target is made more salient. This asymmetry is strong enough to keep infants' attention directed toward the A side. However, as the targets become spatially closer and presumably less distinct, spontaneous mistakes to B begin to accumulate.

*Novel predictions.* So far, the model and the dynamic assumptions underlying it generate several additional testable predictions based on the simulated effects in the

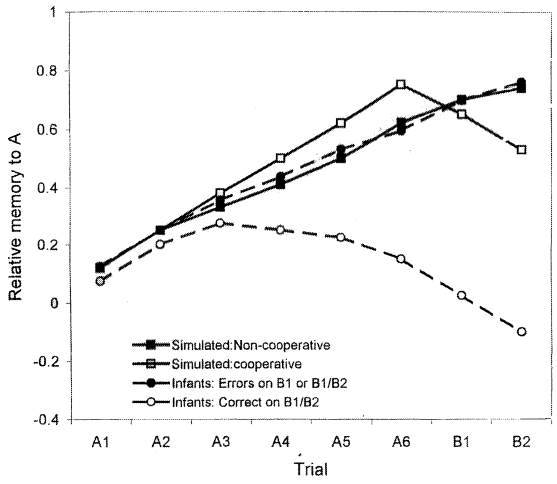



Figure 15. Relative memory for A in noncooperative and cooperative regimes for simulations shown in Figures 14 and 15 plotted along with experimental data reported in Figure 2 for 9-month-old infants who made errors on B1 or B1 and B2 and infants who were correct on B1/B2. The noncooperative regime simulations closely fit the data for the infants who made the errors. However, the cooperative regime represents older infants who are correct on both A and B. The 9-month-olds who were correct did so for a different reason: they made spontaneous errors to B on the A trials. Correct performance arises from different mechanisms in younger and older infants.

movement field. First, the error resides, we maintain, in the ability to generate a reach in a specified direction and to retain a memory of that action that is sufficiently strong to swamp a new visual input. The model assumes that a critical contribution to the error is the inability of young infants to maintain a self-sustained activation peak in the movement field once the input disappears. One new prediction is that reaching location perseveration may be evident in infants younger than 8 months. This has never been tested because in the traditional view, eliciting the A-not-B error requires that infants can be trained to uncover hidden objects, and infants under 8 months will not do so. But in a no-hidden-object task, perseveration may be seen earlier.

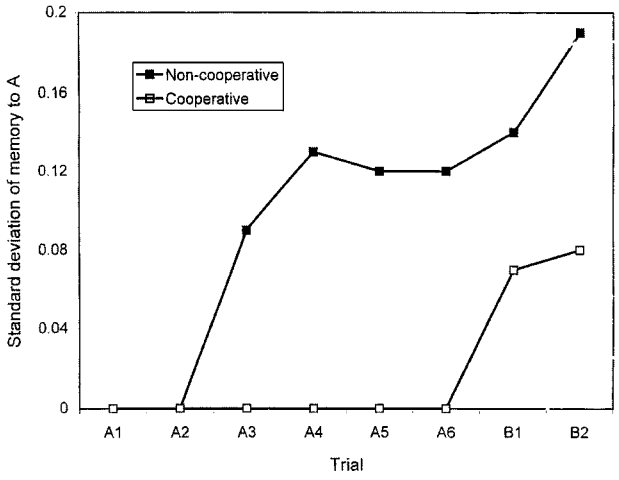



Figure 16. Standard deviations of the memory to A measures of Figure 15.


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More specifically, we suggest an inverted U-shaped developmental function: before about 5 months, infants do not reach consistently to even single targets. They will find the A-not-B task especially daunting and may not reach at all, or if they do, in such a sluggish and inconsistent manner as not to build up the memory to the A place. In addition, very young infants have unstable posture while reaching and their own postural wobble may act as perturbations to the growing motor memory. However, as soon as infants begin to stabilize their reaches and posture (Thelen et al. 1996), A-not-B errors should be evident.

In the simulations, we assigned the developmental shift from errors to correct responding to the parameter of the field, $h$, that determined the balance of activation widths and strengths in the interaction kernel, and we gave it two discrete parameter values to shift it between the noncooperative and the cooperative regime. But we know from developmental evidence that there is an interaction between age and delay between hiding and search such that even 12-month-old infants will make the error if the delay exceeds 5 seconds or so. This would lead us to believe that the developmental changes in the motor field's ability to sustain a peak of activation are gradual, not switch-like as in the simulations. Indeed the exact point at which the planning field switches from a noncooperative (and thus perseverative) regime to a cooperative (nonperseverative) regime depends on all contributions to the resting level, not only on $h$. Different amounts of task input, controlled, for instance, by the attractiveness of both lids, affect this transition. Less attractive lids corresponding to less task input lead to lower resting levels in the field, which shifts the transition to larger values of $h$.

If so, we should be able to produce A-not-B type errors in infants and children much older than even a year, if we manipulate either the delay or the strength of the inputs. In particular, A-not-B errors should be evident with increasing age as the delay increases, the task environment provides fewer tonic specifications, and/or the specific input is less salient (i.e., shorter in duration, less attention-grabbing, etc.). This prediction is borne out in two recent studies. Smith et al.'s (1999a) use of the sandbox with 2-year-olds was described previously. In addition, Spencer and Hund (submitted) showed systematic response biases in 6-, 8-, and 11-year-olds and adults in a "space-ship" task where the task field was completely homogeneous and the delay between target and reach initiation was increased up to 20 seconds.

**5.1.5. The role of target distinctiveness.** As we stated earlier, there is suggestive experimental evidence that perseveration is reduced when the babies can more easily tell one target from another. In the following set of simulations, we systematically increased the distinctiveness of the targets: such a manipulation has not been done before experimentally. Then we report a test of the model predictions.

Figure 17 reports the relative memory strength to A measure from simulations generated as $c_{A/B}$ has been increased from 1 (both targets contribute equally) to 3 (targets contribute asymmetrically) in the noncooperative regime. When the targets are alike, a clear A-not-B error emerges on both B cues. At moderate distinctiveness, there are more spontaneous errors, that is, more incorrect reaches to B on A trials, seen especially in early trials, and a decreased tendency to make errors on the B trials. Indeed, infants are

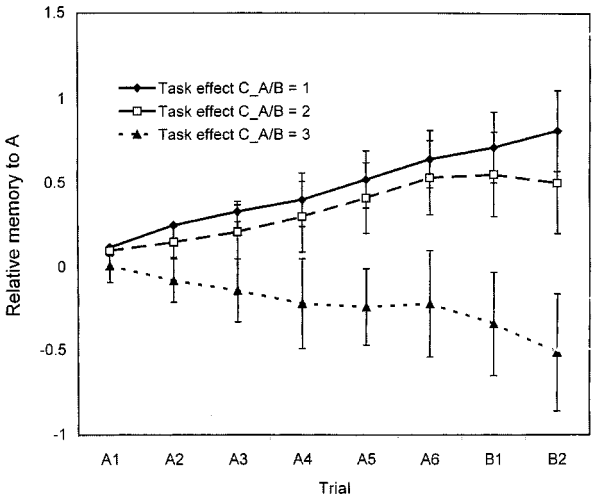



Figure 17. Relative memory for A for the effects of target distinctiveness. $c_{A/B} = 1 - 3$. Parameters: $S_{spec} = 4$, $S_{task} = 1$, $S_{mem} = 3$. Shown with standard deviations.

likely to be correct on the second B trial. This effect is highly pronounced in the highest degree of distinctiveness, where infants continue to respond at chance levels throughout the early trials, and because of their many spontaneous reaches to B, more likely to be correct on both B trials.

*Model predictions and tests.* These parameter effects of target distinctiveness were specifically tested by Diedrich et al. (2000a). One group of 9-month-old infants reached for one of two red targets on a red background. Three other groups performed the task with a B target that varied in its distinctiveness from the A target and background, which were always red. The B targets ranged from moderately distinctive (orange), to more distinctive (yellow), to highly distinctive (striped yellow and blue with red polka-dots and smiley faces). The percentage of A-not-B errors on the first B trial depended on target distinctiveness: 78% made the error in the red condition, 83% in the orange condition, 61% in the yellow condition, and 22% in the striped condition. Infants confused the red and orange lids, the yellow lid pulled the error rate down to chance responding, but infants were still making errors. In the striped condition, infants were likely to be correct – they could tell the A lids on the A trials and the B lid on the B trial.

Examination of the experimental memory strength measure shows that, as indicated in the simulations, the target distinctiveness may produce the discovered effects through two processes. First is through increasing the spontaneous error rate, and thus reducing the number of reaches to A (Fig. 18). This is seen with the striped lids – the most distinctive condition, where there is a dramatic diversion of pulls beginning by the second or third trial. Recall that in trials A1–A6, the infant is being cued with the red lid, but even when that lid is made more salient by pushing it forward, the striped lid attracts sufficient visual attention to cause many reaches to that target. With more reaches to B, there is a strong tendency to reach again for B on the B trials. In contrast, in the other three conditions, when infants are trained to the A side they tend to stick there; just the presence of the B lid is not sufficiently compelling to pull them away. And in the red and orange conditions, the A habit predominates even when they are specifically directed to B. However, the yellow lid does exert some coun-



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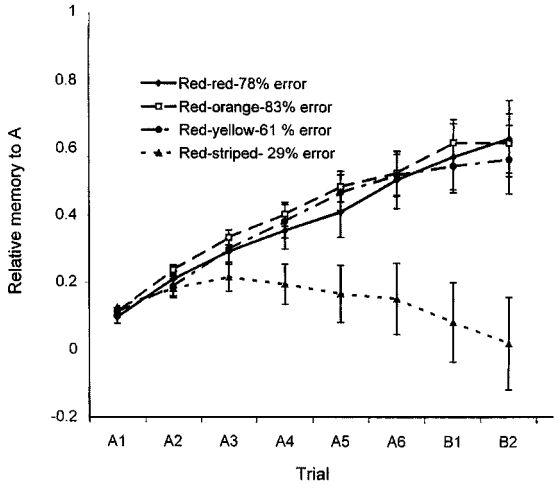



Figure 18. Experimental results on target distinctiveness. Relative memory for A measure from Diedrich et al. (2001).

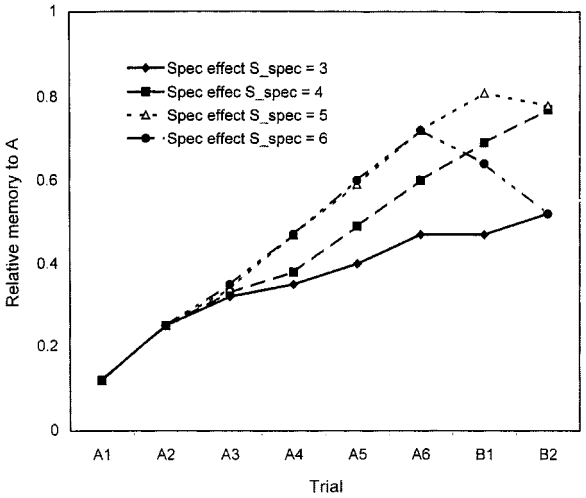



Figure 19. Relative memory for A measure for increasing specific input, $S_{spec}$ = 3 to 6.

terbalancing cue on B1 because more babies reach to B than when B was red or orange. The specific input cue partially overcame the perceptual-motor memory built up at A.

The ability to "know" whether to go to one location or another is thus exquisitely influenced by the nature of the tonic visual field. These influences are dynamic, however, and depend both upon the relative differences between the targets as well as the system's immediate history of looking and reaching. We show by the final set of simulations that similar complex and coupled dynamics are predicted for the remaining input parameters, those of the specific input.

**5.1.6. Strength of the specific input.** In contrast to the task input, the specific input is time-limited. The experimenter hides the toy or waves the lid and then this stimulus is gone. One assumption of the model is that the relative strength of the specific input should also contribute to the A-not-B error. A plausible prediction from the model is that, with stronger specific input, there should be *less* A-not-B error and less spontaneous error as well, because the salient cue would more strongly specify the target.

The simulations revealed, however, more complex and dynamic effects. Figure 19 shows the memory strengths from four simulations produced by increasing $S_{spec}$ from 3 to 6. For these simulations, we increased specific input the same amount for both A and B trials. Consider first, the two extremes. When specific input at A and at B was weak, the model made many spontaneous errors to B and was thus more likely to be correct on B trials. This is also shown by the high standard deviations in Figure 20. This simulates the course of events for the 20% of the infants who are correct in the canonical task, and suggests that one factor in their "spontaneous" errors may be the relative, transient salience of the particular cueing to A or B. In contrast, when the cues at A and B were highly salient, the model predicted correct responding on both A and B trials, as attention remained riveted to the cued side, revealed also by low standard deviations in Figure 20. This suggests that even younger infants should not err on either A or B when the cueing event is highly attention-grabbing. Behaviorally, younger infants with highly salient cues look like older infants who can retain the cue longer in memory.

The two intermediate values revealed a nonlinear effect. The canonical A-not-B effect is seen at $S_{spec}$ = 4, where there are some number of spontaneous errors on the A trials, but sufficient A responding to build a strong memory to that location so that it cannot be overridden by the B cue. The simulation at $S_{spec}$ = 5 demonstrates well the dynamic competition between inputs and memory. Here the cue is strong enough to keep A responding mostly correct, but the B cue is just not quite attractive enough to counter the growing A memory and the model predicts errors on B1 and fewer errors on B2, where variability was especially increased.

We can account for these complex results by considering more closely the changing dynamics, where several interacting processes are likely at work. First, recall that the motor field continues to evolve over the delay period under the influence of the three inputs, task, specific, and memory (e.g., Figs. 13 and 14). But the impact of the specific input is most potent at the very beginning of the trial. This generates a clear empirical prediction: with stronger specific input *and with no delay on any of the trials,* infants should

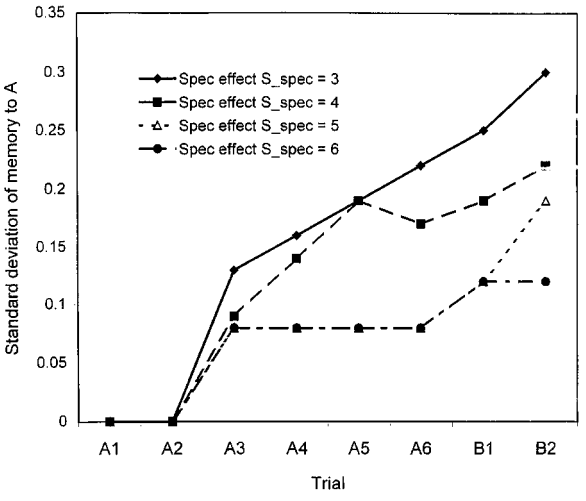



Figure 20. Standard deviations of the A memory shown in Figure 19.


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Table 1. *Predicted interactions between specific input and delay*

| Levels of specific input at A and B, from lowest (1) to highest (4) | A-not-B errors | |
|---|---|---|
| | No delay | Delay |
| 1 | R | E |
| 2 | R-C | E |
| 3 | C | R |
| 4 | C | C |

R = random responding on B1
E = errors on B1
C = correct on B1

be more often correct on the A trials and on the B trials as well because their decisions will be dominated by the specific input. The specific input has other, less direct, effects, as well. Weaker specific inputs at A create a comparatively weaker pull to that side and more noise-induced spontaneous errors. Additionally, and in concert, as the field evolves, the memory trace left by strong specific inputs exerts a stronger influence to the A side, also decreasing the spontaneous errors. However, these effects would tend to increase A-not-B responding, which is not what the simulations produced.

The simulations suggest that the effect of the stronger A memory built in the A trials is counteracted by the also stronger B cue at the B trials at the higher levels of $S_{spec}$. In other words, the effects of the specific input are nonlinear: at low and moderate levels, and with delay, the memory input dominates and the B cue cannot overcome it. However, at higher levels of specific input, and despite the clear good performance at A, the cue at B will be sufficient to cause correct responding. This is certainly consistent with the experiment of Smith et al. (1999b) described earlier, where diverting infants visual attention at the B trials could reinforce or perturb the A-side habit. The B cue here clearly acted to increase the specific input to one side or the other.

These simulations reveal intricate dynamics whose plausibility we are currently testing by empirical studies. The studies involve manipulations of the relative strengths of the specific inputs by varying the saliency of the stimuli (e.g., highly colored flashing lights vs. brown bottoms of the lid), their duration (long vs. short presentations) and the delays between terminating the specific input and allowing the motor field to evolve (1 sec vs. 3 sec). The logic of the experiments and the predictions are summarized in Table 1. Preliminary results strongly support the predictions (Clearfield et al. 1999). Such experiments can be followed by another class where the strengths of the specific input are directly pitted against the memory dynamics, such as was done by Munakata et al. (1997) where a toy at the B trial was tested against a plain lid on the A trials.

## 6. Discussion

### *6.1. Strengths of the model*

**6.1.1. Realistically simulates all A-not-B results.** The model simulates the known age, delay, and context effects of the well-studied Piagetian Stage IV error in a very powerful way. It also captures additional new effects discovered by Smith et al. (1999b) in experiments designed from a qualitative dynamic perspective. These include (1) hidden objects are unnecessary; (2) the task is novel and perceptually confusing; (3) reaching and looking are coupled; (4) the reach memory is body-coded; and (5) repetition is essential. The model accomplishes all this without invoking constructs of "object representation," or other knowledge structures. Rather, the infants' behavior of "knowing" or "not-knowing" to go to the "correct" target is emergent from the complex and interacting processes of looking, reaching, and remembering integrated within a motor decision field. Moreover, the model accounts for the contextual variations discovered in dozens of studies within a single set of dynamic equations. This does away with the need for invoking new cognitive strategies for the discontinuities between performance, for instance, at no-delay versus delay or between the A and the B trials (i.e., "direct" vs. "inferred" search strategies). The nonlinearities inherent in dynamical interactions produce overt behavioral switching as a result of small changes in parameters and as a function of the time-evolution and previous history of the system.

Likewise, the model shows explicitly how behavior that looks like inhibition or lack of it can arise from processes where no explicit inhibitory mechanisms are switched on. Inhibition plays an important role in Diamond's (1985) influential account. In particular, Diamond maintains that the ability to inhibit reaches to A increases with the age-related maturation of the prefrontal cortex. The puzzle, however, is in the A-not-B task, infants look like they are unable to inhibit their reaches to A at some ages, with some delays, and with certain task features, but can inhibit A responses at the same ages, under different circumstances. It is not clear in Diamond's account, why, if prefrontal maturity alone is the critical element, the number of A reaches or the distinctiveness of the targets should matter so profoundly or what indeed switches inhibition on or off at any given age. In the dynamic field model, inhibitory connections are embedded in the interactions in the field. But the behavior of either reaching or not reaching is emergent entirely from the dynamic interactions between activation processes and their changing strengths over time. We need not manipulate inhibition per se. Both age- and context-effects are simulated in exactly the same regime.

**6.1.2. Reaching errors come from general processes.** A second important strength of the model, therefore, is to situate this particular developmental phenomenon – perseverative reaching under particular conditions – within the continuum of normal *processes.* We demonstrate that the error can be understood in both its "classic" form and in all its previously perplexing variations *entirely* by the dynamics of these processes. We are able to account for known and quite subtle interactions between, say, delay timing and age, but we also make entirely novel predictions about the relative contributions of the visual input, number of reach repetitions, and delay as well. These complex interactions would have been difficult, if not impossible, to understand without the formal properties of the model. In addition, the model accomplishes these matches with relatively realistic parameters based on known experimental effects, for example, the trial, inter-trial, and delay times and specific input parameters.

Although we have devoted this paper to the developmentally-specific A-not-B error, the model was adapted



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from one originally formulated to account for much more general issues of movement programming. (The primary modification here was to take into account the task input, to deal with delays, and to vary the degree of cooperativity leading from an input-driven to a self-sustained mode of operation.) In its original form, the Erlhagen and Schöner (2001) model captures an impressive number of important and well-researched phenomena associated with motor planning, but conventionally described in information-processing terms. These included not only the Ghez effects we described in an earlier section, but also many basic reaction time findings such as increases of reaction time with increased number of choices, decreased probability of choices, and increased distance between targets, as well as many different priming effects, stimulus-response compatibility, and pre-cueing experiments.

That the very same dynamics that produce these fundamental aspects of adult perceptual-motor functioning also give rise to the developmentally-constrained A-not-B error gives strong support to our contention that the error is not one of object representation in the accepted Piagetian sense. Rather, the error appears as part of a continuum of developmental changes affecting reaching, looking, remembering, and the planning for action. One assumption of the infant model is that there are changes in the ability of the movement planning field to sustain an activation peak in the absence of specific input. Once the cue – either waving a lid or hiding a toy – has disappeared, infants must hold the location of that cue "on-line" for a few seconds until the target is reachable. Clearly, this ability to keep relevant, but no longer visible, stimuli in memory is a critical developmental achievement. The degree of cooperativity of the field may have other potentially powerful developmental effects as well. For instance, limited cooperativity leads to an increase of the effective time scale over which the field builds up, as the excitatory action contributes relatively little to the growth process. This results in a relative "sluggishness" of the field and thus of infants' abilities to reach a threshold activation sufficient to generate a reach. Informal observations have indicated dramatic differences between new reachers and 1-year-olds in their ability to generate a reach once the target is presented. Young infants intently stare at the attractive toy, open their mouths in anticipation of the toy, and yet they may take many seconds to actually begin arm movements directed to it. Older infants, in contrast, rapidly produce an appropriate movement (Thelen, unpublished observations). We believe that the parameter changes that lead to this decrease in infant "reaction time" may be part and parcel of the same processes that produce perseverative reaching errors.

But other parameters may be undergoing developmental change as well. The contribution of developmental changes in the perceptual differentiation of the targets (Gibson 1969) is unknown, for instance. However, it is reasonable to assume that 6-month-olds view two brown lids on a brown box differently than do 12-month-olds. Experience interacting with many different objects and, as locomotion develops, viewing the world from many different perspectives must produce the ability to distinguish more action-relevant features. The model allows us to simulate and then empircally test not just the presumed developmental changes in the field cooperativity, but also changes in the relative activation spread in the input fields, a measure of the relative distinction of the targets.

Finally, we believe that the A-not-B-type task can provide a sensitive assay for infants' changing abilities to form perceptual categories, and therefore, a way to investigate the functional integration of the "what" and "where" visual streams. Based on the model, we understand that graded differences in both the task and specific input are coupled together in the decision field for action and that these inputs are retained in memory along with parameters of movement. Thus, if we ask infants to repeatedly reach for one of two brown, circular lids several times, what would happen if we then substituted, say, a brown star-shaped lid for the first B stimulus? By asking when they "notice" a difference in a second test stimulus after they have become accustomed to the first, we can probe the nature of the stimulus memory in the context of reaching in a specified location. Is a star-shape sufficiently distinct as an object to-be-reached-for to pull infants away from the original habit? Could we get infants to generalize to a class of any novel brown-colored lids by switching shapes on each A trial? Could we get them to ignore novelty completely by appropriate training or, conversely, might training on variable targets retard the specific spatial perceptual-motor memory altogether? Many other variations are possible. This procedure is logically similar to the frequently used habituation-dishabituation paradigm, where infants are allowed to look at a target until they lose interest and then they are tested with a second more-or-less different target. The attractive difference is that with A-not-B type techniques (for infants who can reach) the infant remains keenly interested in the task throughout, which eliminates a motivational confound.

**6.1.3. Biological plausibility.** The model as it stands is neutral as to an anatomical instantiation in the central nervous system; it is a model of the behavioral dynamics. The leap from behavioral dynamics to neural anatomy must be undertaken with great caution. Nonetheless, we believe that there are two ways in which the behavioral dynamics are biologically realistic and fully compatible with processes occurring at the neural level.

First, dynamic field concepts can provide a plausible depiction of behavior of populations of neurons representing simple movement parameters such as movement direction (Georgopoulos et al. 1988) or extent (Erlhagen & Schöner 2001; Fu et al. 1993, for further discussion). The usefulness of such concepts was recently demonstrated by Bastian et al. (1998) in an experiment where monkeys were given varied amounts of prior information about upcoming movement tasks. Recorded activity from populations of neurons in motor and premotor cortex showed that this information biased the dynamic population field before the movement task itself was cued, much like the memory and task inputs preshape the field in the current model.

Having suggested this potential link to neural population activity, we want to reiterate that at this point, our model is not anatomically specific, but is best conceptualized in terms of the entire ensemble of processes involved in the A-not-B task. These coupled processes – attention, motivation, visual processing, planning, moving, and remembering over several time scales – surely require the entire, densely connected, and multiply re-entrant circuits of the brain. For instance, at least eight distinct motor fields have been identified in primate frontal cortex and each receives varying inputs from frontal and parietal visuospatial cortical regions as well as multiple connections from basal gan-


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glia and cerebellum, all of which are critically implicated in motor sequencing, timing, planning, memory, and learning (see, for example, Jeannerod 1997; Schwartz 1994). In the last few years, studies in awake, behaving monkeys have clearly demonstrated that the kind of integrative processes we propose in the model can be detected widely in the brain, and especially in areas long thought to be primarily motor. Using tasks that have much in common with the A-not-B error, these studies suggest that motor cortical areas may be deeply involved in both sensorimotor transformation *and* a retention of the stimulus in memory. Table 2 summarizes a sample of such studies in four regions of the cortex. In each study, investigators recorded activation of single neurons or neural populations in different reaching tasks. In some, delays were imposed between stimulus and response, to probe the maintenance of the stimulus over the delay. In others, monkeys were given instructional sets to disassociate the actual movement direction from the stimulus direction. An entry in the table means that researchers found significant neural response to the identifiable aspects of the task.

Table 2 makes clear that the kinds of integrated neural activity assumed by the infant model is widely distributed, even in brain areas traditionally believed to be primarily "sensory" or "motor." The traditional distinctions do not hold (see Jeannerod 1997, for a full review). Each area has neurons responsive to nearly all aspects of a reach-over-delay-type task: cells in the motor cortex respond to the visual stimulus and hold a cue over delay and the prefrontal cortex has neurons that respond to movement direction. Likwise, neuroscientists have revised their views of function of parietal cortex. This area, long thought to be a site of visual integration, is now believed to both abstractly represent space and to form motor plans and intentions (Andersen 1995; Snyder et al. 1997).

### *6.2. What develops?*

**6.2.1. The role of the prefrontal cortex.** The issue of what brain regions support the sensorimotor and memory integration suggested by the model is important because, based on the work of Diamond (1990a; 1990b; Diamond & Goldman-Rakic 1989), the A-not-B error is believed to be a hallmark task for dorsolateral prefrontal cortex maturation. The heart of Diamond's argument is the similarity between infant perseverative behavior and the kinds of errors made by monkeys and adult human patients with prefrontal lesions in similar, but not identical tasks. Both groups have difficulty remembering a stimulus over a delay and seem unable to inhibit prepotent responses. (Human prefrontal patients have many additional problems with attention, sequencing of actions, planning, and emotional modulation [Damasio 1995; Fuster 1989].) We believe that the issue is not whether the prefrontal cortex is involved in short-term memory and action planning, for the evidence is incontrovertible. Rather, we question whether the developmental changes seen between, say, 8 and 12 months in the A-not-B task can be confidently attributed to maturational changes in prefrontal cortex alone, so that it is now assumed to be a marker of such maturation (Diamond et al. 1997).

There are several reasons why we think such a conclusion may be premature. First, it is true that infants perseverate,

Table 2. *Neural activity in four brain areas related to sensorimotor functions in A-not-B task*

| Neurons encoding: | Motor | Premotor | Parietal | Prefrontal |
|---|---|---|---|---|
| Visual stimulus | | | | |
| cue-based | 12, 16 | 4, 10, 17 | 1, 11 | 4, 5, 13 |
| set-based | 16 | 4, 17 | 1, 11 | 4, 5 |
| location-specific | 16 | 10, 17 | 1, 11 | 5 |
| object matching | | | | 13 |
| Look | | | | |
| eye position or saccades | | 2 | 1 | 5, 13 |
| Delay | | | | |
| cue held over delay | 12 | 4, 10, 12, 17 | 1, 11 | 4, 5, 13 |
| Plan | | | | |
| active before move, onset | 7, 8, 16 | 3, 4, 17 | 1, 11 | 4, 5 |
| Reach | | | | |
| direction | 6, 16 | 3, 17 | 1, 11 | 5 |
| force | 9 | 15 | | |
| arm posture | 5, 17 | 10, 15 | 11, 15 | |
| somatosensory | | 10, 17 | | |
| Remember | | | | |
| memory of previous acts | 16 | 17 | | 13 |

Source: 1. Anderson et al. (1979); 2. Boussaod (1995); 3. Caminiti et al. (1991); 4. Pellegrino & Wise (1993); 5. Fuster (1989); 6. Georgopoulos et al. (1988); 7. Georgopoulos et al. (1989a); 8. Georgopoulos et al. (1989b); 9. Georgopoulos et al. (1992); 10. Graziano et al. (1977); 11. Kalaska & Crammond (1995); 12. Kettner et al. (1996); 13. Miller et al. (1996); 14. Scott & Kalaska (1997); 15. Scott et al. (1997); 16. Shen & Alexander (1997a); 17. Shen & Alexander (1997b).



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and so do lesioned monkeys and humans. But there is little evidence to show that infants make perseverative errors using the same underlying neural mechanisms as adult primates with the loss of a piece of adult brain. It is seductive, but dangerous, to assume that an animal lesioned after thousands of trials of training equals an inexperienced infant. The experience-dependent and epigenetic processes of brain development cannot be reversed by lesioning. Moreover, the evidence from lesioning infant monkeys is equivocal (e.g., Malone 1994). For all of these reasons, we believe it is theoretically inappropriate to try and "lesion" the model to simulate prefrontal patients and monkeys.

Second, the problem of circularity. The evidence that prefrontal cortex "matures" between 8 and 12 months is performance on A-not-B type tasks at the same time that A-not-B tasks are taken as the hallmark of prefrontal maturation. Indeed, when measured by a host of other tasks (which are also deficient in frontal patients) such as Tower of Hanoi problems as well as by anatomical, electrophysiological, and imaging techniques, the progression of prefrontal cortex toward adult-like states is very gradual, extending into adolescence (Malone 1994). What task and what aspects of that task, then, can be independently labeled as containing the essential elements of prefrontal maturation?

Diamond et al. (1997) identify two criterion responses that are both necessary and sufficient to mark prefrontal involvement: remembering the hiding location over a delay and the ability to inhibit the previous responses. But our model and experiments suggest that both of these seemingly fixed properties of the task are not fixed at all, but entirely dependent upon the way the experimenter structures the targets, the cues, and trial sequences. This is a particularly critical issue because Diamond's version of the A-not-B task seriously conflates the two aspects of memory identified in the model. Diamond's procedure is complex (see Diamond et al. 1997), but essentially presents the infant with two trials at A with a certain delay, after initial training to uncover a hidden toy. If the infant correctly reaches twice to A, the infant gets a switch trial, where the toy is hidden at B. If the infant is correct on all three trials, then the delay is increased with the two A trials now positioned at the former B location. If the baby was incorrect, the delay is decreased. This procedure is repeated until experimenters find the delay at which infants were correct on A trials, but made the error at B, and this delay is what determines their prefrontal functional level. Diamond adds elaborate rules for determining how many reversals, how many correct trials and how many perseverative strings to one location are allowed before the experimenter changes the procedure. The problem with this procedure is that, while providing multiple reversals to test perseveration, it is completely uncontrolled as to the number of actual reaches to either A or B before any particular "test" reversal, and these data are not reported. As we have seen in both model and experiments, the number of elicited and spontaneous reaches to each target is absolutely critical in building up the memory input, and there is a complex relationship between the delay, the cue, and the preshaping of the field with the history of reaches. Say, for instance, an infant is tested at a 3-second delay and reaches correctly to both A and to B. The experimenter would increase the delay to 5 seconds and test with two more reaches to B and a reversal to A. If the infant then makes the error, Diamond would interpret this as a prefrontal deficiency involving inhibition or delay. But, on average, given the strict relationship between number of reaches and tendency to stick (Smith et al. 1999b), the baby would make the error at 5 seconds because he or she would have reached more recently three times to B. But the baby also might have made the error at 3 seconds, because the delay and the number of repetitions are not independently controlled. So it is impossible to tell whether failure is due to delay or to repetition, or success is due to chance. As these procedures commonly make 12 or 15 target switches to establish the criterion delay, each involving three or more reaches, the infant ends up reaching many times to both A and B. The tendency of the infant to reach *randomly* to A or B is greatly increased. In addition, to keep the infant's interest in the procedure, Diamond et al.(1997) sometimes change the toy that is hidden. If our model and experiments are correct, this changes the strength of the specific input, which itself has a complex and nonlinear interaction with repetition and delay. All of these manipulations matter, and thus, the experimenter's ability to interpret either switches or perseverative strings as caused by either delay or inhibition is seriously compromised.

**6.2.2. Modeling developmental change.** Infants will reach correctly over longer delays as they get older. This fact is not at issue. What is still open for discussion is by what mechanisms this change comes about. The lesson from the model, the experiments, and from Table 2 is that there are multiple parallel, integrated mechanisms contributing to this performance at any time and in any particular context. Thus, there may be, and probably are, many avenues of change.

In the present model, we capture these distributed processes in a single integrative motor planning field. In particular, we simulate the appearance and disappearance of errors by changing $h$, the field's cooperativity. This is the heart of a dynamic model: a small change in one parameter value creates an instability that drives the field into a new regime, the ability to self-sustain the stimulus input. Manipulating the $h$ parameter raises the resting level of the field, and thus increases the weight of the field interactions relative to those of the inputs to the field. In this sense, we are changing the relative influence of the internal, mental processes over those dominated by the input alone.

The question then becomes: What might be driving such a developmental change? Behaviorally, infants look as though they gain an increase in working memory, and it is seductive to attribute this to autonomous "maturational" changes. But the process may be more complex and contingent. For instance, recall that the simulations and experiments showed that 9-month-old infants perseverated or not as a function of the visual task parameters. This cannot be attributed to maturational improvements in working memory. Rather, an especially salient target or cue acted to change the balance between the internal dynamics of the field and those of the input so that the input dominated. In this sense, the strength of the input determined the strength of the memory.

This shift from perseveration to non-perseveration with a real-time manipulation may hold one clue to what might be happening over developmental time. For example, we know that with development, perceptual abilities become more refined (Gibson 1969). With perceptual learning, infants may better distinguish the two hiding covers. Thus, what might initially be represented as two broad, diffuse


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bands of overlapping excitation might at a later time be characterized as more sharply focused, stronger peaks of activation. Because of the connectivity of the field, stronger input at these sites increases the interaction effects and can lift the whole field above the *h* threshold, with the resulting increase in cooperativity. A stronger attentional mechanism would have a similar effect if increased attention resulted in stronger perceptual activation. Indeed, the process of perceptual learning may be reflected in changes in the connectivity itself: As perceptual discrimination sharpens, the local sites of excitation may be pruned back, leading to more sustained and localized peaks of activation, again with the behavioral appearance of enhanced working memory.

But there are other ways as well in which infants' experiences may lead to improvements in A-not-B performance. Between 8 and 12 months, infants undergo many relevant and important behavioral changes associated with reaching for objects and remembering spatial locations. During this period, infants gain a measure of control over their arms so that their reaching stabilizes to become more direct, smooth, and appropriately timed (Thelen et al. 1996). They learn to sit independently and to use their stable torso as a base of support for reaching in many areas of the space without tumbling over. They learn to shape their hands in anticipation of objects to be reached and then to differentiate the fingers to pick up small items. They begin to break out of the nearly obligatory object-in-the-mouth movement to hand-to-hand transfer, and fine manipulation. They start to incorporate manual actions with locomotion such as crawling and walking. And they begin to have highly differentiated manual activities with objects of different properties, such as squeezing soft toys and banging noisy ones. It is a time of active exploration of the properties of objects by acting on them and of active exploration of space by moving through it. It is likely that these actions would profoundly change all areas of the brain, but especially those directly involved in attention, perception, planning, movement, and memory, including, but not limited to those areas mentioned in Table 2. Any or all of these multiple developmental advances can be envisioned as "priming" the field similar to the simulation of the resting level, *h*. Alternatively, such activities may work to change the dynamics of the motor memory so that there is more rapid decay between trials. Perhaps these changes occur primarily in the prefrontal cortex, but at this time, there is no direct evidence – except analogies to lesioned animals – to support this.

### 6.3. Limitations

The model, therefore, while biologically plausible, captures an integrated behavioral outcome. It is not anatomically specific and is not directly couched in terms of central nervous system structures and circuits. Whether anatomically-driven models such as the powerful ones of Bullock and Grossberg (1988) can also simulate the infant behavioral dynamics is an important question and one that we would hope to see addressed.

Second, given our emphasis on the motor aspects of this task, the model is incomplete in not yet coupling with the trajectory dynamics of the actual reach that results from the motor plan. This is important because the memory that builds to capture subsequent reaches is not just a spatial location memory and a memory of the task space, but also some lingering trace of the trajectory of the arm going toward the target (Diedrich et al. 2000b). Moreover, as reported in Smith et al. (1999b), the motor memory embodies the infants' postural set as well so that postural manipulations perturb the location memory. It will be a major challenge to actually bridge the mental dynamics of the plan and the execution dynamics of the reach. We have taken the first step by casting the mental events in compatible dimensions.

Third, the model is limited to a single parameter dimension, that of spatial location. It is a "where" stream model, in the language of the classic division of visual processing into a dorsal "where" stream and a ventral "what" stream. In this sense, we have turned the traditional interpretation of the A-not-B task as one of object recognition on its head. In the model, we have deliberately interpreted the hiding event as a stimulus that increases activation at a "where" site, making that position more or less salient, and between cookies and toys as one of intensity, not object identity. But, in everyday life, "what" and "where" components of actions are completely interwoven. People do not just reach out for locations, they reach for particular items, and the nature of the objects and people's intentions with them determine the characteristics of their manual actions (Castiello 1996). Somewhere in the final set of motor plans, these aspects of the visual world must be melded together (see Jeannerod 1997, for a contemporary interpretation of visual motor processing). As we mentioned previously, A-not-B type experiments have important potential for understanding how this integration develops, and thereby helping to blur the distinction between pure "knowing" and mere "acting." This might be accomplished by experiments where targets similar in salience but differing in object properties are tested using various parameters of delay and repetition. Theoretically, we believe that dynamic field models can in principle be expanded and extended to encompass the semantics of action decisions as well as their spatial aspects. The driving assumption here is that such semantic content is represented in the same fluid, continuous, and time-based spaces as those integrating the multiple contributions to the spatial reaching decision of the A-not-B error.

Finally, the present model is restricted to static visual targets. Infants are more adaptive. They can actually intercept moving targets at remarkably young ages (von Hofsten 1983). Moreover, they can reach in the dark to auditory targets after they have seen them first specified visually (Clifton et al. 1993). We believe the model can accommodate these special conditions and we are currently pursuing this line of work.

### 6.4. Comparison to other models of the A-not-B error

Our model shares a basic assumption with two connectionist models of A-not-B performance, but there are fundamental differences as well. Both the earlier Dehaene and Changeux (1989) and the more recent Munakata (1998) models are similar to the present one in suggesting a competition between the history of the system and the current perceptual specification. In the first, Dehaene and Changeux (1989) simulated both the A-not-B error and the delayed-match-to-sample behavior using a simple two-layer connectionist model. The A-not-B error arises from the structure of the first layer, which establishes a direct stimulus-response linkage and works by Hebbian learning. During



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each A trial, the association between the sensory representation of the A location and the grasping response is strengthened. After a number of trials to A, the response is so strong that the B representation cannot override it. Behavior is determined by the history of success. The second layer contains an active memory component whereby the memory of the specific association can be maintained during a delay. When this layer is connected to the first, the perseverative response to A can be counteracted by the current association. Younger infants make the error because their second layer, presumed to be located in the prefrontal cortex, is not yet connected.

There are several ways in which the Dehaene and Changeux model is unrealistic. First, the model learns on the basis of reinforcement for correct responses and punishment for incorrect ones. Reinforcement, that is, being "correct," does not seem to matter in real infants, and indeed, researchers have not identified what aspects of the A-not-B task are actually motivating. Infants may not distinguish between grabbing the "wrong" cover or just reaching itself, from obtaining the hidden toy. Our model does not need reinforcement or punishment to get the perseverative error. Second, their model situates the error in a different process (layer 1) than the one associated with correct responding (layer 2). Since the layers are either connected or not, the model cannot account for the many graded and contextual effects that are the hallmark of this task. Our model well captures the interactions between the stimulus situation, motor field characteristics, and memory within a single system. Finally, and related, they cannot account for the delay effects. In their scheme, young infants will make the error no matter what the delay. In contrast, we can quite naturally explain delay effects and, again, the important interactions between delay and other aspects of the task.

Munakata's (1998) connectionist model is more successful in simulating the real-life variations in the A-not-B task than the one offered by Dehaene and Changeux. Her current model is a departure from a previous connectionist model of Munakata et al.'s (1997) where the A-not-B error was conceptualized solely as a failure of object representation strength. In the more recent version, Munakata (1998), inspired by Smith et al.'s (1999b) finding that hidden objects are unnecessary, rejects "infants' knowledge of an object's location . . . as a reified entity, disembodied from underlying processing mechanisms" for an account based on two types of memory mechanisms, active and latent. The model consists of a six-layer architecture. Three input layers encode information about the object location, cover type, and toy type. The one hidden and two output layers contain units representing locations A, B, and C. The output is mapped onto both reaching and gaze or expectancy layers which differ in their frequency of response, since infants can presumably always look, but not always reach. The hidden and output layers have both inhibitory connections between units and self-recurrent excitatory connections back to each unit.

Starting with a small initial bias toward correct responding, the network develops an association between the input location and the motor response by Hebbian learning adjustments to the connection weights. As in other models, the repeated presentation at the A location strengthens the mapping to the A response. Because the B response stimulus shares input representation, when B is cued, the more potent A networks respond and the error occurs. The developmental change is modeled by changing the degree of recurrence in the network. With more recurrence, the activation of the units changes more gradually over time and the active memory can be maintained longer over the delay. This leads to a decrease of the influence of the A-latent memory and an increase of relative strength of the B-active memory. At low levels of recurrence, the B representation fades more rapidly than the latent A memory, accounting for the delay effect. Finally, Munakata (1998) can simulate the effects of multiple and distinctive covers by reducing the degree to which the sensory representations overlap in the network. Overlap is necessary for the B cue to activate latent A representations.

Thus, despite the different instantiations, there are strong similarities between Munakata's model and our own. Both have two forms of memory that interact, giving rise to the delay. Both simulate the age effects by comparable mechanisms allowing for self-sustained representations of the cue. And both can assign parameters to aspects of the input that result in shifts from error to no-error performance within the same system.

Nonetheless, there are also some non-trivial differences (Smith & Scheier 1998). First is our adoption of a unified field in motor space that integrates the visual aspects of the task based on experimental results that looking and reaching are coupled. The unified field is the essential notion of embodiment. Munakata, in contrast, assumes two disassociated systems for looking and reaching, which do not interact. She does not represent target location in the same metric as body-location, which makes it impossible to incorporate the peculiarly egocentric and syncretic nature of infant reaching (Diedrich et al. 2000; Smith et al. 1999b). Second, Munakata's model does not take into account the persistence of the task input, separate from the actual cue. Thus our model can, without further additions, simulate situations where both the specific input and the distinctiveness and/or location of the covers is parametrically varied. Finally, Munkata's model does not attempt to model the stochastic nature of real performance, that is, that babies, when cued to A, sometimes reach to B, and that this chance B behavior influences the next reach. Our model behaves like real infants not only in the outcome of error or no error, but also in the real time, trial-by-trial execution of the behavior.

## 7. Conclusions

Readers who have made it to this point have perhaps asked themselves along the way, "Why all this fuss about such a simple little task in infants?" As the considerable army of A-not-B researchers have all discovered, the task and its infinite subtleties are endlessly intriguing. This was true when the A-not-B error defied a unified explanation, and is even more so now that we are moving closer to such an explanation. This is because, thanks to Piaget's genius, we have an entry into much more general processes of perceiving and acting by observing them in human creatures who are not fully skilled.

There are two take-home messages. The first is that both the robustness of the task and its exquisite context-sensitivity are emergent from the dynamic processes that produce decisions to act in the face of a rich visual scene and the recent and more distant memories of similar actions. But this


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

snapshot of a developmental moment in infancy is revealing only as it illuminates issues inherent any time we use an experimental task to probe the contents of mind. If the A-not-B task can be understood in terms of these coupled dynamic processes, then similar analyses can be applied to a large class of tasks used by infant researchers to determine what infants know.

For instance, we have situated these dynamics in motor plans for reaching. But the model is equally suited to the dynamics of eye and/or head movements, which are, of course, motor decisions as well (see Kopecz & Schöner 1995; Kopecz et al. 1995; Schöner et al. 1997). Extending this model to gazing would be a particularly critical theoretical breakthrough, because much of what we know about infant cognition is garnered through looking experiments, using both static displays and events. There is a widespread assumption, for example, that experiments using preferential looking or, more commonly, habituation/dishabituation designs, are a more sensitive indication of infants' core "knowledge" than their behavior in the more demanding reaching experiments (see Bertenthal 1996; Munakata et al. 1997, for example). Thus, the disassociation between what infants show that they "know" by looking and what they "know" by reaching is attributed to the performance demands of a manual action, while looking remains unfettered by motor constraints. But in a very real sense, looking at or away from an event display is a motor act integrating attention, vision, and memory processes just as much as reaching toward one place or another. The motor fields are different, but the activities are formally identical and should be captured by the same class of dynamic equations. Likewise, a formal model should generate precise predictions about how the parameters of the task change the outcome, and should reflect the delicate nonlinearity we have demonstrated here. By this reasoning, neither looking nor reaching provides a direct readout of the contents of mind. Both are constituted on-line, within the moment, the bounds of the task, and the child's history in similar situations. Looking tasks, like the A-not-B, can tell us a lot about how these processes work together, but they cannot claim privileged access to the enduring contents of mind.

### *7.1. Embodiment: Are we there yet?*

The second message of this analysis relates to the embodied nature of mental activities. How, the reader may ask, can we stake a claim to embodiment when our model does not incorporate the behavioral output in terms of the kinematics or kinetics of the actual movement?

We acknowledge again that a full model of infant reaching would integrate the decision level with the real-time control of the movement. Indeed we have reason to believe that this can be accomplished. First, in their original version of the dynamic field model, Kopecz and Schöner (1995) linked the levels of visual information and motor planning to the actual control of gaze in saccadic eye movements. They did this by having the motor control level align the gaze to the planned position by generating an eye trajectory with an appropriate endpoint. Of course, the motor control of three-dimensional, multi-joint reach trajectories is considerably more complex than that of one-dimensional eye saccades. Thus, the linkage between the decision level and the control of the movement is more difficult to model; but the difficulty, we believe, is in identifying the appropriate parameter space at the motor control level. When the movement parameters are well-defined, the field model can link decision to output (and output back again to decision).

The success of the field model in this regard has been quite dramatically demonstrated in robots. Schöner et al. (1995) implemented this linkage from information to planning to movement using an autonomous, mobile robot controlled by dynamic field architecture. The architecture allowed the robot to approach a target and avoid obstacles by means of field dynamics that integrated visual input, memory, planning, and generated movement. Finally, with Wolfram Erlhagen and Estela Bicho, we recently adapted this robot to simulate infants' A-not-B behavior, using the current field model architecture. The robot, equipped with microphones, faced two tonically active sound sources (A and B), analogous to the task input. One sound source (A) signaled the robot with a short tone, similar to the specific input in the visual simulations. These two inputs were integrated in the field. After the simulated delay period, a homogeneous input ("go" signal) drove the field into a decision-making mode in which a single, localized peak emerged. Once this peak reached a criterion threshold, the robot moved in the specified direction, to A or B. The probabilities of deciding to go to A or B were similar to those seen in real infants. Then, the read-out of the decision field – the actual heading direction of the robot – was used as input in subsequent trials as memory of the just-completed direction. After a number of successful A trials, the robot perseverated when subsequently cued to B, as did the infants. Importantly, the robot could be made to perserverate or not by the parametric settings of the input and the critical $h$ parameter of the field.

It is gratifying that the model could be implemented in a real-world, moving device. There is, however, even a deeper sense in which the robot (and the simulations) are models for embodiment. This is because the dynamic field acts, in the words of Andy Clark, to "simultaneously describe aspects of the world and prescribe possible actions" (1997, p. 49). In other words, as the field is the site of integration of the external world and the memory of previous actions in it, no other stored maps or central controllers need intervene. The world and experiences in the world are both the controller and the controlled. The consequences are that the division between what is "conceptual" and what is "perceptual-motor" may be very hard to draw. Perception and mental planning contribute to a decision to act – the essence of human cognition – but the memories of actions are equally involved. Most important, both the history of acting and the current situation are expressed in the same action-centered dynamics. If the model captures any truth about how acts are generated, then body memory is encoded in these densely-connected fields from the start.

From this point of view, then, the critical developmental process may not be transgressing some line dividing the conceptual and the perceptual-motor – the traditional issue – but the ability to use memory and to make decisions off-line when the situation demands. It is obvious that there are occasions when mental activity is well-served when less dominated by the immediate perceptual scene and less tied in an obligatory way to what was just done, or when immediate action is not warranted. Under such circumstances, the dynamic field might play out primarily under the influence of the experiential pre-shape, the memory of earlier



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

responses in such situations, without the immediate input and without reaching the threshold for movement. On the other hand, people face many other occasions when a tight "clamping" to the environment, as Glenberg (1997) recently called it, is highly desired. There are circumstances when too much contemplation puts one at risk for life and limb! But everyday life is a dynamic mix between the more mental and contemplative and ongoing action. Indeed, skilled people shift rapidly and effortlessly between these modes: between acting immediately and tightly coupled to the input, and delaying action in favor of remembering and planning.

We suggest that people can shift between on-line and off-line so frequently, so rapidly, and so seamlessly only because acting and thinking are in commensurate dynamics. The alternative is that the two modes are incommensurate: symbolic and conceptual codes for the purely "mental" part and dynamics for the perceptual-motor. How do they seamlessly interface? We come up against both the symbol-grounding problem and its inverse: how do symbols (concepts) arise from perception and how does the symbolic (conceptual) code get transduced into the dynamics of movement?

Development surely involves acquiring the ability to hold aspects of the environment in memory and using those stored memories to plan new actions. But infants must also learn to act expediously when a task is better performed on-line (Ballard et al. 1997). Moreover, children must also gain the ability to switch between these modes of functioning. The lesson to be taken from the A-not-B model is that these are not wholly separate ways of functioning, but rather emerge from a single set of dynamic interactions. It is because we express perception, memory, and action in the same graded and time-based dynamic currency that we can account for the myriad contextual effects in the A-not-B task. This same currency is the foundation for the abilities of adults to so effortlessly shift between off-line and on-line. Thus, while development brings forth new forms of behavior – for example, correct responding rather than perseveration – the processes underlying these shifts are continuous. Indeed, the model illustrated how continuous parameter changes can generate nonlinear performance.

Finally, does this model have anything to say to Piaget's issue: when do infants acquire the object concept? We believe this question is ill-posed and cannot be answered because there is no such thing as an "object concept" in the sense of some causal structure that generates a thought or a behavior (Smith et al. 1999b). There is only "knowledge" of objects as embedded in the immediate circumstances and the history of perceiving and acting in similar circumstances. What motivates us to search for our missing keys is not some disembodied belief in the permanence of objects. Rather, it is a lifetime of discoveries that our keys have not vaporized after all and the recognition that the present situation is shaped like those in the past.

Indeed, the field model leads us back, in deference, to William James. There is no such "thing" as "an idea," said James (1890, p. 236), in any pure or reified form. There are only dynamics:

> For there it is obvious and palpable that our state of mind is never precisely the same. Every thought we have of a given fact, is, strictly speaking, unique, and only bears a resemblance of kind with our other thoughts of the same fact. . . . Often we are ourselves struck at the strange differences in our successive views of the same thing. . . . But what here strikes us so forcibly on the flagrant scale exists on every scale, down the imperceptible transition from one hour's outlook to that of the next. Experience is remoulding us every moment, and our mental reaction on every given thing is really a resultant of our experience of the whole world up to that date. (1890, pp. 233–34)

ACKNOWLEDGMENTS

This research was supported by NIH RO1 HD22830, Indiana University, and by an NIMH Research Scientist Award to E.T. We thank Fred Diedrich, John Spencer, Melissa Clearfield, Jana Iverson, Dan Robin, Bob Titzer, and Dewey McLin for many contributions, both theoretical and empirical, to this work. We are especially indebted to Wolfram Erlhagen for sharing unpublished theoretical work and discussion. Fred Diedrich, Olaf Sporns, Arthur Glenberg, David Robertson, Charles Twerdy, Richard Carlson, Tim van Gelder, and several anonymous reviewers made many useful comments on the manuscript. We discussed these ideas and experiments with Jay McClelland, Liz Spelke, Marc Jeannerod, Mark Latash, and Giuseppe Pelizzer at the 1995 Workshop on Object Retrieval. A more lively and challenging group of colleagues could not be found. Finally, we are grateful to Gerald Edelman, Einar Gall, and the Neurosciences Institute for supporting the 1995 Workshop and for graciously hosting E.T. as a Visiting Fellow in Fall, 1997, where a portion of this article was written.

NOTE

**1.** In informal experiments, we noticed that infants have a strong predilection to reach for whatever is closest to them. Gavin Bremner (personal communication to ET) also observed this.

# Open Peer Commentary

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## Accounting for infant perseveration beyond the manual search task

Sarah E. Berger

*Department of Psychology, New York University, New York, NY 10003.*
**sarah@psych.nyu.edu**

**Abstract:** Although the dynamic field model predicts infants' perseverative behavior in the context of the A-not-B manual search task, it does not account for infant perseveration in other contexts. An alternative cognitive capacity explanation for perseveration is more parsimonious. It accounts for the graded nature of perseverative responses and perseveration in different contexts.

Thelen, Schöner, Scheier, and Smith's dynamic field model of movement planning accurately describes and predicts infant perseveration in the highly specific context of the A-not-B manual search task. However, it cannot account for infant perseveration in other contexts, such as in non-reaching motor tasks. I present an alternative cognitive capacity theory that can describe persev-


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

eration beyond the dynamics of a manual search task and may shed light upon the underlying cause of such striking behavior regardless of its context.

A cognitive-capacity model explains why perseverative reaching goes away as infants get older, and why at any given age infants may perseverate in some contexts, hut not in others. Analogous to neuroimaging findings that performance is influenced by both memory demands and task difficulty (Barch et al. 1997; Cohen et al. 1997), the cognitive capacity theory proposes that higher-level executive functions compete with other task demands for limited cognitive and attentional resources. When a task is inherently demanding, not enough resources are available to perform more complex cognitive processes, such as inhibition. When a task is relatively easy, infants can devote their full capacity to fulfilling other demands. According to this explanation, infants perseverate in particularly demanding contexts but as they become more skilled, the task becomes less difficult and perseverative errors decrease. In the manual search task, for example, the parameters described in the dynamic model serve to increase task difficulty by experimentally increasing the salience of a stimulus to be inhibited. The cognitive capacity explanation is meant to account for perseveration in any context, and, in fact, predicts perseverative errors regardless of age so long as the context is sufficiently demanding.

By Thelen et al.'s account, infants motor memory for reaching influences subsequent reaches. Central to their account is that babies use the same postures and reach trajectories trial after trial, strengthening the memory for that action. By the cognitive capacity account, specific motoric repetition is not necessary in some contexts, and not sufficient in others, to elicit perseverative responses. Indeed, in a preliminary study of 13-month-old walking infants in a locomotor A-not-B task (Berger 2000) we elicited a high rate of perseverative errors in a high-demand condition in which infants repeatedly traveled to a goal at the same location, but the methods they used to get there varied from trial to trial. Furthermore, even when infants' movements were highly similar trial after trial, in a low-demand condition, they still did not perseverate. Suppose we interpret the meaning of the dynamic direction field $u(x)$ more abstractly than Thelen et al. have defined it. Instead of pure motor memory, let the direction field correspond to a set of possible directions for action, which then drive the body once the direction of action is chosen. This definition would not necessitate identical movements, only identical motor goals in order to account for perseverative behaviors. Using this definition, infants' perseverative behavior in the high-demand version of the locomotor A-not-B task could he explained (i.e., infants perseverate on the path to location A because they have done so repeatedly, rendering it more "attractive" than the path to location B), as well as infants' perseverative behavior in the manual search task. However, neither the definition provided in the article nor this revised definition can explain infants' behavior in the low-demand condition, where they never perseverate, in any way. The general direction of action to reach the goal at location A in the low-demand condition is the same trial after trial, just as in the high-demand condition or the manual search task. However, in the low-demand condition infants have no difficulty taking a new, direct path to B after a switch in the goal's old location from A its new location at B. The cognitive capacity theory can account for the results of Thelen et al.'s task as well as other tasks that elicit perseverative responses including cognitive tasks and motor tasks that do not involve repetition of movements.

The dynamic movement field depicts graded information about the space in which the infants are reaching. The A and B locations are portrayed as just two possible locations in the continuum of space. Activation of the dynamic field above a threshold is necessary to activate a reach to a particular target. Activation that reaches threshold denotes a reach, but the model cannot specifically characterize infants' responses that fall below threshold. The dynamic systems model accounts for a single threshold representing a reach but, in fact, there is evidence that infants display a wide range of perseverative behaviors and that perseverative responses themselves can also he graded. To portray them, the model would have to have several thresholds or specify what the sub-threshold activation really represents. In the locomotor A-not-B task, for example, infants perseverate at one extreme by taking the old, familiar path to reach the goal at its new location, making a detour rather than going directly to the new location. Infants in this study also perseverate more subtly, including starting to take the A path before turning hack and eventually taking the B path. In the manual search task, as well, there are examples of infants looking at one location while reaching to the other (Diamond 1990a) or reaching in the direction of the A location, but not actually making it all the way there (Diamond et al. 1989, cited in Diamond 1990a). Therefore, the depiction of distributed activation in the dynamic systems model should express the graded nature of the perseverative error.

## Plus maze experiments and the boundary conditions of the dynamic field model

Melissa Burns[a] and Michael Domjan[b]

[a]*Department of Psychology, Texas Christian University, Fort Worth, TX 76129;* [b]*Department of Psychology, University of Texas, Austin, TX 78712.*
**m.burns@tcu.edu** **domjan@psy.utexas.edu**
**www.psy.tcu.edu/psy/burns.html**
**www.psy.utexas.edu/psy/faculty/domjan.html**

**Abstract:** In the dynamic field model, parametric variations of the same general processes predict how infants reach for a goal. Animal learning investigators argue that locating a goal is the product of qualitatively different mechanisms (response learning and place learning) Response versus place learning experiments suggest limitations to the dynamic field model hut where those limitations begin or end is unclear.

The Thelen et al. target article describes a paradigm (the A-not-B problem) used to study cognitive development in children and offers a detailed model to characterize the results obtained with this paradigm. The model has considerable generality. However, its boundary conditions have not been clearly specified. In an effort to stimulate such a specification, we wondered whether the theory could account for the results of studies with nonhuman animals (mostly laboratory rats) that have striking similarities to the A-not-B problem examined with children.

The nonhuman animal studies employ the plus maze, which consists of four arms extending from a central platform at 90 degree intervals. The end of one arm (the south arm, for example) is designated as the start area, and the end of another arm (the west arm, for example) is designated as the goal area and has a piece of food. Training consists of placing the rat in the start area and permitting it to obtain the food by going to the center and making a left turn to reach the goal at the end of the west arm. This task is analogous to the A-not-B problem presented to children in that during training a particular side (left, for example) always has the hidden treat the children have to reach for. The question is, what do the rats (and the children) learn during the training trials that enables them to make the correct choice?

In the studies with nonhuman subjects, two prominent hypotheses have been considered. According to one view (the response learning hypothesis), the subjects learn a particular motor response (turning left) to reach the food location. According to the competing view (the place learning hypothesis), the subjects learn to identify the spatial location of the food (the end of the west arm). The response and place learning interpretations have been evaluated by conducting a test trial in which the subjects are started from the opposite side of the maze (they are started from the north arm). If the subjects learned a particular response (a left turn), they should end up in the east arm when started from the north. In contrast, if they learned the particular location of the



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

food, they should continue to return to that location (the west arm) even if they are started from the north side of the maze.

The A-not-B problem is similar to the plus maze problem in that principle training with the object hidden in cup A (on the left, for example) can lead to response learning (reaching left) or place learning (reach towards the location where the child saw the object being hidden). In some studies (e.g., Bremner & Bryant 1977), the testing procedure with children is also similar to what is done with the nonhuman animal subjects, in that the child is moved 180 degrees to face the two cups from the opposite side. Bowever, in most cases, testing is done by moving the hidden object to cup B (on the right) rather than by moving the child. In either case, response learning is expected to produce the same reaching response (reaching left), and place learning is expected to produce reaching toward the location where the object was hidden last. Thus, when the object moved to side B (on the right), response learning (reaching left) will cause an error if the child is kept in the same start position but will cause a correct response if the child is moved to face the two cups from the opposite side (Bremner & Bryant 1977). Place learning will lead to a correct response when the object is shifted to side B regardless of the child's vantage point (because place learning involves reaching toward the place where the object was hidden on that trial).

Much like infants, rats show place learning under some circumstances and response learning in others. In general, when there are distinctive visual cues, place learning predominates (Blodgett & McCutchan 1947; Blodgett et al. 1949). In contrast, in visually homogenous environments, response learning is dominant (Blodgett & McCutchan 1948; Hill & Thune 1952).

The plus maze experiments did not provide support for the place learning or response learning hypotheses exclusively. Rather, the experiments encouraged the view that there are two strategies for learning to find food in a particular location. The two-system hypothesis has been further supported by physiological evidence that suggests that the two learning systems are mediated by different neural mechanisms. Inactivation of the hippocampus via lesions (Kesner et al. 1993; McDonald & White 1993; Packard & McGaugh 1992) or drugs (Packard & White 1999; Packard & McGaugh 1992) disrupts place learning but not response learning in rats. In contrast, inactivation of the caudate nucleus inhibits response learning but not place learning (Kesner et al. 1993; McDonald & White 1993; Packard & McGaugh 1992; 1996; Packard & White 1991). Thus, the neural mechanisms of response and place learning are dissociated in rats. Evidence for the two system hypothesis can also been found with infants. The emergence of place learning in infants (DeLoache & Brown 1983; Newcombe et al. 1998) is consistent with what is known about neurological development. Human hippocampal maturation continues until about 21–22 months-of-age (Kretschmann et al. 1986). Response learning is present much earlier (Bremner & Bryant 1977; Newcombe et al. 1998).

The dynamic field model effectively predicts when an infant will correctly locate a goal object in its environment. According to the model, whether a correct or an incorrect response occurs in the A-not-B problem depends on parametric variations of the same general processes. Thus, correct versus incorrect responses are not viewed as the results of qualitatively different underlying mechanisms. Investigators using the plus maze (with some human but mostly nonhuman subjects) have been concerned with similar issues. However, they have couched those issues in terms of the distinction between response and place learning. These two types of learning represent qualitatively different mechanisms, rather than parametric variations of the same underlying processes. Furthermore, these qualitative distinctions are supported by differences in neural mechanism. The literature on response versus place learning suggests limitations to the dynamic field model. However, it is not clear where those limitations begin or end.

## Embodiment is all in the head

Paul Cisek

*Département de physiologie, Université de Montréal, Montréal, Québec H3C 3J7 Canada.* **cisekp@magellan.umontreal.ca** **pavel@cisek.org**
**www.cisek.org/pavel**

**Abstract:** I suggest that neurophysiological data, usually interpreted in cognitivist terms, is actually more supportive of dynamic frameworks such as that of Thelen et al. I outline a model of embodied action used to interpret neural data from frontal and parietal cortex, and suggest that it bears strong similarity to the framework described in the target article.

By phrasing a classic cognitive phenomenon, the A-not-B error, in terms of the dynamics of action planning, Thelen et al. question one of the most fundamental assumptions of cognitive psychology: the distinction between disembodied computational mechanisms of pure cognition and the physical implementation of behavior by the brain. This distinction is very dear to modern psychology because it was instrumental in making a study of human thought and the "cognitive revolution" justifiable at a time when not much was known about brain mechanisms.

Much has changed in recent decades. Owing to the accelerating growth of neuroscience, there is now a movement to bring psychology and biology back together, a movement often called "cognitive neuroscience" (Gazzaniga 2000). But cognitive neuroscience is not merely a broad endeavor to understand the functional architecture of the brain. In practice, it is an attempt to map a very particular conceptual toolbox, that of cognitivism, onto brain data. Alternative viewpoints, such as "embodied cognition" (Núñez & Freeman 2000) or the "dynamical approach" (Beer 2000) do not penetrate the brain sciences enough to influence the interpretation of data, simply because most neurophysiologists have not heard much about them.

This seems ironic if one believes, as I do, that the neurophysiological data is actually much more compatible with these alternative viewpoints. The framework outlined by Thelen et al. is an excellent example, but they too prefer to hold back on explicitly mapping their model onto the brain. Below, I am not so cautious, and propose a way in which diverse neurophysiological data can be interpreted in terms of a functional architecture for embodied action.

Below, I briefly sketch out my own pet framework (Fig. 1) which shares a great deal with Thelen et al.'s (This is shameless self-promotion, I know, but I believe the comparison can provide a simple bridge between neural data and dynamical viewpoints such as that of Thelen et al.). This framework is based upon a distinction between two kinds of pragmatic concerns that animals face while actively interacting with a physical world: *action specification and action selection.*

The natural world continuously presents us with many opportunities for action, and sensory information arriving from that world significantly constrains the parameters of these potential actions (Gibson 1979) The arrangement of surfaces and objects around one's body constrains the possible directions of locomotion. The egocentric location, orientation, and size of a graspable object constrain the possible limb configurations, hand orientations, and finger apertures required to grasp it. In the framework of Figure 1, the brain automatically uses spatial information arriving from the world to begin to specify the parameters of currently available potential actions. Of course, one cannot perform all possible actions at the same time. Neither is it likely that the brain attempts to begin planning all interactions possible with the environment at any given moment. Thus, there must exist mechanisms to reduce the number of potential actions and to ultimately select one for overt execution. These decisions can also be based upon sensory information, such as the identity of objects in the world.

In this framework, behavior is seen as a constant battle between currently available opportunities for action. In other words, the


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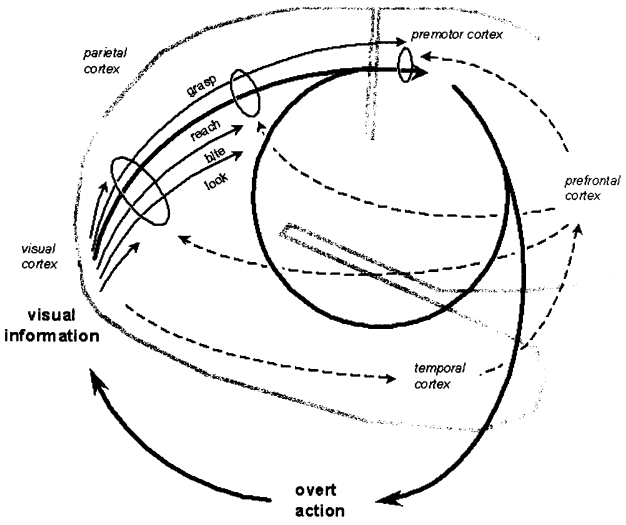



Figure 1 (Cisek). The specification-selection model. Solid lines indicate pathways for specifying potential actions. Dashed lines indicate pathways carrying information used for selection, particularly emphasizing the role of the prefrontal cortex. Ovals schematize various selection mechanisms which eliminate potential actions from further sensorimotor processing. The selected action (thick solid line) is released into overt execution and develops through both externa1 and internal feedback.

brain continuously transforms sensory information into the parameters of potential actions, while accumulating information useful for selecting one of these into overt execution. There is no border between planning and execution systems, and even the final selected action is not completely specified as a "desired trajectory" typical of many robotics type controllers. Instead, as suggested by both neurophysiological and psychophysical data (reviewed in Kalaska et al. 1998), only the initial part of a movement is prepared and the movement then develops on-line via both external feedback through the environment and internal feedback through "forward models" (Miall & Wolpert 1996). Below, I briefly speculate on how this framework can be used to interpret neural data from parietal and frontal cortex.

In the specification-selection model, spatial information is gradually transformed from a sensory format describing the world (e.g., object position) into a motor format specifying potential actions (e.g., direction from hand to object) along the posterior parietal cortex (Kalaska et al. 1997). This is consistent with the characterization of the parietal cortex as part of a dorsal visual system involved in visually-guided movement (Milner & Goodale 1995). As the sensorimotor transformation occurs, information for many potential actions is progressively eliminated from further processing through a series of selection mechanisms. When the information is still in a sensory format, selection occurs based on sensory features such as salience or spatial location – we may call such selection a mechanism of "attention" (Castiello 1999; Neumann 1990; Tipper et al. 1998). In the early part of the visual system, attentional influences enhance information from particular regions of space while other regions are suppressed (Reynolds et al. 1999). Surviving information is transformed further, and the dorsal stream diverges into separate systems concerned with different classes of actions, such as reaching in MIP (medial intraparital area) (Kalaska 1996; Snyder et al. 2000), grasping in AIP (anterior intraparietal area) (Jeannerod et al. 1995), biting in VIP (ventral intraparieted area) (Duhamel et al. 1998), and looking in LIP (lateral intraparietal area) (Andersen 1995; Snyder et al. 2000), each with its own idiosyncratic representation of space (Colby & Goldberg 1999). These representations are quite impoverished, however, with only the most salient features of the environment being represented (Kusunoki et al. 2000). Because the same regions are clearly implicated in early movement planning (Andersen 1995; Snyder et al. 1997; 2000), we can say that only the most promising potential actions make it this far along the dorsal stream. At this point, the expected consequences of potential actions can be used to influence further selection – we can call such selection a mechanism of "decision-making." Indeed, decision variables such as movement probability and expected payoff influence activity of parietal cells (Platt & Glimcher 1999), as does behavioral context (Kalaska 1996). Surviving potential actions are carried to frontal regions such as the dorsal premotor cortex, where final action decisions are reflected (Kalaska & Crammond 1995) before overt movements are generated.

A number of brain regions may provide the influences needed for the selection mechanisms described above. visually-guided action selection may utilize information from the ventral stream, where cells are sensitive to stimulus features (Milner & Goodale 1995; Tanaka 1993). Such features could he used to bias selection along different areas of the parietal action specification system by enhancing promising potential actions while suppressing others, or simply by influencing the dorsal stream to fixate on behaviorally-relevant information (Cisek & Turgeon 1999). Because action selection is a fundamental concern faced even by our distant ancestors, we should expect that it involves phylogenetically old structures such as the basal ganglia. A behavioral competition in the basal ganglia (Mink 1996) may bias selection by influencing specific cortico-basal-thalamo-cortical loops (Middleton & Strick 2000). At the same time, because action selection is also likely to have become significantly more sophisticated in the recent evolutionary history of primates, it probably also involves neural structures that are particularly developed in the great apes, such as the frontal lobes of the cerebral cortex. Complex criteria for sophisticated selection may be processed in the prefrontal cortex, where cells are sensitive to those stimulus features relevant to response selection (Rainer et al. 1998) and where information for making decisions is accumulated (Kim & Shadlen 1999). Our exceptionally large frontal lobes may have enabled the human ability to select actions based upon increasingly complex criteria, and classical frontal syndromes, which affect a patient's ability to select actions appropriately, illustrate what can go wrong when that ability is lost.

The theory of Thelen et al. is quite compatible with such a "motor chauvinistic" perspective on neurophysiological data. The locus of activity in their movement planning field specifies potential actions in a space of action parameters (in their case, a space of potential directions), and the amplitude at each locus reflects the influences of selection. Thus, competing potential actions can coexist as distinct hills in the landscape of cellular activity. A highly desirable action which demands great precision may be coded as a tall narrow hill, while a less desirable and more unconstrained action may be a low and wide plateau. All kinds of selection influences, from "attention" to "decision variables," may be combined together to bias the competition between hills of activity which correspond to different potential actions. Such a planning field model can be used to simulate the results of many of the studies I mentioned above.

John Kalaska and I are currently performing an experiment directly designed to explore the neural substrates of the theoretical framework shown in Figure 1 (Cisek & Kalaska 1999). In the experiment, when a monkey was faced with two potential reaching actions, one of which would eventually be performed after a delay of several seconds, neural activity from the dorsal premotor cortex (an area implicated in preparation for movement; Boussaoud & Wise 1993; di Pellegrino & Wise 1993; Wise et al. 1992) indicated that both movements were prepared siinuitaneousiy before one would be selected for overt execution. That is, instead of making a cognitive decision first and then preparing action (as would be predicted by the "sense-think-act" architecture of traditional cognitivism), the monkey first specified multiple potential actions and then selected among them (as predicted by the



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

"specification-selection" architecture). These results are reminiscent of a number of recent experiments involving ambiguous targets (Basso & Wurtz 1998; Bastian et al. 1998; Munoz & Wurtz 1995; Platt & Glimcher 1997), and can be easily simulated with a movement planning field model like that of Thelen et al., as was explicitly done for the experiment of Bastian et al. (1998).

One can predict that competition between actions in a movement planning field will be evident even in the final movement trajectory, with subtle deviations occurring when the activity bill of an unselected potential movement slightly overlaps that of the selected movement. Indeed, such deviations have been shown for reaching movements in the presence of distractors (Tipper et al. 1998), and simulated with a model (Tipper et al. 2000) which is conceptually very similar to that of Thelen et al.

The authors are of course well aware of the support that such models can gain from neurophysiological evidence. Their discussions in sections 4.1.1 and 7.1.3 make this clear. However, I think that one can go far beyond these preliminary comparisons. In fact, one can go so far as to suggest that most neural activity is not so much concerned with representing the world as with "mediating interactions with the world," through specifying potential actions and selecting among them. One can use neurophysiological data, traditionally interpreted from the perspective of cognitivism, to support theoretical frameworks such as that of Thelen et al. or that of Figure 1, which stand in opposition to many of the assumptions of cognitivism. In fact, if we are indeed poised to witness a shift away from the disembodied computational assumptions of traditional cognitive psychology to a more embodied science of behavior, such a shift may be primarily driven by the growing literature of neurophysiological data.

ACKNOWLEDGMENTS

I thank John Kalaska for helpful feedback on this commentary. The work was supported by NIH.

## Looking closely at infants' performance and experimental procedures in the A-not-B task

Adele Diamond

*Center for Developmental Cognitive Neuroscience, Eunice Kennedy Shriver Center at University of Massachusetts Medical School, Waltham, MA 02452.*
**adiamond@shriver.org**

**www.shriver.org/research/irc/cdcn/staff/diamond.htm**

**Abstract:** Thelen et al.'s model of A-not-B performance is based on behavioral observations obtained with a paradigm markedly different from A-not-B. Central components of the model are *not* central to A-not-B performance. All data presented fit a simpler model, which specifies that the key abilities for success on A-not-B are working memory and inhibition. Intention and action can be dissociated in infants and adults.

The target article by Thelen et al. is ambitious, but ultimately disappointing. (1) Central to their model of A-not-B is an attempt to account for findings obtained using a paradigm fundamentally different from A-not-B. By arguing that their procedures closely approximate those used in A-not-B studies, they misrepresent what was done in A-not-B studies. (2) Thelen et al.'s model has fatal flaws. (3) The premise of the target article is to solve a puzzle that is not a puzzle. (4) The role of prefrontal cortex was too easily dismissed. (5) Dissociations between "knowing" and "doing" were caricatured.

1. The behavioral paradigm used by Thelen and colleagues (e.g., Smith et al. 1999b) to study A-not-B performance differs from the A-not-B task in several key respects (see Table 1). For example, whereas in the A-not-B task, a trial at B is only administered after an infant has reached correctly; Thelen et al. administered the B trial after a set number of trials at A, even if the infant had reached incorrectly on the last trial of that set. There are obvious problems in measuring "perseveration" in a participant who has not shown a consistent response that might then be perseverated.

2. Thelen et al.'s model faces a number of major problems: (a) *"The relative ambiguity of the task input is a critical parameter in the model"* (sect. 2.2.1, emphases in original). However, this is not critical to the A-not-B error; indeed the kind of ambiguity Thelen et al. produced by using lids and background of the same color and placing the lids close together is not present in most A-not-B studies (see Table 1). That such ambiguity is not central to the A-not-B error, but is central to Thelen et al.'s model, is a huge problem for their model. (b) They claim that a critical part of why infants err on the A-not-B task is because it is entirely novel. However, that cannot be critical because when I tested infants on the task every 2 weeks for 6 months I still saw the A-not-B error consistently at all ages (Diamond 1985). All that practice and repeated exposure had only a modest effect on performance. Older, practiced infants made the A-not-B error as robustly (albeit at a longer delay) as younger, novice infants and as robustly as older, novice infants. (c) They claim that *the act of reaching to A* is critical to why infants err and the probability of making the A-not-B error is a function of the number of previous reaches to A. However, while the number of reaches to A matters when the number of A trials is as large as 8–15, there is no effect whatsoever on the number of repeated reaches to A within the range of 1–3 or even 2–5 (Butterworth 1977; Diamond 1983; Evans 1973). Thelen et al. assert that repetition is essential; but it is not needed at all: The A-not-B error is as robust after one reach to A as it is after three repetitions to A. An even bigger problem for the authors is that infants do not need to reach to A at all to make the A-not-B error. Both Evans (1973) and I (1983) found, with no pretraining trials to A, as robust an A-not-B error from just observing the experimenter retrieve the toy at A as from the infant reaching and retrieving the toy at A. (d) They attribute the A-not-B error in part to the poor reaching skills of infants between 7–12 months. However, infants of 10–12 months are quite skilled reachers, and they show the A-not-B error as robustly as younger infants. (e) They predict that with multiple reversals infants should reach randomly. That prediction has not been confirmed. In Diamond (1985), Diamond and Doar (1989), and Diamond et al. (1994), we administered multiple reversals and found that (i) infants did not reach randomly, but showed a predictable pattern to their reaches, and (ii) on later trials infants performed no worse and were no more likely to reach randomly than on earlier trials.

3. The puzzle that Thelen et al. set out to solve is: "While the A-not-B error is entirely robust in the canonical form we described above, even seemingly small alterations in the task conditions can disrupt it" (sect. 1, para. 3). (a) However, many alterations in the task do not affect performance. For example, the A-not-B error is found whether the hiding places differ in left-right or up-down location (Butterworth 1976), and is found whether the toy is hidden under cups (e.g., Neilson 1982), in containers (e.g., Butterworth 1975), behind screens, curtains, or doors (e.g., Harris 1973), on the tabletop under a cloth (e.g., Fox et al. 1979) or inside wells (e.g., Diamond 1985). (b) The A-not-B error is so robust that, despite marked variability in task administration, virtually every lab finds this behavior. Many alterations that make the task easier do not disrupt the A-not-B error altogether, but simply affect the delay at which it occurs and/or how often it is repeated. (c) Alterations in the task that affect how easy it is to remember where the toy was hidden or how strong the pull is to repeat the previously rewarded action (e.g., varying the discriminability of the hiding places, salience of what is hidden, delay between hiding and retrieval, number of reaches before the reversal, or visibility of the "hidden" toy) *should* affect the likelihood of finding the A-not-B error if my theoretical position (that the key abilities required for the A-not-B task are working memory and inhibitory control) is correct, and they do. I see no puzzle here.

There are no data that Thelen et al. present which cannot easily be accounted for by the theory I presented in the early 1980s.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

Table 1 (Diamond). *Differences between the procedures used by Thelen, Smith, and their colleagues and those used in studies of A-not-B*

| Procedural Element | Procedures used by Thelen et al. | Procedures used by A-not-B Researchers |
|---|---|---|
| (a) Discriminability of the covers from the background surface. | Lids designed to blend into the background: Brown lids on a background of the same brown color<br>Thelen et al.: "The notable characteristic of [our] task input was its lack of visual specificity. The two lids . . . blended into the background of the box." | Covers designed to stand out from the background. For example:<br>White covers on black tabletop (Bremner 1978)<br>White covers on green background (Butterworth & Jarrett 1982)<br>Blue or red covers on a brown background (Sophian & Yengo 1985)<br>Light blue covers on dark brown background (Diamond 1985)<br>Most A-not-B researchers go out of their way to make the covers discriminable from the background surface. |
| (b) Distance between the covers. | Lids placed quite close together (12.5 cm apart, center to center); this is less than half that in most A-not-B studies. | Covers usually placed considerably further apart:<br>30 cm apart center to center: Acredolo et al. 1985; Appel & Gratch 1984; Benson & Uzgiris 1985; Diamond & Doar 1989; Evans & Gratch 1972<br>28 cm apart center to center: Bremner 1978; Diamond 1985 |
| (c) Illumination of the room. | Low illumination; dimly lit room in one study. (That would make it harder for infants to see the covers, tell them apart, and distinguish them from the background.) | A-not-B testing is conducted in a brightly lit room.<br>In no study of A-not-B have the lights been dimmed. |
| (d) Presence of distraction during the delay. | No distraction. Infant permitted to continue to look at, or turn or reach toward, the cued location throughout the delay. No attempt to break infant's fixation on the correct well or to prevent position cueing. | Some studies provided no distraction, but others have:<br>Visual fixation of the correct well prevented (e.g., Diamond 1985; Diamond & Doar 1989; Diamond et al. 1994; Fox et al. 1979; Freeman et al. 1980).<br>Bodily cueing toward the correct well prevented (e.g, Diamond 1985; Diamond & Doar 1989; Diamond et al. 1994; Fox et al. 1979; Harris 1973) |
| (e) Amount of initial reaching experience at A: | | |
| (e.1.) Number of training trials at the "A" location. | Four training trials administered at A. | No training trials at A (training trials administered at a central location): e.g., Benson & Uzgiris 1985; Bower & Patterson 1972; Diamond 1985; Evans 1973: Fox et al. 1979; Horobin & Acredolo 1986; Schuberth et al. 1978; Sophian & Yengo 1985; Willatts 1979). Where training trials at A have been administered, only 1 or 2 are given, not 4. |
| (e.2.) Number of initial trials at the "A" location. | Six trials administered at A.<br>This is 50%–500% more initial trials at A than in A-not-B studies. | In *all* A-not-B studies: Typically only 1 or 2 trials administered at A, and not more than 4, except in studies specifically designed to look at the effect of variation in the number of initial trials at A. |
| (f) Rule for determining when to switch to the "B" location. | B trial administered after a set number of trials at A, regardless of infant's performance on the A trials. B trial administered even if the infant had reached incorrectly on the preceding A trial. | B trial administered after infant has reached correctly 1 or more times at A, regardless of whether that takes 1, 2, 3, or 4 trials at A. B trial administered *only* following a correct reach at A. |

(*continued*)



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Table 1 (Diamond). (*Continued*)

| Procedural Element | Procedures used by Thelen et al. | Procedures used by A-not-B Researchers |
|---|---|---|
| (g) Criterion for determining whether a reach is correct or not. | Painstaking frame-by-frame analyses of the videotape to see if a slightly faster contact to one lid can be detected. If so, it is scored as a reach to that lid, not both. | Scoring is never done by frame-by-frame analysis. An infant who touches both covers at almost the same instant is not scored as having intended to reach the cover that was contacted a millisecond earlier, but as having reached to both covers. |
| (h) Infants' rationale for reaching, the reward for reaching correctly, and the penalty for reaching incorrectly. Why reach to one location rather than another? | Reaches were usually for a visible lid, exactly like the visible lid at the other location. No toy was usually hidden and no reward usually provided for a correct reach. When a toy was hidden, infants were allowed to have it on each trial, whether or not they had reached correctly. Thus, there was no difference in outcome of a correct or incorrect reach. | The two covers are identical, but reach is for a hidden toy, located under only one of the covers. The reward for a correct reach is getting to play with the toy. Some studies penalize an incorrect reach by not letting the infant have access to the toy on that trial (e.g., Diamond 1985; Diamond & Doar 1989; Diamond et al. 1994; Horobin & Acredolo 1986). |

Thelen et al. suggest that "it is not clear in Diamond's account, why . . . the number of A reaches or the distinctiveness of the targets should matter so profoundly" (sect. 6.1.1). Yet, it follows straightforwardly that anything that increases the strength of the prepotent tendency that must be inhibited (as would increasing the number of reaches to A) or that makes less distinct the information that must be held in mind (e.g., reducing the distinctiveness of the targets) should make errors more likely. Indeed, the parameters of Thelen et al.'s model – "the target position must be remembered during the reach" (working memory, sect. 3.1.4) and "the motor memory of the just-completed movement is also retained and integrated into the next plan" (thus requiring inhibitory control) – map directly onto the parameters in my account. The onset of locomotion might be related to improved A-not-B performance because locomotion onset provides an index of maturational level or because infants attend more closely to the kind of information they must hold in mind in the A-not-B situation once they are locomoting; these findings pose no problem for my theoretical perspective. Finally, Thelen et al. assert that my theory "cannot account for the looking-reaching decalage." (sect. 1.1, para. 4) It can and it has (Diamond 1998).

For the record, Thelen et al. make some assertions about experimental design and procedures used in my work that are incorrect (a) "The delay and the number of repetitions are not independently controlled. So it is impossible to tell whether failure is due to delay or to repetition" (sect. 3.2.1). Not true. Diamond (1985) reported the effect of systematically increasing or decreasing the delay within a session. Diamond (1983) reported the effect of systematically varying the number of repetitions at A. (b) "The actual number of reaches to A before the switch is not reported and is unknown." It is known and reported (Diamond 1983). As reported, (a) in >90% of testing sessions, infants were correct on both initial trials at A and so received only 2 A trials, and (b) infants never made more than one error on the initial A trials and so never received more than 4 A trials. Diamond (1983) and others have shown that varying the number of A trials within that small range has no effect whatsoever on the A-not-B error. (c) "These procedures commonly make 12 or 15 target switches (sect. 6.2.1, last para.)." Not so. These procedures never make more than 5 switches at the delay used for testing and never more than 2 switches at other delays prior to that – hence no more than 7 switches (Diamond 1983; 1985; Diamond et al. 1994; 1997).

4. Thelen et al. question whether A-not-B performance can be assumed to be a marker of dorsolateral prefrontal cortex maturation. The following addresses their reservations in turn:

(a) The evidence for prefrontal involvement comes from work with rhesus monkeys where only a similar, but not identical, task was used. Not so. The task used with monkeys (Diamond & Goldman-Rakic 1989; Diamond et al. 1989) was as identical as the A-not-B task used in one infancy lab is to that used in another infancy lab. I have argued that results on a similar task (delayed response) are also relevant because A-not-B and delayed response are essentially the same task and the developmental progressions on both tasks are identical in human infants and infant rhesus monkeys (Diamond 1991a; Diamond & Doar 1989). Most compelling, the results with human infants and with monkeys with lesions of dorsolateral prefrontal cortex closely parallel one another. They fail the task in the same ways and under the same conditions; parametric variations in the task have the same effects on the performance of both groups. See Table 2.

(b) "Second, the problem of circularity. The evidence that prefrontal cortex 'matures' between 8 and 12 months is performance on A-not-B type tasks" (sect. 6.2.1). Not so. In human infants, studies of surface electrical activity indicate maturational changes in prefrontal cortex during the period that infants are improving on A-not-B and such changes are correlated with A-not-B performance (e.g., Bell & Fox 1992; 1997; Fox & Bell 1990). There is also considerable independent evidence of prefrontal cortex maturation during the period (1–4 months of age) that infant rhesus monkeys are improving on the A-not-B task. Take one aspect of prefrontal maturation (maturational changes in the prefrontal dopamine system): During the period of 1–4 months, the density of prefrontal dopamine receptors is increasing (Lidow & Rakic 1992) and the distribution within prefrontal cortex of axons containing tyrosine hydroxylase (essential for the production of dopamine) changes markedly (Lewis & Harris 1991; Rosenberg & Lewis 1995).

(c) "The progression of prefrontal cortex towards adult-like states is very gradual, extending into adolescence (sect. 6.2.1). That is correct; it extends even into adulthood (Huttenlocher & Dabholcar 1997 and Sowell et al. 1999). It is hardly contradictory for prefrontal cortex to undergo critical maturational changes between 8–12 months of age but not to be fully mature until many years later (see, e.g., Diamond 1996). I have never said, however, that "prefrontal maturity *alone* is the critical element" as Thelen et al. erroneously attribute to me. I have always maintained that prefrontal maturation plays a critical role, but not the only role.

5. Thelen et al. take strong exception to assertions of a division between what children "know" and what they can demonstrate they know. Their characterization that "one foundational assumption behind these dual-process (knowing vs. acting) accounts is that there lives, in the baby's head, a creature that is smarter than


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

Table 2 (Diamond). *Close parallels between the performance of prefrontal monkeys and human infants on the A-not-B and delayed response tasks*

| Experimental finding | Rhesus macaques with lesions of dorsolateral prefrontal cortex | $7\frac{1}{2}$–9 month old human infants |
|---|---|---|
| Succeed when there is no delay. | Harlow et al. 1952; Bättig et al. 1960; Goldman & Rostvold 1970 | Harris 1973; Gratch et al. 1974 |
| Succeed when allowed to continue to orient toward the correct well. | Bättig et al. 1960; Miles & Blomquist 1960; Pinsker & French 1967 | Cornell 1979; Fox et al. 1979 |
| If their attention is directed back to the A well after the hiding at B, they perform worse. Conversely, if B is covered after A, they perform better on the B trials. | Bartus & Levere 1977 | Diamond et al. 1994; Harris 1973 |
| Succeed if a landmark reliably indicates the reward's location. | Pohl 1973 | Butterworth et al. 1982 |
| Fail on reversal trials and on the trials immediately following reversals at delays of 2–5 sec. | Harlow et al. 1952; Bättig et al. 1960; Goldman & Rostvold 1970; Fuster & Alexander 1971; Diamond & Goldman-Rakic 1989 | Evans 1973; Gratch et al. 1974; Diamond 1985 |
| Succeed on the initial trials at A. | Diamond & Goldman-Rakic 1989 | Diamond 1985 |
| Show "deteriorated" performance at delays of 10 sec. | Diamond & Goldman-Rakic 1989 | Diamond 1985 |
| Try to self-correct after making an error. | Diamond & Goldman-Rakic 1989 | Diamond 1985 |
| Show the A-not-B error if the hiding locations differ in left-right location. | Harlow et al. 1952; Goldman & Rostvold 1970; Diamond & Goldman-Rakic 1989 | Gratch & Landers 1971; Diamond 1985 |
| Show the A-not-B error if the hiding places differ in up-down location. | Fuster 1980 | Butterworth 1976 |

the body it inhabits unfortunate caricature. Sometimes infants know the right answer, and are attempting to demonstrate it, but the experimenter has set up a situation where the infant's immature motor abilities get in the way of the infant being able to complete the intended action. I have shown that although infants of 5–7 months understand the concept of contiguity, psychologists have mistakenly assumed they did not, because psychologists "asked" infants to demonstrate this knowledge in situations where infants' inability to precisely aim their reaches and their inability to inhibit the grasp reflex got in the way of demonstrating their cognitive competence (Diamond & Gilbert 1989; Diamond & Lee, in press). All of us have been in situations where we knew more than we could show at the moment. For example, suppose a person you have called very often changes her phone number; suppose even that the first few digits remain the same. You will often dial the old number, or at least begin dialing it. Sometimes that will be because you forgot that the number had changed (i.e., at that moment you didn't "know" the correct number). Sometimes, if you are like me, that will be because even though you go to the phone reminding yourself of the new number, you dial the old number anyway (i.e., your behavior was "captured" by a prepotent action tendency and did not accurately reflect what you "knew"). The more your working memory is taxed (by holding other things in mind or distractions), and/or the harder the prepotent tendency is to inhibit (the more often you called the old number recently, the more similar the beginnings of the two numbers), the more likely you are to make this error. When normal adults are distracted, stressed, tired, or not paying close attention they often make errors characteristic of frontally-damaged adults or frontally-immature children.

In quoting me concerning this ("Infants really know where the [object] is even when they reach back to where they last found it"). Thelen et al. omitted the critical modifier ("sometimes"). We have *long* known that caching and looking are coupled in infants; I documented that (Diamond 1983; 1988; 1991b), as have others (e.g., Bruner 1973). I have said and written repeatedly that it is rare to see a stark dissociation between where an infant is looking and reaching, just as it is rare to see a clear, full-blown surprise reaction in an infant to finding A empty when the toy was hidden at B. However, both of these behaviors, when they occur, provide a glimpse into the two abilities required by the A-not-B task – the ability to inhibit the prepotent tendency to reach back to A and the ability to hold in mind where the reward was last hidden.

## Movement planning and movement execution: What is in between?

N. Dounskaia and G. E. Stelmach

*Motor Control Laboratory, Arizona State University, Tempe, AZ, 85287.*
**natalia.dounskaia; stelmach@asu.edu**
**www.asu.edu/clas/espe/mclab/motorcontrolwebpage.html**

**Abstract:** Although the model proposed by Thelen and co-authors provides a detailed explanation for the processes underlying reaching, many aspects of it are highly speculative. One of the reasons for this is our lack of knowledge about transformation of a hand movement plan into joint movements. The leading joint hypothesis (LJH) allows us to partially fill in this gap. The LJH offers a possible explanation for the formation of movement and how it may be represented in memory. Our explanation converges with the dynamic model described in the target article.

Thelen et al.'s model provides a logical scheme of the complicated processes involved in reaching in general, and in particular, in infants. However, many aspects of this model are hypothetical because many mechanisms underlying reaching are still largely unknown. In this commentary, we focus on the gap between what is



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

known about the central processes that precede a movement and knowledge about how movements are executed. Thelen et al. points out that, on the one hand, the cognitive events, such as attention, vision, and planning of movement in extrapersonal space have been studied predominantly from the psychological perspective. On the other hand, control and execution of motor actions have been studied through the analysis of their kinematic, biomechanical, and neuromuscular aspects. However, the processes intermediate to these two stages of movement performance are still unclear. It is still unclear how a movement preplanned in extrapersonal space is decomposed into joint movements, how activity levels at each muscle are determined, which control characteristics are memorized for future movement reproductions, and how the memory of movement emerges.

The lack of knowledge about the processes underlying reaching formation makes the dynamic model developed by Thelen and coauthors a conjecture. The authors provide only vague support for the dynamic interactions among the task, specific, and memory inputs described by equation (9). However, some evidence for such interactions, particularly at the age when the "A-not-B error" is observed (7–12 months), can be inferred from the literature, when findings of the following two groups of studies are compared.

First, recent developmental research provides some insights into the process of acquisition of reach in infants. Thelen et al. (1993) suggested that each infant "discovers" reaching through active exploration of the match between mechanical features of the infant's body and the task. A possible interpretation of these observations is that acquisition of a motor skill consists of learning how to operate the mechanical structures of the limbs. Another group of studies focused on multijoint control in adults offers a solution of exactly how the mechanical constraints of the human limbs are utilized and how the learning process is possibly organized. Multiple experiments have pointed out a difference in the type of control applied to certain joints during multijoint movements (Almeida et al. 1995; Bastian et al. 1996; Dounskaia et al. 1998; Dounskaia et al. 2000; Hoy & Zernicke 1986; Koshland & Galloway 1998; Latash et al. 1995). In our previous work (Dounskaia et al. 1998; 2000), we put forward a hypothesis that the differences observed in joint control reflect a control *strategy* employed by the central nervous system (CNS) to utilize interactive torques arising among the joints of a moving limb. Considering two-joint movements, we suggested that one joint ("leading" joint) serves as an initiator for movement of the whole limb, putting the other joint ("subordinate" joint) in motion owing to mechanical interactions. During unconstrained single-joint movements when the subordinate joint needs to be stabilized, its musculature copes with the influence of the leading joint motion (Almeida et al. 1995; Latash et al. 1995). When included in movement, the subordinate joint appears to serve as a fine tuner for the whole movement, its musculature being responsible for adjusting movement arising from the leading joint motion according to task requirements.

This interpretation of two-joint movement organization is in accordance with the features of control that have been observed at the two joints. The characteristics of the leading joint movement have much in common with the characteristics usually observed during single-joint movement: Alternating bursts of agonist and antagonist muscle activity are directly responsible for acceleration and deceleration at the joint. Accordingly, the kinetic analysis usually demonstrates that the torque produced by the muscles spanning the leading joint is the main source of movement at this joint. As a rule, movement characteristics at the subordinate joint are different. In contrast to the leading joint, muscle activity at the subordinate joint often opposes acceleration and deceleration at this joint. As the kinetic analysis demonstrates, the reason for that is the subordinate joint is predominantly driven by the interactive torque arising from the leading joint motion and not by the muscle torque. Muscle activity at the subordinate joint *seems* to play a complementary role to the interactive torque, correcting the passive movement of the subordinate joint in accordance with the task.

The described organization of movement is based on the supposition that the CNS exploits the biomechanical structure of the limb to make control economical and efficient. Therefore, learning is viewed as searching for an optimal way to operate the limb mechanical structure. Developmental research supports this supposition, suggesting that the skill of reaching emerges through establishing a match between the task and biomechanics of the limbs (Thelen et al. 1993) According to the LJH, this process can be decomposed into (1) finding which joint is to serve as leading and (2) modulating the subordinate joint movement when it is influenced by the leading joint motion (Dounskaia et al. 2000). Presumably, these two constituents of learning result in the emergence of a simple reach structure that can easily be stored in memory and used as a feedforward component of control in future movement reproductions. Obviously, no stable structure of reach yet exists in the long-term memory at the age of 7–12 months when the "A-not-B error" is observed.

Indeed, the shoulder functions as the leading joint during the majority of reaching movements, except for a very limited range of movements that do not involve much of shoulder motion (Bastian et al. 1996; Koshland 1998). Konczak et al. (1995) report that the shoulder movement in the infant's reach becomes similar to that observed in adults only toward the end of the second year, and the elbow movement is still not established even at this age. This suggests that at the age of 7–12 months considered in the target article, no stable match between the target and joint movement yet exists in long-term memory. However, a repetition of a movement to the same target might create an image in short-term memory. This image might be influential during a short period of time and might cause errors when another target is presented, as observed during the A-not-B task. The reason for that is that no stable solution for the movement to the other target exists, neither in short-term nor in long-term memory. In this situation, the infant faces a dilemma of putting effort into working out a new plan or simply using the available plan even though it might be erroneous. The advantage of the model presented by Thelen and coauthors is that it offers a detailed description for the complicated interactions among multiple dynamic processes that finally result in the reaching motor act.

## The behavior-cognition link is well done; the cognition-brain link needs more work

Walter J. Freeman

*Department of Molecular and Cell Biology, University of California, Berkeley, CA 94720–3200.* **wfreeman@socrates.berkeley.edu**

**Abstract:** Thelen et al. have a strong case for linking behavior with mind through nonrepresentational dynamics. Their case linking mind with brain is less compelling. Modified avenues are proposed for further exploration: greater emphasis on the dynamics of perception; use of chaotic instead of deterministic dynamics with noise; and use of intentionality instead of motivation, taking advantage of its creative dynamics to model genesis of goal-directed behaviors.

1. Thelen et al.'s view is that the recognition and use of objects by humans is enabled by sensorimotor brain dynamics that develop during Piaget's pre-verbal somatomotor phase in infancy and persist in adults, enhanced by continuous experience; further, they state that a supervening cognitive mechanism, which is conceived to operate by manipulating symbols and representations of objects according to rules, does not exist in brains. Thelen et al. succeed very well in achieving the aim stated in their abstract: to support "an embodied view by casting the mental events involved in perception, planning, deciding, and remembering in the same analogic dynamic language as that used to describe bodily movement." They are less successful in the form re-stated in their Introduction: "Finding a common language for behavior, body,



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

and brain" as "a first step for banishing the specter of dualism once and for all." While I agree that there is "no time and no task when such dynamics cease and some other mode of processing kicks in," and I believe that they have constructed a strong link between bodily behavior and mentation, the further link between mentation and brain dynamics is weak. (I will refer to the "events" they list as "mentation" in order to make clear that I am not defending AI, although in my opinion "cognition" is the proper word to classify them; Núñez & Freeman 1999.)

2. Reasons for the shortfall are to be found in their initial formulation (sect. 3): emphasis on motor functions (for example, "looking" as precursor to "perceiving," the latter combining proprioception and haptics with vision), use of deterministic dynamics (they rightly avoid neuroanatomical specificity, but they use additive "noise" [equation 8] instead of "chaos" to introduce indeterminacy and unpredictability, reminiscent of Hull's [1943] use of an "oscillator" to salvage his deterministic models), and neglect of intentionality (psychologists, in contrast to lawyers, commonly confuse or conflate "intent" – action to be taken – with "motive" – the reason or explanation thereof; Freeman 1999). Thelen et al. are careful to state that they are using a simple model to point to a new direction of behavioral research. My aim is not to detract from their admirable success in achieving the first link, but to point out some impasses led to by the methods they have proposed for pursuing the second link. In other words, their revolution doesn't go far enough.

3. Their basic concept of "knowing through acting" can be based, rightly I think, on the premise that the neurodynamics of perception and of action are essentially the same. Meanings arise as a brain creates intentional behaviors and then changes itself in accordance with the sensory consequences of those behaviors. Piaget (1954), like Aquinas (1272) before him, called this process "the cycle action, assimilation, and adaptation." It is the process by which the self comes to understand the world by adapting itself to the world. While the behavior-mentation link is best approached through the study of action, the mentation-brain link is more easily approached through the study of perception. Both are creative. Aquinas was first to see the reason. Events in the material world are infinitely complex, hence unknowable. All knowledge is derived through generalization and abstraction. This process can be observed directly by recording the EEGs of the primary sensory areas (Barrie et al. 1996), and it can be understood by modeling it with nonlinear differential equations (Freeman 2000a). Modeling shows that the solutions must be chaotic, because the context-dependent EEG patterns are unique to each individual and cannot be derived by "information processing." They are newly created with each action-perception cycle, manifesting what Bartlett (1932) called "schemata" in labile memories as distinct from "the re-excitement in some way of fixed and changeless 'traces'" (p. vi).

4. The type of dynamics used by Thelen et al. and described in the publications cited by them of Amari (1977), Grossberg (1973), and Wilson and Cowan (1973) is convergent, comparable to the simple bistable models that others (e.g., Kruse & Stadler 1995) have used to study stochastic resonance, in which noise serves to enhance extraction of periodic signals. Thelen et al.'s modeling is akin to that of Gibson (1979), who conceived the extraction of "information" from "objects," which he called their "affordances," by an ill-defined deterministic process of "resonance" in the brain. He did not treat of the origin of the affordance in the brain of the actor, for example, to use a chair not just to sit in but to hold a coat, stop a door, or strike an enemy. The formulation of an action begins with a goal that is expressed by an intent, which becomes apparent in a directed sequence of actions. The most difficult frontier for the link between brain dynamics and behavior lies in the inaccessible interior, the creative and chaotic dynamics of the limbic system (Freeman 1999), which is expressed not only by the formulation and issuance of the neural schemata that induce motor action, but equally important the "corollary discharges" that shape the attractor landscapes of the sensory cortices subserving prediction by preafference (Kay & Freeman 1998) of the sensory consequences of the incipient intended actions.

5. By contrast, the neurodynamics of perception is a piece of cake. Moreover, studies of the mentation-brain link require direct observation of brain activity in behaving persons. Brain imaging of changes in metabolism and blood flow that are induced by neuroactivity is much too slow, and the technology for the noninvasive detection of the accompanying magnetic fields at the scalp is too restrictive of movement. Unit activity alone (Georgopolis 1991) will not suffice. Methods for recording and interpreting the scalp EEG patterns from humans are still under-developed, but spatial spectral analyses of EEG activity (Freeman et al. 2000a) indicate that the finely textured spatial patterns found in animal studies (Barrio et al. 1996; Freeman 2000a) may also be observable over the sensory areas in humans. Here emerges one of the most important inferences in the work under discussion, that the dynamics of intentional behavior in adults is an extension of the dynamics found in pre-verbal infants. One quails at the thought of multichannel EEG recording from squalling infants. In contrast, the study of adult volunteers offers an inviting door into the future.

## An affordance field for guiding movement and cognition

Arthur M. Glenberg,[a] Monica R. Cowart,[b] and Michael P. Kaschak[a]

[a]*Psychology Department, University of Wisconsin-Madison, Madison, WI 53706;* [b]*Philosophy Department, Merrimack College, North Andover, MA 01845.* **glenberg@facstaff.wisc.edu mpkaschak@students.wisc.edu mcowart@merrimack.edu**

**Abstract:** An embodied movement-planning field cannot account for behavior and cognition more abstract than that of reaching. Instead, we propose an affordance field, and we sketch how it could enhance the analysis of the A-not-B error, underlie cognition, and serve as a base for language. Admittedly, a dynamic systems account of an affordance field awaits significant further development.

Thelen et al. claim (sect. 6.4, para. 6), "The unified field is the essential notion of embodiment." Our goals are (1) to discuss why a unified field is important for embodiment, (2) to explore in what sense the Thelen et al. field is embodied, and (3) to offer a sketch of how the proposed field can he modified to provide an embodied account of behaviors more abstract than reaching.

Why is a unified field an important notion for embodiment? It is not because that is the only way in which disparate constraints (e.g., from previous experience and from the current situation) can interact. In fact, abstract symbol accounts of cognition excel at just this sort of interaction. That is, some symbols describe past experience and other symbols describe the current environment. Because all of these symbols are in a common, abstract, amodal currency, they can be manipulated in the same medium, a symbol field if you will, such as working memory. One of the achievements of Thelen of al. is to demonstrate that the field of interaction need not be a symbol field. Instead, it can be a field closely related to action, namely, a movement planning field. Thus, by offering an alternative to symbols, the movement planning field helps to demonstrate the power of embodied theories.

There are several ways in which the movement planning field is embodied. first, the field is concerned with movement, not solely cogitation. Second, the field must he structured in terms of (or have access to) egocentric coordinates that take into account the relation of the body to the environment. That said, however, the field is predominately one of spatial locations to which a reach can be made. Such a field is not sufficient to support cognition and choice in other situations such as choosing between two restau-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

rants or choosing between two job offers. logically, spatial relations are of the wrong ontological sort to make the distinctions needed to consider these choices, and theories attempting to analogically map meaning onto metric space have not fared well empirically (e.g., Langston 1998). Nonetheless, we believe that the notion of an embodied field can be extended in a direction that complements the elegant analyses Thelen et al. advance while allowing for analysis of behavior in other domains. In short, Thelen et al. discuss a "where" field, but eventually, a "what" component must be developed to address issues of intentionality (in the sense of aboutness). Our goal is to provide a sketch of an embodied "what" field.

Our suggestion is for an action field built out of affordances, rather than locations. Affordances (Gibson 1979) are relations between objects and perceivers: how the perceiver, given his or her body, can interact with an object. Clearly, egocentrically coded spatial location plays an important role in affordances: how one can interact with an object is partly determined by its distance and location relative to the perceiver. In addition, affordances (and thus the field) must also he sensitive to experience, maturation, and motor control. For example, apprehension of some affordances seems to depend on a type of observational learning: It may not be obvious that a chair affords protection from a snarling animal until one has seen a lion tamer perform such an act. Other affordances depend intimately on bodily experience and control: To the infant, a pencil, a pen, and a cigar all afford pretty much the same thing, namely, moving it to the mouth, because the infant's body does not allow it to make finer distinctions that will come with greater motor control.

More important, the Thelen et al. field meshes, or combines, information from various sources. One constraint on mesh is spatial proximity. That is, interactions in the field only occur within the spatially-defined interaction kernel specified by Thelen et al.'s Equation 4. In an affordance field, however, the mechanism of combination must be sensitive to how the body can smoothly mesh actions. Again, spatial location is relevant, hut not the only source of constraint (Glenberg 1997). For example, we can mesh the affordances of a chair with the actions of eating: sit in the chair and eat. We cannot, however, mesh the affordances of a chair with the actions of swimming. In both cases the object and the actions are juxtaposed, but because of the nature of our bodies, the affordances and actions smoothly combine in the former case, but not the latter. Unfortunately, what is missing from the notion of an affordance field is exactly how different affordances can constrain their combination (an "affordance interaction kernel") and hence the elegant mathematics of dynamic systems analysis.

Nonetheless, an affordance field has the potential to handle some aspects of the A-not-B data that Thelen et al. do not address. For example, Thelen et al. note that the typical situation is confusing for infants. But why is it not equally confusing for older children? One suggestion is that the A and B locations in the typical situation have similar affordances for the infant, much like the pen and the cigar. It is only after the infant learns to make finer distinctions based in part on finer modes of physical interaction that the differences between the containers become more apparent and consequently the situation less confusing.

Granting the action of an affordance field, is there any reason to believe that it could be the basis for cognitive activity more abstract than reaching? Although space does not allow development of the idea, we point to three research programs. First, Ellis and Tucker (1999) develop the philosophical and neurological justification for treating affordances as intentional representations. Second, Newton (1996) develops a foundational account of all understanding (including language) based on action. Her account begins with what it means to understand an action, namely, to have an action plan, and moves to the understanding of objects and tools in terms of how they affect action plans, that is, their affordances for action. She then develops an account of language as a type of tool. Third, Glenberg and Rohertson (2000) and Kaschak and Glenberg (2000) have provided empirical support for an action-based approach to language comprehension. Clearly, these examples are only the beginning of a program for grounding cognition in action, but they serve to illustrate that the idea is not fanciful.

ACKNOWLEDGMENT

Michael P. Kaschak was supported by a fellowship from the National Science Foundation during the preparation of this commentary.

## Why the Piagetian A-not-B phenomenon is no error: A comparative perspective

Jack P. Hailman

*Archbold Biological Station, Venus, FL 33960.* **jhailman@facstaff.wisc.edu**

**Abstract:** A-not-B behavior in various mammals and birds suggests it has been selected for during evolution. One scenario is that displacement to B of one food item from a trove at A should not distract the forager. Piagetian stage V experiments may not test for object permanence, but rather for the more abstract notion that physical objects can be unique.

As Thelen et al. ably document, many researchers doubt Piaget's interpretation of the A-not-B phenomenon as a deficiency in cognitive object permanence. The "dynamic systems" theory articulated by Thelen et al., however, still treats A-not-B as an error, in this case implicitly explained away by proposed proximate mechanisms. In essence, the developing infant is viewed as being constructed in such-and-such a way, and that construction results in the child's seeking an object at a place formerly occupied (A) rather than at its new location (B). A behavioral biologist finds such reasoning incomplete because description of the proximate mechanism does not reveal its raison d'être. A biologist asks: If A-not-B behavior is truly an error, why has it not promoted an evolutionary change in the proximate mechanism that would avoid the error? This question is often called the ultimate factor, and less formally, the ultimate-proximate distinction is known as the "why and how" of behavior. Usually, the most powerful means at our disposal to answer "why" is the comparative method – seeking insights from the similarities and differences among both closely related and distantly related species (e.g., Hailman 1976; 1981; 1998)

Fortunately Piaget has stimulated animal studies of object permanence as well as the extensive literature on infant development. By my count (mercifully not documented here) there exist one to 12 object-permanence papers on at least two species of great apes, four macaques, three other monkeys, three non-primate mammals, five parrots, and two other species of birds. From this literature we learn that the A-not-B phenomenon has been found in at least the chimpanzee (*Pan troglodytes*) (Mathieu & Bergeron 1981), gorilla (*Gorilla gorilla*) (Natale 1989), capucin (*Cebus apella*) (Spinozzi 1989), crab-eating macaque (*Macaca fascicularis*) (Poti 1989), Japanese macaque (*Macaca fuscata*) (Natale 1989; Poti 1989), African grey parrot (*Psittacus erithacus*) (Pepperberg et al. 1997) and New Zealand parrot (*Cyanoramphus auriceps*) (Funk 1996). A relatively clean test with animals is often difficult; for example, Gruber et al. (1971, p. 14) lamented that their house cats (*Felis catus*) "pursued the visible, moving object too persistently" and quickly to permit relevant tests of Piagetian stage V. Methodological quibbling permeates this literature, with one group of researchers claiming that neither cats nor puppies (*Canis lupus*) commit the A-not-B "error." Uncertainties aside, the widespread if not universal occurrence of the A-not-B phenomenon in mammals and birds has both proximate and ultimate implications. No specific proximate brain mechanism proposed to underlie the phenomenon in infants is likely to generalize well to non-primate mammals or especially to birds. This phyleric constraint supports the reservations expressed by Thelen et al. concerning Diamond's prefrontal cortex proposal. With respect to ul-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

timate factors, since few if any species have evolved unequivocal avoidance of A-not-B, perhaps the behavior has actually been selected *for* by evolution and hence is no error at all.

Insofar as I can tell, no one has previously suggested a scenario in which natural selection would favor A-not-B behavior, but such a scenario emerges readily from natural history. For a concrete example consider the North American woodpecker known as the northern flicker (*Colaptes auratus*), which despite its lineage spends a lot of time on the ground eating ants. Some kinds of ants build familiar mounds but in other species the nest is wholly underground, accessed by one or more ant-sized holes. Suppose the bird sees an ant run down into such a hole, and then an ant emerges from the hole and scuttles off quickly into the nearby leaf litter before the bird can strike. The hole is A and the leaf litter is B: Where should the bird search for a tidbit? Clearly A-not-B behavior is no error for the flicker, and one can readily imagine many similar situations for various animal species.

The animal scenario for A-not-B being a functionally correct choice rather than an error suggests a reconsideration of Piaget's notion of object permanence. Piaget invented the visible displacement protocol while playing games with his own children, and he reasoned that in eventually overcoming the A-not-B "error" the child's conception of the permanence of physical objects had improved. But what is it that the visible displacemant test really asks concerning what a child or animal has (or has not) learned? I think that what it asks goes beyond the subject's understanding that physical objects have permanence. I suggest that the test asks the child or animal whether it has learned this critical rule: There is only one object in the game being played – as opposed to a hoard of virtually identical objects hidden in one place like ants in a nest. If there is a unique object, then when it is visibly displaced from A to B, location A must logically be devoid of further similar objects. Only if a subject understands the object that initially went to place A must be the same object that moved from A to B does the A-not-B choice become an error. Understanding the notion of uniqueness of an object would seem to depend on first understanding that external physical objects have permanent existence. I therefore submit for consideration the simple thesis that Piagetian stage IV performance (recovering an object seen to be hidden) adequately shows object permanence full-blown. Visible displacement (stage V) and invisible displacement (stage VI) are testing for derivative cognitive achievements such as the more abstract notion of uniqueness of objects.

## Bridging the gap: Dynamics as a unified view of cognition

Derek Harter,[a] Arthur C. Graesser,[b] and Stan Franklin[c]

[a]*Department of Mathematical and Computer Sciences and the Institute for Intelligent Systems,* [b]*Department of Psychology and the Institute for Intelligent Systems,* [c]*Department of Mathematical and Computer Sciences and the Institute for Intelligent Systems, The University of Memphis, Memphis, TN 38152.* **{dharter; a-graesser; franklin}@memphis.edu**
**www.msci.memphis.edu/~harterd/**
**www.mnemosyne.csl.psyc.memphis.edu/home/graesser/**
**www.msci.memphis.edu/~franklin/**

**Abstract:** Top-down dynamical models of cognitive processes, such as the one presented by Thelen et al., are important pieces in understanding the development of cognitive abilities in humans and biological organisms. Unlike standard symbolic computational approaches to cognition, such dynamical models offer the hope that they can be connected with more bottom-up, neurologically inspired dynamical models to provide a complete view of cognition at all levels. We raise some questions about the details of their simulation and about potential limitations of top-down dynamical models.

A useful top-down dynamical model should not only simulate what is already known to be the case empirically but should also take the lead in identifying potentially illuminating patterns of simulated data for future testing and experimentation. With this in mind, we built a quick computer simulation of Thelen et al.'s model and were intrigued by what we found. We start this commentary by sharing some of these observations.

In the model presented by Thelen et al., the $h$ control parameter sees a resting level for the field that in effect controls the amount of interaction exhibited among the sites of the field. By varying the resting level of the field, the model displays behavior that is largely driven by its inputs on one extreme, to a self-sustaining mode where sites excite and inhibit one another and can sustain activation when it is no longer present as input (sect. 4.1.3) The results presented for the critical B trial (sect. 5.1.4, Fig. 12) show that without the self-sustaining excitation in the non-cooperative regime, the history of past performance soon overrides the specific input of the hiding event and the A site is selected. In the cooperative regime meant to simulate more mature infants, the self-sustaining activation of the field causes it to successfully remember the specific input through the delay so that the model predicts a selection at site B. However, it appears that in the simulations, at least for a value of $h = -6$, the activation peak will nor decay rapidly enough to predict perseverative reaching for delays of 5 seconds or longer for these more mature infants. But there is more to this story than merely fussing about whether the model's parameters predict the appropriate behavior.

We have questions about the predicted patterns of behavior as a function of varying the values of $h$. Are there resting field values in which the self-sustaining activation of the sites is not sustained indefinitely, but can eventually decay once the specific input is no longer present? That is to say, is there a change in the behavior of the model, from $h = -12$ to $h = -6$, so that at some values the cooperative regime only lasts for a limited amount of time? Are there values of $h$ where a combination of cooperative and non-cooperative regimes are observed during a single simulated run of the experiment? Or does the model in effect switch from one mode of behavior to another at some value of $h$ (e.g., an abrupt bifurcation)? Without this domain of a mixed regime of cooperative and non-cooperative behaviors, it becomes unlikely that manipulation of the resting field level effectively captures the maturation of infant performance, because such performance is characterized by a rapidly increasing ability to tolerate delay (among other effects) during the critical phase of development.

From our own explorations of the model, it does appear that there exists a region of the resting field control parameter that exhibits a mixture of cooperative and non-cooperative behavior. This area of the $h$ parameter is very small and shows rapid change from a completely non-cooperative regime to a completely cooperative one. Interesting enough, the rapid area of change that we observed would presumably be expected if the model does capture some aspects of real development. This area (and not at $h = -6$) more appropriately represents 12-month-old infant's performances. Regimes dominated by cooperative behavior, like $h = -6$, represent an ability of short term memory to cue behavior towards the correct location for an indefinite amount of time; this is representative of very mature adult performance. But there is also an area of mixed characteristics that represents a very exciting region of the model; very small changes in the levels of the specific task and memory input will cause disproportionate nonlinear changes in the predicted behavior and capabilities to tolerate delay. It remains to be seen how well predictions from studying the model in this region will correlate to actual observed behavior and make predictions of unseen effects.

Such questions about the effectiveness of various control parameters to capture the processes of maturation do illustrate the limitations that such top-down dynamical models have in providing satisfying explanations of cognitive phenomenon. Clark, who calls such models "pure" dynamical models, puts it this way:

> All these "pure" models do not speak directly to the interests of the engineer. The engineer wants to know how to build systems that would ex-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

> hibit mind-like properties, and, in particular, how the overall dynamics so nicely displayed by the pure accounts actually arise as a result of the microdynamics of various components and subsystems. (Clark 1997, p. 120)

In spite of this limitation with dynamical models, they do offer an exciting alternative to standard computational models of cognition. It has always been difficult to imagine how the dynamics of action and motor processes could he connected to disembodied, context free models such as those studied in traditional computational approaches (Hendriks-Jansen 1996). Dynamics as the language of cognitive processes offers hope as a unified model, where the dynamics of groups of neuronal elements give rise and organize into higher levels of dynamical description. Top-down dynamical models, such as this one, seem to offer a clearer path towards a future synthesis of all levels of explanation of cognition. Neurologically inspired, bottom-up, dynamic embodied models, such as those proposed by Edelman and Freeman (Edelman & Tononi 2000; Edelman 1987; Freeman 1999; Skarda & Freeman 1987) exhibit top-down dynamical behavior of the type presented here by Thelen et al. We are convinced that a synthesis of all levels of cognitive explanation through a language of dynamics and embodiment offers the appropriate viewpoint of cognition.

## Mirror writing: Adults making A-non-B errors?

Mark L. Latash

*Department of Kinesiology, Pennsylvania State University, University Park, PA 16802.* **mlll@psu.edu**

**Abstract:** Errors and episodes of "freezing" seen during mirror writing by adults can be incorporated into the model suggested in Thelen et al.'s target article This requires assigning an important role to internal inverse models stored in memory. The strongly anti-dualism position of Thelen et al.'s leaves little room for the Bernsteinian notion of activity.

I would like to suggest that Thelen et al.'s very powerful and attractive hypothesis can also be used to interpret phenomena of motor errors by adults when they are required to perform a well practiced task in unusual conditions.

When a person tries, for the first time, to write so that the letters look right in the mirror rather than on paper, major problems emerge that include "freezing" of the extremity, slowness of movement, and many incorrectly written letters (errors). Recently, we have performed a study of the effects of practice on mirror writing (Latash 1999). Subjects were asked to practice writing a standard phrase for three days, while changes in the amount of time and the number of errors per phrase with practice were monitored. The subjects were also asked to report their subjective, introspective feelings during the experiment. Virtually all the subjects reported feeling very clumsy during their first attempts at mirror writing with frequent episodes of an inability to move the limb at all. In such cases, a mental effort helped them get out of the impasse and continue the task. By the end of the first day of practice, most subjects did nor experience freezing episodes, although errors were still present.

After three days of practice, a dramatic improvement occurred leading to a drop in both movement time and the number of errors per standard phrase. Features of individual handwriting could be seen clearly in the mirror images of the mirror writing samples. Large transfer effects were seen to writing by the non-dominant extremity, which did not practice, and to new phrases. The high degree of transfer of the effects of practice to the non-dominant hand suggests that this coordination has been learned using external space variables rather than effector-specific variables. On the other hand, the high degree of transfer to new phrases suggests that the effects of learning were not specific to external coordinates or trajectories either, but more general and applicable to the whole pre-existent repertoire of movements associated with everyday writing.

In the earlier paper (Latash 1999), we interpreted the findings within the general idea of the central nervous system building an internal inverse model reflecting the properties of the moving effectors, external force field, and maybe other variables (cf. Wolpert et al. 1998). Planning in task space variables translates into a space of internal variables (an inverse transformation done by an internal model) and then leads to adequate performance in the task space (a direct transformation). During mirror writing, actual trajectories of the tip of the pen on the paper differ from those during regular writing. Hence, individual joint trajectories, muscle force patterns, and muscle activation patterns should all be different as well. Thus, to assure accurate mirror writing, an internal model should map the same input (same letter shapes) onto a different output (different patterns of control signals), that is, a new model should be created.

Problems that typically occur during first attempts at mirror writing are also seen during first attempts at mirror drawing (Cook 1933) Writing is a highly automated, individualized movement, while drawing is not. Hence, one may assume that problems with mirror writing are related not to a strong internal memory-based input specific to the personal handwriting of the subject but to a more general problem with mapping control signals onto required trajectories in external coordinates. If so, internal inverse models should be part of the memory input suggested by Thelen et al. In the A-non-B errors, an important role is played by memory on recent reaches, while during mirror writing, the source of errors is using inadequate internal inverse models built during the lifetime.

Many subjects reported that their hand sometimes refused to move in a required direction, when they looked in the mirror, and stubbornly moved in an opposite direction or stayed motionless as if it had a mind of its own. They also reported that looking aside and imagining the hand and the task was very helpful in avoiding such errors and getting out of the episodes of freezing. Apparently, visual feedback was a factor that hurt rather than helped the performance. Within the theory suggested by Thelen et al., interplay between memory, perception, and action defines the act of reaching. An important feature of the model is cooperativity that is assumed to be immature in younger infants who typically display A-non-B errors. Does the mirror-distorted feedback temporarily bring adults back to infancy with its hypothesized prevalence of the non-cooperative mode of functioning assumed in the model?

Reports by subjects, mentioned in the last paragraph, question Thelen et al.'s claim that knowing is perceiving, moving, and remembering. Apparently, a purely intellectual effort was able to help the subjects get out of the impasse. The strongly anti-dualism position of Thelen et al. seems to leave no room for the notion of activity, as envisioned by Bernstein (1966/1998), and moves dangerously close to a stimulus-response account: Pavlov would definitely agree that behavior is shaped by experience (memory), perception, and ongoing activity. I think that Thelen et al.'s theory is valid as long as the mind is substituted with an instruction, either explicit or implicit, or with a sequence of stimuli that are strong enough to override initiative.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

## Self-organizing brains don't develop gradually

Marc D. Lewis

*University of Toronto, Toronto, ON M5S 1V6, Canada.*
**mlewis@oise.utoronto.ca**

**Abstract:** Some dynamic systems approaches posit discontinuous changes, even universal stages, in development. Conversely, Thelen and colleagues see development as gradual because it relies on real-time interactions among many components. Yet their new model hinges on one parameter, neural cooperativity, that should change discontinuously because it engenders new skills that catalyze neural connectivity. In fact, research on cortical connectivity finds development to be discontinuous, and possibly stage-like, based on experience-dependent and experience-independent factors.

Thelen and her colleagues cap a decade of leadership in "dynamic systems" approaches to development by shifting gears into the racy (for developmentalists) world of formal modeling and simulation. This is just what their critics have demanded (van der Maas 1995; van Geert 1996), and the result is both powerful and provocative. I find their model exemplary among developmental approaches, but it comes with some unnecessary baggage: the notion that development, because it is not centrally controlled, and because it represents the interaction of contextual and organismic constituents, must be gradual. Other dynamic systems researchers see development as discontinuous, even stage-like (Lewis 2000; van der Maas & Molenaar 1992; van Geert 1998), and so do some developmentalists who have specifically examined A-not-B performance (Zelazo et al. 1998). Granted, toddlers have problems finding toys in sandboxes, and even adults can search for a kitchen utensil in its old location, long after A-not-B know-how has come on line. But then most developmental gains are like that. My theory of mind goes out the window when I'm arguing with my teenage daughter, but I still believe I "got it" at the age of 3½. In this commentary, I argue that developmental change is discontinuous, consistent with dynamic systems assumptions and research on developing cortical connectivity, and that Thelen et al.'s model implies exactly that. Development may also correspond with universal stages, but that's a separate issue, as I will show later.

Thelen et al. have long argued that gradual changes in many parameters are responsible for the developmental shifts to which their interactions give rise (e.g., walking, reaching). And they insist, probably rightly, that the interactions of these multiple components are highly sensitive to contextual forces. Hence, no single parameter should be held responsible for developmental change. Yet the present article suggests a parameter like no other: neural cooperativity. Cooperativity represents the mutual excitation of closely connected neurons involved in perception-planning-action sequences. Given high cooperativity, a selfsustaining feedback loop evolves in real time, permitting older infants to hold onto the memory of the recent B target and not be swayed by the inter-trial memory build-up favoring the A target. In the model, cooperativity was produced by increasing connectivity among sites. However, the authors emphasize that developmental change is gradual, not switch-like, for this and other parameters, and that's why A-not-B performance remains plastic and sensitive for so long. Moreover, they pit their gradualist position against the idea that a maturational change in the prefrontal cortex, supporting some central mechanism such as working memory, is responsible for A-not-B success. After all, they remind us, brain development is experience-dependent.

But that's the point! Cortical connectivity increases as a result of coactivation or cooperation among neurons (Changeux & Danchin 1976; Greenough et al. 1987; Hebb 1949). This in itself implies a positive feedback relation, which should make development in a task or domain look exponential rather than gradual. But there's more to the story. When cooperativity among a set of neurons produces a novel skill (e.g., retrieving hidden objects successfully from one of several locations), it should recur much more often and hence give rise to a steep gradient of increasing connectivity-cooperativity. Self-organizing systems are characterized by circular causality, in which a higher-order emergent form entrains or enslaves the cooperative interactions that give rise to it (Haken 1987). In brains, these higher-order forms are intentional states directing action toward specific goals (Freeman 2000b; Kelso 1995). When neurons couple in the service of a new goal, there are good reasons for them to couple again. The first time an infant successfully reaches for a nonobvious target and retrieves what he or she is looking for, both the intention and the capability for doing so are strengthened.

Thus, developmental shifts, and I mean the abrupt, qualitative shifts that Thelen and colleagues dislike, seem inevitable in a nonlinear system in which cooperativity begets novel functions which further catalyze cooperativity. In fact, in their landmark volume, Thelen and Smith (1994) argued that whatever combinations "work" get selected out of massive variability, becoming attractors that stabilize in development. Indeed, emergent skills round up the variability of their underlying neuronal circuits and recruit them to enduring patterns of connectivity. Thus, discontinuous shifts make sense, not only in nonlinear dynamic systems, not only in self-organizing brains, but also in Thelen and her colleagues' own vision of development.

Then why don't they reach the same conclusion? The attractive thing about gradualness may be that it sounds incompatible with Piagetian stages. Not necessarily because Piagetians believe in sudden transitions. Many of them do not and, interestingly, many who do take a dynamic systems approach (van der Maas & Molenaar 1992; van Geert 1998). But Piagetians do believe that development is constrained by central cognitive parameters, and they do hold this kind of engine responsible for universal stages. Thelen and colleagues want to emphasize that many factors internal and external to the organism contribute to the organization of behavior at each moment of development. But they conflate stages with discontinuities, and they end up throwing both away too quickly.

I have argued that Thelen et al.'s model of neural cooperativity, and the experience-dependent synaptogenesis it suggests, could generate discontinuities without universal stages. But that doesn't mean there are no stages. I end with a look at Thatcher's (1998) analysis of developmental change in EEG coherence. EEG coherence reflects cortical connectivity, and Thatcher reports the following: It grows exponentially for the first six years; it deviates from this trajectory in abrupt oscillations that look like phase transitions or bifurcations in a dynamic system; it is best modeled by equations with positive and negative feedback components that represent both cooperation and competition. So far, Thatcher's methods and models suggest the exponential-discontinuous growth profile that Thelen and colleagues should have predicted. But Thatcher also finds that the major oscillations in EEG coherence correspond quite well with Piagetian and neoPiagetian stage transitions! Thatcher concludes, along with Greenough and colleagues (1987), that cortical development is driven by both experience-dependent and experience-independent (dare we say maturational?) influences. Granted, Thatcher's phase transitions are highly global, they have not been observed in the first year, and they do not correspond all that well with substage transitions. So the jury is still out on the staginess of the A-not-B task. But his research does show that the discontinuous growth profile of a self-organizing brain is not incompatible with stages.

Thelen et al.'s model provides a powerful way to conceptualize an emergent skill in terms of the interactions of perceptual, planning, memory, and movement parameters, and a bridge to neurodynamics that is badly needed in developmental theory. But the implications of the model, and of the approach in general, may not be incompatible with the phase transitions, and even stage transitions, that other developmentalists still find attractive. Thelen et al. have taken the A-not-B phenomenon and moved it to a new hiding place, and they advise us to stop looking in the old one. I'm suggesting that we don't give up on the notion of stages quite yet: we might find something much more interesting than what was originally hidden.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

## An embodied theory of cognitive development: Within reach?

Jeffrey J. Lockman

*Department of Psychology, Tulane University, New Orleans, LA 70118.*
**lockman@tulane.edu** **www.tulane.edu/~psych/psychome.html**

**Abstract:** Thelen et al. not only offer an important new theoretical account of the Stage 4 object permanence error but provide the foundation of a new theory of cognitive development that is grounded in action. The success of dynamic field theory as a more general account of cognitive functioning, however, will depend on the degree to which it can model more generative capacities that are not limited to simple choice situations. Imitation and problem solving are suggested as two capacities that might be productively modeled within their approach.

Thelen et al.'s target article represents, or I should say, embodies, a major achievement. Piaget's proposals about object permanence and specifically the A-not-B error have intrigued developmental psychologists for decades. The reasons go beyond a desire to understand infant search for hidden objects. Piaget's work on object permanence was meant to serve as a window onto the origins of symbolic thinking, means-ends problem solving, spatial knowledge, selfenvironment differentiation and so forth – in short, cognitive capacities that are thought to make human cognition human.

The important messages from this paper are not only that Piaget's interpretations about infant search behavior are incorrect (as well as related interpretations by legions of researchers who have investigated object permanence), but so too are his sweeping proposals about sensorimotor development and its relation to subsequent cognitive development. As more than implied by Thelen et al., their work calls for a new type of theory about cognitive development and cognition in general. But are behavioral scientists other than those of the dynamic systems persuasion ready to heed the embodiment cry? To address this question, let me make a few observations about what Thelen et al. have accomplished with respect to the object permanence literature and what needs to be accomplished for an embodied theory of cognition to take wider hold in our discipline.

To say that the object permanence literature is an unwieldy one is something of an understatement. That Thelen et al. have managed to bestow a compelling new action-based theoretical organization upon this literature with new testable predictions is of considerable consequence. Many of the methodological points that Thelen et al. make about previous research on object permanence are extremely well taken. For instance, the idea that the training trials, which precede full hiding at location A, likely influence the probability of search at B and thus preclude clear interpretations of many past reports now seems patently obvious. But many of us infancy researchers are probably collectively scratching our heads and asking, "How could we have missed this?"

Still, and without taking anything away from Thelen et al's achievement, the A-not-B task is particularly well suited to the demands of dynamic field theory. In the modal version of the task, two responses to two different locations are pitted one against the other, but the responses themselves (reaching) are in an important sense the same. (A larger issue here is what constitutes a new response or transfer or generalization – pivotal considerations for this modeling effort, but which go beyond the scope of this commentary.) Whether such a constrained set of affairs is representative of most cognitive acts performed by children or for that matter adults is open to question. Of course, the Stage 4 AB task is an experimental instantiation of a psychological ability that in real life is subject to countless variations. Indeed, most cognitive acts in real time, even those of young children, are generative. Children are constantly creating new acts that are not just repetitions of past ones. Unlike the Stage 4 AB task, these acts entail more than a change in location. The generative types of abilities that I have in mind include the wonderful flights of fancy that children engage in during pretense play. But this type of generative capacity is also evident in actions that are more grounded in reality, as is the case when children explore a new object, environment or event. Perhaps it will be possible to model these behaviors or sequences of behaviors with dynamic field theory. Perhaps pretense is not so unconstrained after all and exploration of novel environments, events or stimuli occurs in predictable ways. Nevertheless, the extension of dynamic field theory to situations other than those that require simple choices will insure its wider acceptance.

Along these lines, let me suggest two prime areas in the cognitive development literature to which dynamic field theory might be applied. The first is imitation. Imitation is fundamentally a motor act. Nevertheless, most developmental work in the area, at least in the first years of life, has centered on the Piagetian issue of representation. Newborns have been studied to determine whether they possess even a slender capacity to represent other's actions and older infants have been studied to examine whether they can represent and reproduce others' actions after substantial delays (Meltzoff 1985). One of the virtues of Thelen et al's embodied cognition approach is that rather than asking whether representation is present or not, it directs us to consider strengths of responses in the context of the organism's immediate and past response history. Applied to imitation development, this approach would call for a very different set of research questions. Investigators might better examine the conditions (near and longer term) that promote imitation of a target action and the degree to which a match is achieved. Representation/memory would be one component of the model but neither a privileged nor the only one.

Another basic cognitive ability that appears to be a promising candidate for dynamic field modeling is problem solving, especially the so-called phenomenon of insight. The favored (disembodied) visual metaphor for this process is a flash-bulb suddenly being illuminated in the thinker's head. This characterization, however, may not accurately portray the processes and associated behaviors that lead up to the problem solution. The trial and error behaviors that routinely accompany young children's problem solving attempts and even those of adults, may also contain elements of the correct solution to the problem, as occurs when a child is learning to use a tool (Lockman 2000). Modeling the dynamic field that comprises children's actions during so-called trial and error exploration might provide insights into the processes that underlie problem solving.

In sum, Thelen et al. have provided a fundamental challenge to investigators of cognitive development, a challenge that goes beyond where infants choose to search for an object. The reach of dynamic field theory, however, will depend critically on the degree to which it can account for a vast array of cognitive phenomena that do not fall neatly into simple motor choice situations devised by experimental psychologists.

## On the need for conscious control and conceptual understanding

Stuart Marcovitch and Philip David Zelazo

*Department of Psychology, University of Toronto, Toronto, ON, M5S 3G3, Canada.* **{stuartm; zelazo}@psych.utoronto.ca**
**www.psych.utoronto.ca/~zelazo**

**Abstract:** The dynamic systems approach simulates a wide range of effects and generates novel predictions, but it fails to explain age-related behavioral changes in psychological terms. We argue that the roles of conscious control and explicit knowledge must be addressed in any model of A-not-B performance, and a fortiori, in any model of goal-directed action.

The dynamic systems approach employs a single set of mathematical equations to model various psychological processes within a common framework. Thelen, Schöner, Scheier, and Smith focus on the well-studied A-not-B error to demonstrate the wide applicability and utility of dynamic systems modeling. Thelen et al.


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

present an extensive, but nonetheless selective, review of the effects of different variables on A-not-B task performance. Their model not only accounts for these influences, but also, more important, it generates novel and testable predictions. Furthermore, the recursive nature of the dynamic systems equations allows the model to simulate behavior in real time. This can be an important window into the development of psychological systems within the context of a given task.

Despite these strengths, however, we believe that the approach presented by Thelen et al. requires a better description of the developmental mechanisms that lead to the age-related changes in behavior observed in infants and young children. Thelen et al. assert that "the main developmental effect lies in *h,* the ability of the field to enter the cooperative self-sustaining regime" (sect. 4.1.3, last para.). They define a cooperative field as one that is "endowed with interactions such that sites that are close together are mutually excitatory, whereas more distant sites are inhibitory" (sect. 4.1.3, first para.), and they use one value (−6) for fields in a cooperative regime, and another value (−12) for fields in a non-cooperative regime. In other words, all things being equal, what separates the infant who reaches perseveratively on the first B trial from the infant who reaches correctly is the state of cooperativity, which is effectively either "on" or "off." The emergence of this cooperative field should be linked to the development of certain psychological processes, but the target article fails to explain what these might be. For example, does cooperativity correspond to conceptual understanding of object permanence? Is it reasonable to assume that this change is saltatory?

One aspect of psychological development that is ignored by the dynamic systems approach is the ability consciously to override previous habits (i.e., conscious control). In contrast, the development of conscious control is a central feature of our hierarchical competing-systems account which is based, in part, on the findings of our meta-analysis of the A-not-B error (Marcovitch & Zelazo 1999). Our model describes two dissociable systems that work in conjunction to guide behavior. The first system is response-based, and its influence on search behavior increases as a function of the number of "A" trials. The second system is a conscious representational system that has the potential to control the response-based system. Development consists mainly in the growth of this conscious representational system through experience-dependent maturation. As the infant develops, he or she becomes increasingly aware of his or her own conscious states, and consequently becomes increasingly able to use this knowledge to guide behavior (Zelazo & Zelazo 1998). We quantified the effects of these systems by combining two functions, each with one free parameter. The response-based system was estimated by an exponential function and the representational system was quantified by a Gaussian distribution (see Marcovitch & Zelazo 1999, for details). The model tested favorably on data collected using a multilocation A-not-B task (Diamond et al. 1994).

The hierarchical competing-systems account has much in common with the dynamic systems approach. The response-based system clearly has its own dynamics, and the strength of the system is a function of motor experience in the context of the task. As a result, similar predictions arise from the two approaches. For example, both theories predict perseverative behavior on tasks with no hidden toys, and neither theory invokes a separate inhibitory mechanism to account for perseverative behavior.

Both approaches also predict an effect of the number of "A" trials, although they do so in different ways. Indeed, this is a crucial difference between the two approaches. As far as we can tell, the dynamic-systems approach would predict that the effect of the number of "A" trials occurs across the life span, although presumably this effect interacts with cooperativity. However, our model predicts that the effect is only manifested in the absence of the influence of the conscious representational system, such as might occur in infancy and in certain contexts later in life (e.g., when attention is divided). To illustrate the distinction between the two approaches, imagine an alert adult on the simple A-not-B task. He or she will be unlikely to perseverate, regardless of the contextual manipulations. For example, even after reaching to the "A" location 100 times, an adult will switch easily to the "B" location when the object is visibly hidden there. In contrast, a 9-month-old infant would be increasingly likely to search perseveratively as the number of "A" trials increases. To describe these age differences simply in terms of increased cooperativity seems reductionist and fails to address the way in which conscious representations (and conceptual knowledge) can override previous motor habits.

The ability to maintain a conscious representation (e.g., of the object's current location) appears to be a prerequisite for explicit conceptual understanding (e.g., object permanence). A toddler who consciously represents the object at location "B" and searches there despite an extensive history of searching at location "A," does so because he knows the location of the object (although this infant may be unable further to reflect upon this knowledge), and this knowledge has consequences for behavior. In general, increasing awareness of one's knowledge permits increasingly flexible uses of that knowledge, including the coordination of apparently incompatible pieces of knowledge (Zelazo 1999). The dynamic systems approach does not separate conscious, explicit knowledge from other forms of knowledge (e.g., motor) and provides no mechanism whereby the way in which knowledge is represented (conscious vs. unconscious, explicit vs. implicit) may have functional consequences. We see this as a major weakness in the dynamic systems approach.

The dynamic systems approach is successful insofar as it can simulate infant behavior on the A-not-B task and generate new predictions. However, the mechanisms that drive the main developmental parameter, cooperativity, are left unexplained and it remains unclear how to understand the change in cooperativity in psychological terms. Eventually children acquire the ability to override previous habits, and this ability appears to be a product of explicit understanding and conscious control. A future challenge for the authors will be to explain how changes in the model's parameters embody changes in these fundamental psychological processes.

## Can there be embodiment without a body/brain?

Denis Mareschal

*Center for Brain and Cognitive Development, School of Psychology, Birkbeck College, London, WCIE 7HX, United Kingdom.*
**d.mareschal@bbk.ac.uk** **www.psyc.bbk.ac.uk/staff/dm.html**

**Abstract:** A mature science strives to provide causal explanations of observed phenomena rather than focusing on taxonomic descriptions of data. A field theory model is a step towards providing a truly scientific account of development. However, the model is under-constrained in that it ignores the boundary conditions defined by the physical constraints imposed by the infant's developing brain and body.

The eminent nineteenth century physicist Lord Rutherford once wrote that there were two kinds of science: physics and stamp collecting. Apart from expressing a strong bias for his own field of research, what Rutherford was trying to say was that, in the nineteenth century, physics was the only science to propose mechanistic theories of what caused phenomena. All other natural sciences (e.g., botany, zoology, and anatomy) consisted in the meticulous description and classification of observations but did not provide explanations of these observations. For Rutherford, the sign of a mature science is its focus on causal explanations rather than detailed description.

Mathematics has long been the language of mechanistic theories. Through mathematics we can express how idealised systems evolve over time. Moreover, the recent boom in computer tech-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

nology means that we can now implement some of these mathematical models as computer simulations and watch the models evolve over time. This provides us with a tangible tool for exploring how changes to our mechanistic theories affect the emergence of behaviour (perhaps through changing some parameters of the model). Finally, possibly the greatest contribution of computational models is that they constitute an explicit common platform from which the research community as a whole can take a debate forward.

In providing a formal, well-specified, mechanistic account of development that is implemented as a working computer simulation, Thelen et al. and colleagues have taken an important step towards what Rutherford would call mature science. They are to be commended on the model's ability to capture such a broad range of behaviours in terms of a single set of processes, the way the model incorporates apparently deviant behaviour within the continuum of normal behaviour, and the way they build a bridge between behavioural development in infancy and existing theories of motor control.

Of course, in an imperfect world, all good things can be improved upon. This is definitely the case with the field theory model proposed. In its current form, this model is underconstrained. Thelen et al. recognise that there are a large number of systems that would implement the field dynamics they describe. To resolve this problem one needs to recognise that the infant is actually embodied within a specific physical body. To understand what causes development in infants, we must be able to link the emerging behaviour to the unfolding of the underlying biological structures. This means incorporating developing motor control constraints and brain development constraints within the model. To put it another way, we need to specify more fully the boundary conditions of the field describing behaviour and development.

Because the model is underconstrained, it is difficult to identify what class of computational systems are subsumed within the dynamics of the field equations. For example, it is difficult to tell whether the Munakata (1988) model is not simply a constrained implementation of the same field equations (i.e., a degenerate case of the more general theory). Furthermore, there are a number of parameters in this model that require an arbitrary setting (e.g., the crucial background activation value $h$) linking these more closely to neuroanatomical constraints would reduce the apparent arbitrariness of these values. Alternatively, given the level of abstraction of the models, one may prefer to see that this model's response is not critically dependent on a narrow band of parameter values. Such a finding would add substantial weight to the generality of the account proposed.

One possible source of constraints on the model is the connectivity of the developing brain. We are beginning to understand more and more about the developing connectivity and functionality of different parts of the brain during infancy. There is now ample behavioural and ERP evidence that while dorsal and ventral routes may be functionally operational from a very early age, the ability to co-ordinate information within these representations emerges gradually over the first nine months, and depends largely on the task used to probe competence (Johnson et al., in press). Indeed, I have argued that the lack of co-ordination between dorsally and ventrally coded information plays a critical role in explaining infant object-directed behaviours (Mareschal et al. 1999).

Another possible source of constraints is the fact that infants and children grow into adults. Perseverative errors are very common in human adult suffering from an array of neurological disorders (Norman 1981). Although it is true that the causes of errors in damaged adults may not be the same causes of similar errors made by infants, a good model should still be able to explain why an adult damaged in a specific way will show perseverative actions similar to infants. In other words, developmental models must match up with adult performance at some point. The only way for this to happen is for neuropsychological and neuroanatomical constraints to be incorporated within the developmental models. In practice, the computational models of adult perseverative behaviours that already exist (e.g., Cooper & Shallice 2000; Humphreys & Forde 1998) could be used as end-state targets for the developmental model.

In summary, I believe that providing dynamic mechanistic accounts of behaviour is the only true way to provide a scientific account of development in psychology. Thelen et al. are to be loudly applauded for providing us with such an account. However, behaviour is embodied within a specific physical body that has its own physical and computational constraints. To understand behaviour fully, we must build models that incorporate those constraints.

## Are dynamical systems the answer?

Arthur B. Markman

*Department of Psychology, University of Texas, Austin, TX 78712*
**markman@psy.utexas.edu**
**www.psy.utexas.edu/psy/faculty/markman/index.html**

**Abstract:** The proposed model is put forward as a template for the dynamical systems approach to embodied cognition. In order to extend this view to cognitive processing in general, however, two limitations must be overcome. First, it must be demonstrated that sensorimotor coordination of the type evident in the A-not-B error is typical of other aspects of cognition. Second, the explanatory utility of dynamical systems models must be clarified.

In point 3 of the introduction to the target article, Thelen et al. suggest that successfully explaining the A-not-B error using a dynamical systems approach to embodied cognition will demonstrate that "the same kind of analysis can be applied to any task at any age." The model developed by the authors is an impressive effort that brings together aspects of the developing motor, perceptual, and memory systems as well as components of the task itself. The performance of the model mirrors that observed in infants, and also makes new testable predictions. Thus, it is worth considering whether the authors have succeeded in demonstrating that dynamical systems models of embodied cognition are broadly applicable to cognitive processing. I suggest that (at least) two issues must be addressed before this conclusion can be reached (see Dietrich & Markman 2000, for a related discussion).

First, in order to use the present dynamical systems model as a prototype for the application of these techniques to other areas of cognition, it must be assumed that this task is representative of many other cognitive tasks that need to be explained. On its face, the A-not-B paradigm appears to be an excellent candidate to play this role. This task is one that was assumed by Piaget and others to reflect the infants' developing conception of objects. Thus, demonstrating the sufficiency of an explanation of the A-not-B error based on the dynamics of sensorimotor coordination is a strong point in favor of the authors' position.

As Thelen et al. point out, much research has focused on the role of perceptual and motor factors in the A-not-B error. Despite the early Piagetian position that this task reflected conceptual competence, it has long been suspected that performance in this task was related to infants' developing perceptual and motor abilities. In contrast, sensory and motor processing may not be as important to other aspects of cognition in children and adults (though see Barsalou 1999, for an argument for the central role of perception in cognition). Thus, while the present model is an important demonstration of how a variety of sources of information can be integrated to give rise to the A-not-B error, the authors have not successfully made the case that the A-not-B task is representative of cognitive processes in general.

It is particularly important to make this argument explicitly. Many of advocates of dynamical systems have described successful models of tasks that seem to be clear examples of sensorimotor coordination and have then asserted that these techniques will


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

generalize to all of cognitive processing (e.g., Glenberg 1997; Pfeifer & Scheier 1999; Thelen & Smith 1994; van Gelder & Port 1995). To make this point more convincing, it is necessary to find examples of successful models of embodied cognition that lie more squarely within the purview of more traditional cognitive models (see Markman 2000, for further discussion of this point).

One place to look for evidence of this type is in the general human tendency to repeat one's actions rather than to find novel solutions to problems. As discussed in section 2.2.5, a key aspect of the A-not-B error is that the infant is perseverating in an action that has been successful in the past. This type of behavior is evident in a variety of higher cognitive tasks. As one example, marketing researchers who have analyzed UPC scanner data from consumers' purchases find that the best predictor of what people will buy on any given trip to the store is "brand loyalty," defined as the distribution of products in that category that they have purchased in the past (e.g., Guidagni & Little 1983). That is, people tend to purchase what they have bought in the past rather than reevaluating their purchases on every trip to the store. This tendency to turn even complex tasks into simpler interactions among memory and perceptual and motor systems may provide the basis for arguing for the use of dynamical systems in cognitive models.

Even if such a basis for generalization can be found, however, there is a more vexing problem. It is not clear how dynamical systems serve as a psychological explanation as opposed to a complex description of behavior. This difficulty can be seen at the end of section 6.2.2. As the authors point out, the difference between the performance of younger and older infants is modeled by changes in the parameter $h$, which modulates the cooperativity of the sources of information in the A-not-B task. Unfortunately, the psychological meaning of this parameter is not known.

Thelen et al. do state that assuming this change in parameter values reflects only general maturational factors is unsatisfying. In the end, however, they are unable to be specific about what leads to this change in cooperativity. The authors suggest that changes in attentional mechanisms, perceptual learning, planning, motor control and memory, may all be involved in setting value for this parameter. Thus, the present model demonstrates that a combination of memory, perception, and action is able to produce the patterns of data characteristic of infants' performance in the A-not-B task. What remains to be seen is whether the dynamical systems model presented here will ultimately lead to a deeper explanation of the forces of developmental change in this task than has been obtained from other approaches.

## Navigating the complex dynamics of memory and desire: Mathematics accommodates continuous and conditional dynamics

Gin McCollum

*Neurological Sciences Institute, Oregon Health Sciences University, Portland, OR 97209.* **mccollum@ohsu.edu**

**Abstract:** The mathematical approach to such essentially biological phenomena as perseverative reaching is most welcome. To extend these results and make them more accurate, levels of analysis and neural centers should he distinguished. The navigational nature of sensorimotor control should be characterized more clearly, including the continuous dynamics of neural processes hut not limited to it. In particular, discrete conditions should be formalized mathematically as part of the biological process.

It is good to see mathematical modeling of natural sensorimotor performance, especially modeling that eliminates unnecessary conceptual trappings. Mathematics is the right tool for shaping concepts to accurately characterize complex, essentially biological phenomena. For example, although excitation and inhibition are analogues of the familiar positive and negative, the essentially biological phenomenon is cooperativity, as Thelen et al. have formalized it.

Mathematical theory need not be numerical to be rigorous or (McCollum 2000) predictive. The essential mathematical features here are peaks and cooperativity, not the particular functions. In a mathematically impoverished environment, sometimes it is desirable to use simulations to communicate mathematical results, even though they demonstrate very little, if anything. However, this critique will address the mathematics itself. If theorists aspire to a theoretical nouroscience worthy of theoretical science of the twentieth century, we must critique each other at – or at least closer to – that standard.

***Complexity.*** To extend those results and make them more accurate, the levels of analysis and neural centers (Fig. 1) need to be separated in the model. The present results refer to both the arm movements and the distributed activity of a neural population. Even if the two are similar in one-peaked or two-peaked form, they are distinct phenomena: an arm reach is not neural activity. By recognizing the separate levels of analysis, the mathematics would address the nervous system as a complex system (Auyang 1998).

In Thelen et al's analysis, the complexity is mimicked by introducing a threshold and noise (sect. 4.1.3). Without these, the conditionality of neural signals impinging from various centers would have to be addressed. Continuous and graded activity in one neuronal population does not exclude discrete conditionality from the whole organism. Separating levels of analysis and neural centers adds mathematical structure not represented in the present equations (McCollum & Boyle, submitted; Roberts & McCollum 1996b) At the same time, it simplifies the task of modeling individual neural centers (Roberts 2000) Once the levels of analysis and neural centers are separated, the authors' result that the memory of previous reaches is simply summed with the desire for the toy will be clearer and more salient.

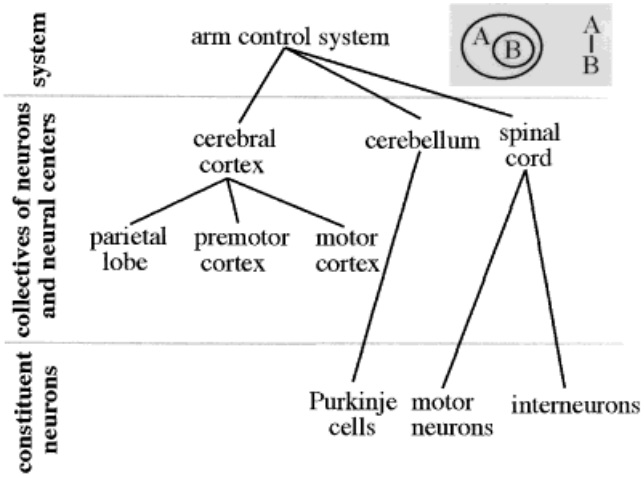



Figure 1 (McCollum). Levels of analysis in the arm control system. The arm control system includes several neural centers, which are collectives of neurons, some of which are shown here. Inclusion can be portrayed in a Venn diagram, as in the gray inset box at the upper-right. Equivalently, a solid line is conventionally used in the mathematics of ordered structures to denote inclusion, as is also shown in the gray inset box. The main diagram is too complicated to show using Venn diagrams, so it uses lines to denote inclusion. For example, on the right, "spinal cord" is shown including both motor neurons and interneurons. The bold sideways labels to the left point out distinctions between levels of analysis. The neurons are the smallest individuals depicted, so they are considered to be constituents. They form collectives, as cell aggregates, neural centers, and coordinated neural centers. The entire arm control system includes all the collectives, and through them, the constituent neurons.



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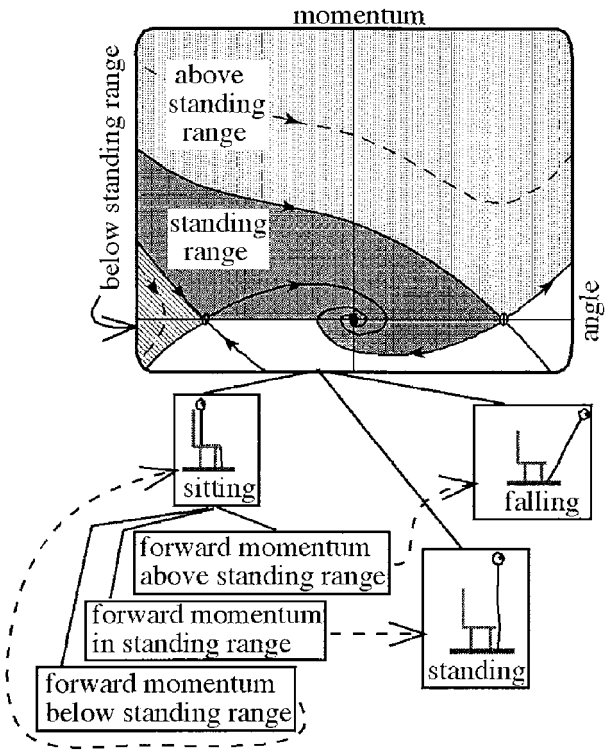



Figure 2 (McCollum). Dynamics of the sit-to-stand movement, embedded in a conditional sensorimotor space. At the top of the diagram is a dynamical space of two links with an attractor in the center, at the standing position. Forward momentum is a continuous variable that is naturally divided into three distinct segments by the destinations of trajectories. Within the shaded region, all trajectories arrive at the attractor, the standing position. At higher forward momenta (vertical stripes), trajectories overshoot the standing position and lead to a forward fall. At lower momenta (diagonal stripes), trajectories return to the seated position. The amplitude of forward momentum is a condition on the trajectory, dividing a continuous range into discrete regions by physics. Like other types of conditions, these can be specified as a conditional space. The dynamical space includes sitting and standing regions, plus regions of falling trajectories. These regions are denoted by boxes with labels and stick figures. The three ranges of momentum with which one may leave the sitting position are conditions further specifying sitting, as indicated in boxes. Contiguity, denoted by dashed arrows, displays the results of the trajectories in the three momentum ranges. Like the dynamical space, the conditional space is a mathematical expression, like an equation. Inclusion and contiguity can be used to express complex organizational structure.

***Navigation.*** Continuous neural dynamics is navigated, as a bird navigates wind currents, using small adjustments to get a large result. This is why adults gauge the growth of desire against better judgment, for example in deciding whether to have another drink or to look at the dessert tray in a restaurant. This is the internal analogue of navigating the biodynamics of body movements. For example, the sit-to-stand movement is governed by physical dynamics that allow the body to fall back to sitting, to arrive at standing, or to fall forward beyond standing (Fig. 2) (Roberts & McCollum 1996a).

The mathematics do not need to he specified numerically for each individual; the topological dynamics is the same for all and is the most relevant. Similarly, the numerical description of the dynamics of reaching will not fit each baby.

Experience allows an individual to produce the correct amount of momentum to ride physical dynamics to a standing position, as a bird rides a wind current. Similarly, each baby – seemingly naive in the experiments – must learn to navigate its own dynamics of memory and desire.

***Conditionality.*** Navigation of neural dynamics is both continuous and conditional. Several discrete conditions are mentioned in the article, but not recognized as such, including: whether the eyes are closed, whether the midline is crossed, number of trials, whether the baby is standing or sitting (sect. 3.2), and change of toy (sect. 5). Similarly, the Ghez group result (Honing at al. 1988a: 1988b) (3.1.2) depend on both continuous development of motor intention and conditionality of signals impinging discretely on it. This is a temporal analogue of the choice of momentum in performing the sit-to-stand movement.

Continuous and discrete are both true and both belong in the mathematical characterization. Algebraic relations formalize the discrete conditions; they do not need to be computed in an ad hoc fashion, but can be formalized mathematically so that the organization of the control system can be understood (McCollum 1994; 1999a; 1999b; McCollum & Boyle 2000; McCollum et al. 1996; Roberts & McCollum 1996b)

The seamless, rapid, on- and off-line switches that babies learn

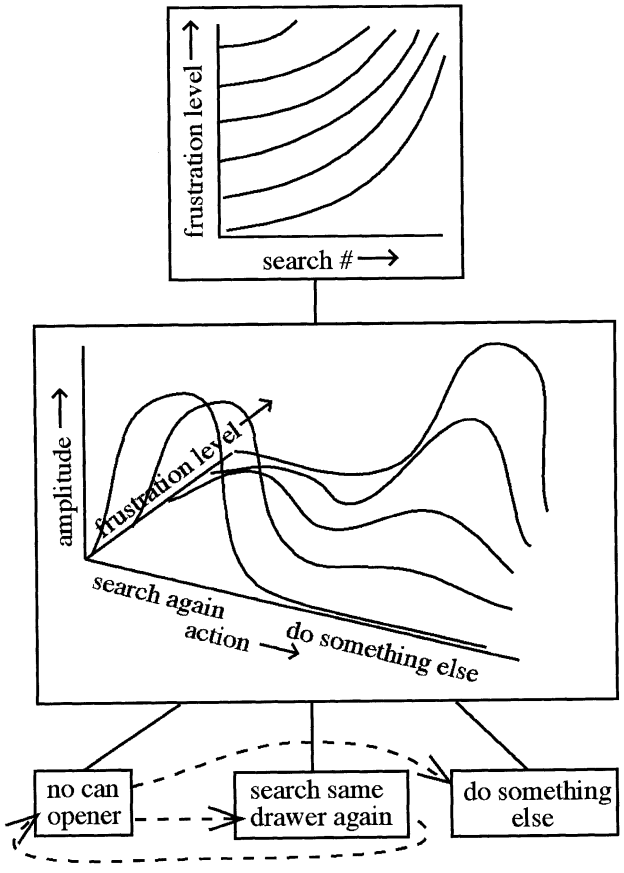



Figure 3 (McCollum). Conditional dynamical space of kitchen reorganization. Mathematical approximation of kitchen reorganization (sect. 2.2.6), including the dynamics of frustration increase with search number and of amplitude ("punch"; sect. 2.2.2) shift from searching again to doing something else (Kauffman 1993; Roberts 2000). Motor dynamics are left implicit, their place indicated verbally. "Governs" is represented by the algebraic relation of inclusion, denoted by a solid line connecting the governing (higher on the page) with the governed. "Leads to" is represented by the algebraic relation of contiguity, denoted by a dashed arrow. The diagram is to be read as a mathematical expression, like an equation (McCollum 1994; McCollum et al. 1995; Roberts & McCollum 1996b).


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(7.1) require a control system based on discrete conditions, as well as continuous dynamics. For adults, the experience of suddenly remembering the reorganization of the kitchen is clearly a discrete jump (Fig. 3). Mathematics is not averse to including discrete conditionality arid separating levels of analysis and neural centers in the formalism; let us use the mathematics to represent the behaviors and the body more accurately.

## Is a field theory of perseverative reaching compatible with a Piagetian view?

Lorraine McCune

*Graduate School of Education, Rutgers University, New Brunswick, NJ 08901.* **mccune@rci.rutgers.edu**

**Abstract:** This commentary is a brief reflection on the relationship between the embodied cognition analysis and a Piagetian theoretical position. In particular, the place of A-not-B in the larger Piagetian framework and the importance of the concept of mental representation, in contrast with perceptual understanding, are noted.

The A-not-B error, first demonstrated in Piaget's (1952) investigation of infants' search behavior, provides a vehicle for Thelen et al. to, in their words, "demonstrate a mechanism for an embodied cognition" (abstract). Thelen et al. provide the most comprehensive operationalization of this task published to date. Their goal, in simplest terms, is to determine underlying parameters influencing infants' reach decision when faced with the confusing two-choice situation. To achieve this goal it became necessary to examine the original task in its details, and review virtually all research engaging either human infants or nonhuman primates in this task. The result is a comprehensive dynamic model including all identifiably measurable parameters that can be shown to influence the direction of a child's reach in this task.

Although the authors deny that "the error is one of object representation in the accepted Piagetian sense" (sect. 6.1.2, para. 3), they note that "One assumption of the infant model is that there are changes in the ability of the movement planning field to sustain an activation peak in the absence of specific input." the analysis presented in the target article goes well beyond anything Piaget had envisioned, but sustaining activation in the absence of specific input seems rather like acting on the basis of internal mental representation as opposed to perceptually available data.

The field of infancy research has continued to draw on tasks originally investigated by Piaget. It seems useful to consider the compatibility of such modern findings with the overall "grand design" presented in Piaget's work. First, it should be noted that Piaget's theory has never been static, and that over the long years of his professional life he offered revisions and reinterpretations based on his own and others' findings. Despite the fact that he was a firm believer in logical structure as an organizational framework for behavior, he was a dedicated empiricist, continually responsive to data. Furthermore scholars using the Piagetian framework may themselves differ usefully in the interpretation of aspects of the theory. I consider the dynamic systems approach with continuous variables seamlessly yielding discontinuity completely compatible with Piagetian perspective. "Stage" is a purely theoretical construct, and Piaget repeatedly notes that all levels of intervening behaviors will be found.

The major stages Piaget identified were three. Sensorimotor, where perception, action, and thought are unified; concrete operations, where the child begins to apply recognizable adult logic, with internal logical operations holding sway in application to concrete problems, despite any contradictory perceptual evidence, and formal operations where purely verbal and abstract problems can also be solved.

The critical transition of the sensorimotor period is not the solution of A-not-B (considered diagnostic of substage 4), but the achievement of a mental representational capacity that can be invoked in contrast with present perceived reality (see also Sartre 1948). Piaget's description of children's gradual achievement of various sensorimotor tasks all served to chart their trajectory to independent representational thinking. Therefore, although six theoretical substages and five domains of functioning were identified in his infancy books, the major transition came only at stage 6 when, either all at once, or perhaps domain by domain, the child demonstrated representational thinking in contrast with perceptual responding.

Is there a valid distinction between perceptual experience and mental representation which demonstrates a strong developmental transition? None of us would deny a distinction between the experience of looking at a New York Times, present before us, and mentally picturing the San Francisco Chronicle banner from several years ago. The former experience is perceptual, the latter representational.

In Piaget's view, learning and development contributed to the mental capacity for representational consciousness, in contrast with perceptual consciousness available from birth. Among all of the Piagetian tasks, placing a toy in a container, then releasing it under a screen, out of the infant's direct view (sometimes termed "invisible hiding," a task criterial for sensorimotor stage 6) has been the most robust measure of the transition to representation. Ramsey and Campos (1978) demonstrated that at stage 6, but not at stage 5, 11-month-olds showed surprise and persistent search when a given toy was hidden as they watched, but their subsequent search yielded a different toy. This research demonstrated a shift in the ability to keep in mind the perceptual characteristics of an object in its absence and recognize the discrepancy between remembered and perceived objects when there is a toy switch. Representational play also first occurs in conjunction with the stage 6 invisible hiding transition, and pre-stage 6 infants do not develop language (McCune 1995; McCune-Nicolich 1981).

In the Thelen et al. model for understanding A-not-B they call upon the "ordinary processes of goal-directed action: looking, planning, reaching, and remembering." I suggest that planning and remembering are both affected by the extent to which a child has developed the capacity for mental representation in contrast with a limitation to perceptual processes. As the authors note, at any age perseveration can be induced by additionally complicating a task. It nonetheless remains useful to maintain consideration of such underlying developmental changes as the development of mental representation. We can't know what Piaget's reaction would be to the Thelen et al. resolution of A-not-B. Modern research methods afford far greater opportunity for exploring and integrating various behavioral and internal components as contributors to behavior than were available at the time of his initial interpretations. Those of us working in the present decades have the advantage of appreciating these new levels of clarity, and at the same time continuing integration with broader theoretical constructs.

The type of analysis and modeling demonstrated in the target article is an extremely valuable enterprise. As the authors note, this approach is now available to additional multiply-determined measures of infant behavior which often show subtle variation in their application contributing unknown variability to outcomes across studies. I agree with their view that the next critical application for a dynamic analysis is the area of infant looking, a measure with widely varying interpretation, which dominates investigation of the development of pre-reaching infants.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

## Infants reach to location A without practice or training

Laraine McDonough

*Department of Psychology, Brooklyn College and The City of New York Graduate Center, Brooklyn, NY 11210.* **Larainem@brooklyn.cuny.edu**

**Abstract:** Thelen and her colleagues' model overemphasizes the role of action in cognitive development. Recent research has shown that infants do not have to be trained to reach for a hidden object. By 7.5 months of age, infants can recall the location of a hidden object with no practice trials. Thelen at al.'s goal to design a parsimonious account of A-not-B behaviors was successful, but at the expense of focusing primarily on implicit and ignoring explicit memory.

Thelen and her colleagues' goal was to design a model of a task that would banish a dual-process distinction between thinking and acting because they believe that discontinuities between what children know and what they can demonstrate are "untenable." They further argue that there is "no time and no task" in which reasoning can be separated from perceiving, acting, and remembering. To accomplish their goal, they designed a model of the A-not-B task, a task they admit is ambiguous and has little to do with an object concept.

Although I agree with some of their premises, I strongly disagree with others. I agree that the A-not-B task as it is traditionally administered is ambiguous and has little to do with an object concept. Smith and her colleagues clearly demonstrated this point. I also agree that perceiving and remembering are critical aspects to knowing. But my main disagreement is with their overemphasis on the role of action in cognitive development.

Most researchers who study dual-process models of memory (e.g., Diamond 1990b; Mandler 1990; Squire 1987) do not make such a gross distinction as between thinking and acting. Instead, they distinguish implicit and explicit memory; with the former being the result of memory without awareness of what was being learned (but demonstrating learning nevertheless) and the latter being knowledge accessible to awareness. Actions can involve implicit and/or explicit memory. Deferred imitation tasks, in which the dependent measure is untrained and unrehearsed actions, are one way to assess explicit memory. This kind of task has been validated by testing amnesic patients who are unable to complete the task or engage in explicit recall (McDonough et al. 1995). In the case of deferred imitation tasks, knowing (explicit memory) guides actions. Actions that have been conditioned through repeated trials in the context of ambiguous tasks are generally thought to be the product of implicit learning.

As a research community that has spent considerable time and energy examining A-not-B behaviors as a part of Piaget's theory of an object concept (Stage IV), we have tended to ignore one logical part of the sequence that bridges Stage III and Stage IV behaviors: the ability to reach for an object that has been hidden in a single location. Object hiding tasks do not have to involve extensive training. My research shows that infants as young as 7.5 months of age are able to recall the location of a hidden object after 90 second delays with no training trials (McDonough 1999). Reaching was not allowed before test and eye gaze was directed to the empty location before both locations were simultaneously covered. At the end of the delay, infants' attention was directed to both locations. After they looked at both locations (a requirement of the task), the apparatus was then pushed within their reach. No participant was eliminated for not reaching. The results showed that infants older than 7.5 months of age reach correctly to the hidden object after delays averaging around 90 seconds. Infants younger than 7.5 months perform at chance expectation on the task, an age effect that replicates Baillargeon and Graber (1988) who used a looking task to assess memory for the location of a hidden object after 70 second delays. Thus, a reaching task can show the same developmental effects as a looking task. Thelen et al. propose that development is found in the "particulars of the environment and timing demands." But it is not the case that the environment develops. What develops is cognition. The point to be made is that looking and reaching tasks can tap into the same cognitive processes, a point Thelen and her colleagues would probably agree with. They suggest in their discussion that eye movements can also be considered actions (although the dynamics are yet to be worked out).

Although it is possible to figure out why an infant or an adult behaves in particular ways by using conditioned behaviors, we are still left without knowing what is understood. After all, actions are not always planfully or thoughtfully carried out and all thoughts are (thankfully) not acted on. Thelen et al. discuss this issue in terms of loose- or tight-clamping to environmental constraints, but we should not ignore the kind of cognitive processes (explicit or implicit) involved in behaviors and how readily available they may be to developing infants. Thelen et al. accomplished their task by proposing what they call a more parsimonious model of development, but they did so by primarily focusing on implicit and ignoring explicit processes.

## What is embodied: "A-not-B error" or delayed-response learning?

George F. Michel

*Psychology Department, DePaul University Chicago, IL 60614–3504.*
**gmichel@condor.depaul.edu** **www.depaul.edu/~gmichel**

**Abstract:** The procedures used to ensure reliable occurrences of the A-not-B error distort and miss essential features of Piaget's original observations. A model that meshes a mental event, highly restricted by testing procedures, to the dynamics of bodily movement is of limited value. To embody more than just perseverative reaching, the formal model must incorporate Piaget's essential features.

All cognitive functions (e.g., perceiving, remembering, thinking and planning) may be subordinate to and derivative from the neural organization and control of movement (Sperry 1965). Thus, mental phenomena may be embodied in the neural dynamics of action. Thelen et al. have provided a formal model of a specific phenomenon (the A-not-B error) believed to reflect a mental event (a stage in the development of the object concept) that nicely integrates it with the domain of movement control. By critically reviewing the studies of this error, they were able to demonstrate how dependent the error is on the typical processes that influence the control of movement. Using that information, their model accounts for the apparent inconsistencies in the reported research and predicts new observations, some of which have been confirmed by subsequent investigation. Although I support the goal to embody mind, I am concerned that those procedures that ensure the occurrence of the A-not-B error miss or distort important aspects of the phenomenon.

In the 1960s and 1970s, those who tried live demonstrations of the Piagetian sensorimomor stages had difficulty finding infants who showed the A-not-B error. However, when the error occurred, it was striking. These demonstrations closely matched the procedure described by Piaget (cf., Gruber & Voneche 1977, pp. 259–60). An attractive toy would be taken from the infant and covered with the "A" cloth of the two cloths in front of the infant. If the infant retrieved the toy, it would be taken again and placed under the "A" cloth. On the third trial, the toy was placed under the second "B" cloth. The error occurred when the infant searched under the A cloth and not the B cloth. Once the error occurred, the infant's memory for sequential events would be assessed. A toy was placed under one cloth and after the infant retrieved it, the toy was placed under the same cloth; but before the infant could uncover it, it was removed and immediately placed under the second cloth. Again, the error occurred when the infant


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

removed the first cloth and did not search under the second cloth. Piaget described these errors as "the active search for a vanished object but without taking account of the sequence of visible displacements" (Piaget 1954, in Gruber & Voneche 1977, p. 257). The A-not-B error was a convincing but elusive occurrence in development until several procedures were created that ensured that a good proportion of infants between 9- and 11-months-old would perform the error. However, these procedures, so well described in Thelen et al., transformed the task into a delayed-response spatial learning task with the toy as the reward. Reexamination of Piaget's original account (Piaget 1954, in Gruber & Voneche 1977, pp. 250–72) highlights the significance of this transformation.

Piaget argued that the infant conceives reality differently from the adult. His "experiments" were designed to identify these differences and to demonstrate how certain categories of mind (e.g., the object concept) are neither innate nor given ready-made in experience. Rather, these categories are constructed step-by-step from the infant's actions. Actions define an object. Initia11y, the object concept does not include existence beyond the immediate engagement of infant's actions. The infant will behave as though the object ceases to exist when it disappears from view. Eventually, the infant actively searches for an object that had disappeared no matter what the circumstances of the disappearance. Between these two extremes, Piaget observed that when the object disappears in two or more distinct places, the infant does not seem to track, conceptually, the sequence of the disappearances. Thus, the A-not-B error makes its appearaince. Piaget reported that his daughter Jacqueline continued to search manually for a toy that she previously had found hidden under his hand when he placed the toy in plain sight but in a different place and even when she looked at the visible toy. To examine this error, Piaget created the prototypic task described above. Performance on this task together with several anecdotes about his daughter Lucienne defined a "stage four" object concept. For example, in response to the question Where's papa? (posed while in the garden), Lucienne turned away from her father standing in front of her and visually searched the window where she had frequently seen her father whenever she and her mother were in the garden (p. 260).

Piaget offered three interpretations of these errors:

1. A defect of memory – the infant, as may the adult, forgot the sequential displacements of the object. However, why did Jacqueline seek a toy where she found it last when it was in plain view and she looked at it several times?

2. A defect of spatial localization – the infant's spatial search strategy is first to search where the object is seen, or search where it was last seen, or search where the object was last found. Again, this interpretation fails to account for erroneous search when the object is in plain sight but in an unfamiliar location.

3. A defect of objectification – the object is only a salient aspect of the total context in which it is contained, including the infant's actions and not a substantial, individualized phenomenon. There are "papa-at-the-window-when-in-the-garden" and "papa-in-front-of-oneself" or "toy-under papa's hand and "toy on the table."

Piaget argued that the three interpretations were complementary because "the object is not a thing which is displaced and is independent of those displacements; it is a reality at [the infant's] disposal in a certain context, itself related to a certain action [of the infant]" (p. 264).

The end of this stage occurs when the child does not return to search a previously successful position A when the object has disappeared at position B, even when the object cannot be found at B. The "toy-in-plain-view" the "toy found-at-position-A," and the "toy-hidden-at-position-B" have become the "same" toy. This compares well with Thelen et al. "there is only 'knowledge' of objects embedded in the immediate circumstances and the history of perceiving and acting in similar circumstances." Missing from Piaget's account are multiple reinforcements of successful retrieval from position A. He permitted only two such successes so as to avoid habit formation. Also missing is any notion of a delay between hiding and retrieval. Indeed, Piaget stated that "at the moment when the [toy] disappears in B [Jacqueline] turns to her left side and looks where it was before, in A" (p. 260). So, will the simulation show a search at A after only two successes and no delay? Will it "search" at A when the desired toy is in plain view but in a different location? Will it "follow" a sequence of disappearances? Will it stop searching when the toy is not found at B or will it return to A? Too much of what defined the infant's performance as the development of the sensorimotor embodiment of a "concept" is missing from just those operational procedures needed to ensure that a reliable proportion of infants will make the error at the time of testing. Concepts may only be movement dynamics but the complexity of such dynamics should not be hidden by the constraint of the testing procedure.

## Objectivity, intentionality, and levels of explanation

Ulrich Müller[a] and Jeremy I. M. Carpendale[b]

[a]*Department of Psychology, University of Toronto, Toronto, Ontario, Canada M5S 3G3;* [b]*Department of Psychology, Simon Fraser University, Burnaby, BC Canada V5A 1S6.* **mueller@psych.utoronto.ca** **jcarpend@sfu.ca**

**Abstract:** Notwithstanding many similarities between Thelen et al.'s and Piaget's accounts of the A-not-B error, we argue that, in contrast to Piaget, they do not explicitly address the issue of objectivity. We suggest that this omission is partly due to the fact that Thelen et al. and Piaget's accounts are pitched at different levels of explanation.

The article by Thelen et al. contains many valuable suggestions for understanding the A-not-B error in particular and for conceptualizing development in general. In many respects Thelen et al.'s account is quite similar to Piaget's account, although we also point out critical differences concerning objectivity. First, both accounts emphasize the practical and embodied nature of cognition (Piaget 1954, p. 359). Second, both accounts acknowledge that action and perception are intrinsically connected (Piaget 1954, p. 93; 1962, pp. 79–80). Third, Thelen et al. as well as Piaget argue that development is continuous in the sense that it is the natural outcome of the infant's interaction with the world and that no special force or mechanism has to be introduced (Piaget 1963, pp. 3–8, 352, 407–19). Fourth, both accounts highlight the fact that behavior reflects the infant's history (this follows from Piaget's notion of assimilation). Fifth, both Thelen et al. and Piaget invoke the prevalence of the practical memory of previous reaches over memory of the sequence of displacements to explain the A-not-B-error (Piaget 1954, p. 339). Sixth, both Thelen et al. and Piaget invoke similar actions (e.g., object exploration, perceptual activities, locomotion; Piaget 1954, pp. 152–83) to account for overcoming the A-not-B-error. Finally, both accounts point out that the specific context affects performance on A-not-B-tasks (Piaget 1954, pp. 58–59).

In light of these similarities, it is surprising that Thelen et al. regard Piaget as belonging to the "cognitivist stance." Thelen et al.'s view appears to be due to their functional interpretation of Piaget's notion of object concept as a "causal structure that generates a thought or a behavior." However, Piaget used the notion of structure not in a causal-functional way but as a tool for describing the formal or morphological properties of a certain type of action. Furthermore, as Chapman (1987, p. 291) has pointed out, Piaget "maintained that his formally defined operatory structures reflect functional realities insofar as they are embodied in the activity of living organisms."

Beyond this (mis) interpretation of Piaget's notion of structure, however, a real difference between the accounts lies in Thelen et al.'s treatment of the construction of objectivity. For Piaget the



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

outcome of sensorimotor development consists in the construction of a practical, objective universe that is "a universe both stable and external, relatively distinct from the internal world and one in which the subject places himself as one particular term among all the other terms" (Piaget 1954, p. 4). Such a universe must include the notion of permanent objects, and the development of object permanence, in turn, is closely intertwined with the construction of space, causality, and time (Piaget 1954).

At the beginning of development, the infant understands objects, space, causality, and time only in relation to her immediate actions and not as partly independent of her actions. The developmental transition from a subjective to an objective grasp of the world requires that the infant gradually detach her action from the object on which it bears (Piaget 1954, pp. 92, 350–62). According to Piaget, the motor of this detachment is neither the emergence of representation nor the simple accumulation of experience but the reciprocal coordination of action schemes into reversible groups (Piaget 1954, pp. 98, 209–10). The A-not-B error reflects deficits in the construction of an objective universe; the fact that the infant searches at a location where she has been successful before indicates that the object still retains a special position and has not been dissociated from the infant's immediate action.

In contrast to Piaget, Thelen et al. claim that "the A-not-B error is not about an object concept per se." However, they also fail to explicitly address the issue of objectivity. This omission is manifest in three aspects of their account. First, Thelen et al. draws analogies between motor planning tasks in adults and the A-not-B error in infants. In fact, Piaget noted that even adults sometimes commit the A-not-B error (Piaget 1954, p. 60). However, although the outcomes are similar, the reasons are not; whereas the adult commits the error in a moment of absent-mindedness, the infant commits the error because she has not yet constructed a universe independent of herself.

Second, Thelen et al. exp1anation of the mastery of the A-not-B task (ability of the movement planning field to sustain an activation peak in the absence of specific input) does not involve any reference to the construction of objectivity. By contrast, for Piaget one key developmental change responsible for the mastery of the A-not-B task consists in the construction of objective temporal relations between events (Piaget 1954, pp. 337–41).

Third, Thelen et al.'s definition of knowing "perceiving, moving, and remembering as they evolve over time" leaves out any reference to objectivity. Arguably, knowing, as well as perceiving, acting, and remembering, are intentiona1 activities, that is, they are about something and intrinsically directed toward things (Brentano 1973). Because Thelen et al. fail to incorporate intentionality into their definition of knowing, it is unclear whom they consider to be the agent of actions and development. At times, it seems they consider the movement planning field and the activation field as agents. By contrast, Piaget's (1971) concept of assimilation denotes intentionality, thereby incorporating the notions of agency and object-directedness.

So far, our discussion has raised questions about the notions of objectivity, intentionality, and agency in Thelen et al.'s account. We suggest that the reason they fail to address these notions is that their account is pitched at a neurophysiological, subpersonal level of explanation (Russell 1996; 1999). By contrast, Piaget (1954, p. xii) analyzed development from the point of view of the infant's consciousness, and his account is thus pitched at the personal, psychological level of explanation. Indeed, at the neurophysiological level, the notion of objectivity is meaningless because neurophysiological functioning adheres to the mechanical laws of cause and effect and leaves no room for objectivity (Straus 1963, pp. 158–86) . Objectivity can only arise for an organism which is intrinsically directed toward objects.

To the extent that Thelen et al.'s account is located at the subpersonal level, it is in principle comparible with a Piagetian-type personal-level account. Because we believe that the infant's actions are meaningfully related toward objects and are not reducible to the behavior of billiard balls pushed by a billiard player, we think that a subpersonal-level explanation cannot be the whole developmental story (Straus 1963, pp. 59–61, 123–25).

## An embodied theory in search of a body: Challenges for a dynamic systems model of infant perseveration

Yoke Munakata,[a] Sarah Devi Sahni,[b] and Benjamin E. Yerys[a]

[a]*Department of Psychology, University of Denver, Denver, CO 80208;* [b]*Department of Psychology, Carnegie Mellon University, Pittsburgh, PA 15213.* **munakata@du.edu** **kore.psy.du.edu/munakata** **ssahni@andrew.cmu.edu** **byerys@nova.psy.du.edu**

**Abstract:** In this commentary, we question (1) how embodied Thelen et al.'s model is relative to their aims, and (2) how embodied the behavior of children is in particular response systems, relative to how much dynamic systems theory emphasizes this idea. We close with corrections to mischaracterizations of an alternative, neural network perspective on infant behavior.

The dynamic systems approach stresses the importance of embodied cognition, which arises through perceptual and motor systems in a physical body that interacts with the world. Although Thelen et al. concede that their model is but a first step toward simulating such embodied cognition (e.g., their model fails to capture the kinematics of movement), the limits to the model's embodiment may run deeper than this. Specifically, behavior, development, memory, and environmental input are all simulated in forms of disembodied parameters, without a clear link to underlying mechanisms. For example, Thelen et al. list a number of developments that might contribute to improved performance on the A-not-B task, including self-locomotion and improvements in reaching. Given their emphasis on embodiment, one might expect their dynamics systems model to provide an understanding of how such developments could arise within an embodied system, to ultimately reduce perseveration. Instead, development is simulated in terms of changes to a parameter, $h$, the resting level of a dynamic field, without explanation of how such changes map onto the hypothesized behavioral changes (e.g., self-locomotion) or body mechanisms. Similarly, the salience of a stimulus is manipulated in terms of another parameter without discussion of how the embodied system instantiates this salience. It is not clear how such parameter changes provide an embodied understanding of A-not-B performance.

The lack of embodiment is also evident in the summing of inputs to the dynamic field, whereby memory is treated as an input just like an environmental stimulus – as a numerical abstraction. Although Thelen et al. view such abstractions as a strength of the dynamic systems framework, allowing for a common language across levels of analysis, such abstractions may miss critical differences in the embodiment of different factors. For example, certain forms of memory may be embodied in synaptic changes whereas responses to particular environmental stimuli may be embodied in the firing of populations of neurons. Because of these different embodiments, memory and environmental inputs can have different consequences for behavior, and they can interact in nonlinear ways with other factors. Such effects are not captured in a disembodied summing of their contributions.

We do not mean to claim that modeling at this higher, abstract level is not useful. Instead, we argue that it does not meet the strong claims of embodiment of the dynamic systems framework, and could benefit from complementary models that are more embodied. Neural network models can be viewed as one particular embodiment of dynamic systems ideas. The neural network framework captures core principles of dynamic systems (e.g., nonlinearities in development, multiple components interacting to produce behavior), while also providing an embodied framework



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

in which network parameters and properties can be mapped onto neural ones.

The task remains to specify the appropriate level of embodiment required to understand perseverative behaviors. Although Thelen et al. stress the importance of the embodied reaching process, the behavior of children may not be so clearly tied to the details of particular response systems. In the A-not-B task for example, after infants merely observe a toy being hidden on A trials, without reaching, they still reach incorrectly back to A on the B trials (e.g., Diamond 1983). In other contexts, older children perseverate similarly across different response modalities that vary greatly in their detailed embodiment (e.g., reaching, verbal response, error detection) (Jacques et al. 1999; Zelazo et al. 1996). Such results suggest the potential role of shared representations underlying perseveration that multiple response systems draw upon, and of more general principles for understanding perseveration that apply across different response modalities. Thus, although we share the view that it is important to specify underlying mechanisms, the data suggest the appropriate level of embodiment may not be in the details of particular response systems.

Finally, Thelen et al. claim that in my (YM's) neural network framework:

1. Performance demands such as manual actions are viewed as hiding infants' true competence, so that the A-not-B task is the wrong behavioral task for measuring knowledge, and
2. simulating the A-not-B task required a departure from earlier models.

Both claims are false. My theoretical, empirical, and simulation work have consistently converged on the idea that both successes and failures provide important constraints on theorizing about cognitive development, and that performance demands cannot fully explain away the failures (Munakata 1998; Munakata et al. 1997; Munakata & Stedron, in press). Further, my A-not-B simulations are a direct extension of my earlier modeling work; critical to both are the strength of object representations (whether the object is a hidden toy or a visible lid), and changes to connection weights as a result of experience. The primary difference between the models is in the nature of the tasks simulated; the A-not-B task leads to a competition between connection weights (biased toward A after pretrials and A trials) and object representations (for the most recent B location). This competition is not present in tasks employing a single hiding location.

Several of the dynamic systems model's predictions and explanations of perseveration (e.g., a U-shaped developmental curve, the processes underlying the effects of distinct lids) match those from my neural network model. Distinguishing dynamic systems and neural network predictions and explanations may be a productive step in exploring infant perseveration, as well as in evaluating theoretical frameworks for development more generally.

## A spatial coding analysis of the A-not-B error: What IS "Location at A"?

Nora S. Newcombe

*Department of Psychology, Temple University, Philadelphia, PA 19122–6085.*
**newcombe@astro.temple.edu**

**Abstract:** Thelen et al. criticize "spatial coding" approaches to the A-not-B error. However, newer thinking about spatial coding provides more precise analytic categories and recognizes that different spatial coding systems normally coexist. Theorizing about spatial coding is largely compatible with dynamic-systems theory, augmenting it with an analysis of what one means when discussing "location at A" (or B).

We all see the world through the lenses of our personal interests. The authors of the target article have distinguished histories of research in motor development and dynamic systems theory, and they have viewed the A-not-B error through that lens. My own research program has focused on spatial development, and so, naturally, I have thought about the A-not-B error in spatial terms. In this commentary, I present a brief account of the dynamics of the error in terms of spatial coding (for a fuller treatment, see Newcombe & Huttenlocher 2000, Ch. 3). I then consider to what extent the dynamic-systems and the spatial-coding stories are truly different. Although they sound different, I believe they may be mutually translatable and hence mutually informative.

***Varieties of spatial coding.*** Traditionally, analysis of the A-not-B error in spatial terms focused on the behavior as a marker of a qualitative transition in spatial coding. This transition was characterized as being a change from an egocentric coding system centered on body coordinates and/or characterized by defining location in terms of sensorimotor action, to an allocentric coding system using external frameworks (Acredolo 1985; Bremner 1985; Piaget 1952). Thelen et al. rightly point out that accounting for the A-not-B error in terms of this qualitative transition cannot explain the effect of delay on the error, nor the looking-reaching decalage.

It has become clear, however, that thinking about spatial coding simply in terms of egocentric versus allocentric coding is incomplete (Gallistel 1990; Pick & Rieser 1982; Sholl 1995). First, while egocentric coding is limited in usefulness to situations in which one is stationary, most mobile organisms have available another body-centered system of reference that is not egocentric. In this system, called dead reckoning or inertial navigation, movement is automatically used to update location information. Second, allocentric coding is not a unitary entity either. It can either involve simple use of coincident cues to mark location, or flexible and powerful use of a framework of distal external landmarks.

In the A-not-B paradigm, one of these four kinds of coding, inertial navigation, is irrelevant, because the child does not move. (This fact raises the interesting question of why locomotor ability should be linked to success in the task.) Egocentric coding is certainly relevant to the task, but note that it comes in several varieties. First, such coding can be in terms of either looking or reaching. Second, babies could base their behavior on either the most frequent look (or reach) or on the most recent look (or reach). However, of these four seemingly sensible possibilities, only one, the most recent look, leads to correct localization of the object on the B trial.

In terms of external coding, coincident cues are useful if they are available. (Thus, from the spatial point of view, it is no mystery at all that distinctive covers aid performance.) When the covers are identical and simple external cues do not suffice, success in the A-not-B situation could be based on coding the location of the well in terms of external landmarks, which would include room cues and also the frame of the table or apparatus surrounding the wells (see Newcombe et al. 2000).

Viewed in this way, the findings from the A-not-B paradigm index infants' evolving ability to rely on the correct cues (most recent look and/or relation to external framework) from among a mix also including plausible but incorrect cues (most recent and most frequent reach, most frequent look). Such evolving reliance is not likely a matter of an all-or-none switch from one mode of spatial coding to another, but rather depends on the relative strength of these competing cues, both initially (when the object is hidden) and at the point in time when an action is allowed (when the strength of some of them may have waned more than others). From this perspective, it seems natural to find an influence of variables such as the motivational value of the object hidden (which may strengthen the influence of the most recent look) or of the length of delay (which may differentially affect the relative strengths of the various cues) One can also explain why looking behavior is developmentally advanced over reaching (Hofstadter & Reznick 1996), because the most recent look may be more tightly coupled to subsequent looking behavior than it is to subsequent motor action. The linkage of locomotion to success, even though there is no movement in the task itself, also is explicable, because locomotion likely leads to revision in infants' reliance on the different forms of spatial coding.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

***Comparison to the dynamic systems account.*** This analysis differs from the dynamic-systems account in ways that may be considered complementary. Most notably, it unpacks the notions of "location at A" and "location at B" into the various coding possibilities that might lead to reaching to one or the other location. That is, the two distributions shown waxing, waning, and competing in the figures in the target article can each be regarded as the sum of various component distributions. Such unpacking may well lead to more fine-tuned empirical predictions.

There may also be points of contention. First, the analysis preserves the notions of a "looking system" that, while interacting with a "reaching system" in normal mature individuals, has some independent dynamics of its own (cf. Milner & Goodale 1995) Second, Thelen and Smith (1994) reject postulating entities such as "knowledge" or "competence," and Thelen et al. rightly note that older children and even adults may make A-not-B errors in certain circumstances. But some circumstances are simply much more common than others. As argued in Newcombe and Huttenlocher (2000), a functional analysis of spatial ecology can be used to ground a definition of knowledge or competence as consisting in having the components required to do the adaptive thing in the vast majority of situations likely to be encountered in living.

Finally, the spatial coding analysis is, in some crucial sense, agnostic with respect to the three main current contenders for understanding the A-not-B using interacting dynamic processes. While it could be integrated with Thelen et al.'s approach, as just noted, it could also be integrated with Diamond's (1990a) theory (specifying what is remembered and what needs to be inhibited) or with Munakata's (1993) model (specifying the nature of the active and latent traces).

## The role of action representations in the dynamics of embodied cognition

Natika Newton

*Nassau Community College, Garden City, NY 11530.*
**natika@worldnet.att.net**

**Abstract:** Thelen et al. present a convincing explanation of the A-not-B error, but contrary to their own claims, their explanation essentially involves mental representations. As is too common among cognitive scientists, they equate mental representations with representations of external physical objects. They clearly show, however, that representations of bodily actions on physical objects are central to the dynamical system producing the error.

Thelen et al. argue convincingly that dynamical systems theory (DST) provides a more adequate explanation of the A-not-B error than does the explanation given by Piaget in terms of concepts of object permanence. One important part of their argument concerns the mental representation of objects. They argue that the "looking, planning, reaching, and remembering" involved in goal-directed actions can account for the error without the need to posit the processing of representations. This target article can he seen as an application of their well-known DST approach to cognition in general.

It is not necessary to reject mental representations in order to accept DST as it is presented here. In fact, the authors' proposed explanation of the error entails a central role for mental representation. Clark (1997) makes a similar point about the function of representations in general in DST. Here I argue that the implicit reliance on representations in the authors' explanation of the A-not-B error is strikingly evident.

Thelen et al. appear to adopt the very common and traditional view of representations as static, "picture-like" mental entities. This view is related to another seriously mistaken one: that imagery is always visual imagery – mental pictures. But just as images occur in all sensory and propriocentive modalities, representational knowledge structures can represent organismic activities and experiences as well as perceptual objects. Even classically defined mental images "of" objects are not static brain states; they are dynamic representations of the activities involved in seeing objects (Damasio 1999). DST theorists should not view representations as useless remnants of discarded theories of cognition. They are indispensible.

The authors cite Jeannerod (1997) in their discussion of the integrative processes involved in the A-not-B task. Jeannerod has strongly argued for the crucial role of motor images as representations of what it would feel like to perform the action. Thelen et al. use very similar language in discussing the role of remembering: "The system retains a memory of previous movements that incorporates the feel of the arm in relation to the target and uses the memory to plan future responses." Mental representations of actions are motor memories, just as mental representations of objects are memories of having seen objects, even if the subject's perceptual activity is not particularly salient in the image.

A defender of their claims might object that the authors do acknowledge distributed dynamic representations of objects (Thelen & Smith 1994, p. 226). My point, however, is that there has been a systematic misunderstanding of representations, even object representations. A static object could never be represented in isolation from the activity of seeing or otherwise interacting with it. Objects are of course represented, but only as located and oriented in dynamically changing egocentric space. Their representation, moreover, includes their "affordances" of potential interactions with the subject, interactions that are themselves represented (Evans 1982; Gibson 1986; Glenberg 1996; Newton 1996). Recent neuroimaging studies show activation of parts of motor and parietal cortex in object imagery (Damasio 1999). In the same way, action representations, or memories, necessarily involve "static" objects: stable elements in the environment in which the action takes place. A memory of reaching for A (not B) is a memory of A reaching, and also a memory of A – a static object. The act and the object are inseparable in the memory representation. In short, the notion of a "static object representation" serves as a straw man here.

If "act/object" representations are indeed centrally involved in the A-not-B error as I have argued, does that fact threaten DST? Some might think so, because in that theory what serve as knowledge structures are distributed among the many processes involved in goal-directed action. There is clearly no room in such a system for symbols to be manipulated on the basis of their "shapes" as in the classic computationalist paradigm. But I am not arguing that they are so manipulated. Given the role played by motor memory in the target article, acknowledgment of representations can actually enhance the coherence of the theory. The authors describe the A-not-B error as "emerging" from the various components of goal-directed actions. Some emergent properties can be seen as self-organizing processes that maintain themselves by appropriating and/or replacing their own substrata – by playing a causal role with respect to those substrata (Ellis 1995). How this can work is debated under the topic of "Mental Causation"; that it appears to occur is noncontroversial. A strength of DST is its potential for illustrating and for explaining apparent mental causation.

There is still no consensus among theorists about an explanation. But the authors' account of the A-not-B error can offer an illustration of mental causation if their "memories" are viewed as act/object representations: the action of reaching for A. In their view, the error depends on the relative activation of the memory. If the activation of the memory of reaching for A is greater than that of the visual stimulus of B, the error will be made. Various factors influence the outcome, but the memory will produce the error under several different kinds of circumstance. The memory, in other words, is, like the error itself, an emergent property of the system of subprocesses. And much more than the error, the memory exerts a causal influence on those subprocesses: the memory causes the error by suppressing a tendency to reach to B. The


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

causal role of the error, which is itself emergent, is not nearly *so* robust. Certainly it has consequences, but it cannot be described as "self-maintaining" in the sense that stable attractors in self-organizing systems are self-maintaining. The basin of attraction of the error is much smaller; the error is more a one-time thing. The memory, on the other hand, serves an organizing role in coordinating the subprocesses ("looking, planning, reaching") to produce the error. Thus act/object representation is a vital component of DST, and not a useless fossil of an extinct cognitive theory.

## Clothing a model of embodiment

Kevin A. Pelphrey and J. Steven Reznick

*Department of Psychology, The University of North Carolina at Chapel Hill, Chapel Hill, 27599–3270.* **kpelphre@email.unc.edu**
**reznick@email.unc.edu**

**Abstract:** By delineating the parametric variations that affect infant performance in the standard A-not-B search task, the Thelen et al. model provides an important contribution to the field of infant development. We discuss several broad issues pertinent to interpreting the model. We note that the phenomenon modeled by Thelen et al. is not necessarily the one originally described by Piaget. We describe data on infant self-correction that are not addressed by the Thelen et al. model. Finally, we suggest that psychological constructs such as representation and knowledge structures are valuable to our understanding of the A-not-B phenomenon in particular and psychological development in general.

If Kurt Lewin were alive, he would probably be very pleased with Thelen et al.'s approach to modeling the dynamics of behavioral development. Unfortunately, his efforts to apply the principles and methods of topology to the development of a psychological field theory remained in the realm of metaphor due primarily to the unavailability of sophisticated computational power. Nonetheless, Lewin argued persuasively for studying the dynamics of the endogenous and exogenous forces that influence a child's cognitions and social behavior in the actual and total situation of which they are a part (Lewin 1936; 1954). In some sense, the Thelen et al. model realizes Lewin's prescient vision.

In contrast, Jean Piaget would probably not be very pleased with the Thelen et al. approach. For one thing, the phenomenon that he describes in *The construction of reality in the child* (1954) as the typical reaction of the fourth stage is notably different from the phenomenon that is now generally accepted as the A-not-B error. Here is Piaget's description of the reaction:

> Suppose an object is hidden at point A: the child searches for it and finds it. Next, the object is placed in B and is covered before the child's eyes; although the child has continued to watch the object and has seen it disappear in B, he nevertheless immediately tries to find it in A! (1954, p. 54)

Clearly Piaget did not posit multiple hidings at location A as a prerequisite for incorrect search at location B. In one observation of Jacqueline at 0; 10, the object is hidden at location A twice but in observations of Lucienne and Laurent, there is usually only one hiding at location A. For example,

> Obs. 42. At 0; 10 (9) Lucienne is seated on a sofa and plays with a plush duck. I put it on her lap and place a small red cushion on top of the duck (this is position A); Lucienne immediately raises the cushion and takes hold of the duck. I then place the duck next to her on the sofa in B, and cover it with another cushion, a yellow one. Lucienne has watched all my moves, but as soon as the duck is hidden, she returns to the little cushion A on her lap, raises it and searches. An expression of disappointment; she turns it over in every direction and gives up. (1954, p. 57)

This point is vitally important here because it is not obvious that the Thelen et al. model would predict a strong tendency toward perseveration after a single trial at location A. The vast literature based on the Uzgiris and Hunt (1975) formalization of the A-not-B error using a task with multiple hidings at location A raises very interesting phenomena, many of them well captured by the Thelen et al. model. Whether or not this literature is about Piaget's "typical reaction of the fourth stage" is an open question.

Second, given Piaget's interest in the acquisition of knowledge, he would surely balk at Thelen et al.'s contention that there is no transcendent knowledge. A person with no implicit belief in the permanence of objects would have no reason for searching for missing keys. Adults clearly have the insight that objects continue to exist when out of sight. Young children have this insight and infants might have it too. Indeed, there is some utility in positing that infants "know" where something is hidden despite reaching for it incorrectly. For example, consider findings reported by Reznick et al. (1998). In two experiments, 9-month-old infants saw an object hidden in one of three locations. Infants who reached incorrectly were allowed to search again in one of the two remaining locations. Despite a long delay between hiding and search (10- to 20-sec), infants responded correctly more often than would be expected by chance on their second reach. Some sort of adjustment of the Thelen et al. model might be evoked to explain this result, but it is hard to escape the straightforward claim that the infant has some knowledge of the object's location but is distracted from acting upon this knowledge in the initial search.

The Thelen et al. model is a vital contribution to the field because it delineates the parametric variations that affect infant performance in a standard search task (e.g., variations in stimulus identity, response modality, number of potential hiding locations, and length of delay). Nonetheless, we believe that the changes in performance observed across these variations are better viewed as different windows on the infant's underlying knowledge structures. This perspective has led us to realize the importance of a research strategy that examines infant performance on a search task under assorted task variations.

It is certainly healthy for the field of developmental psychology to question what is meant by the claim that the infant knows something (about objects or physics or mathematics or other minds). However, to discard the possibility that the infant can have any knowledge whatsoever is excessive. Our field has been down the logical positivist road, and we are sadly familiar with the intellectual and scientific stagnation associated with strong behaviorism. Mathematically rigorous modeling is generative and refreshing, but it is vacuous without terms representing plausible psychological content.

## Cooperative field theory is critical for embodiment

Patrick D. Roberts

*Neurological Sciences Institute, Portland, OR 97209.* **proberts@reed.edu**
**www.ohsu.edu/nsi/proberts.html**

**Abstract:** The field theoretic approach of the target article is simplified by setting the parameters of the dynamical field equation so that the system is near the critical point between cooperative and non-cooperative dynamics. However, embodiment of cognitive development would require a closer connection between the dynamical field interactions and the physiology of the cerebral cortex.

***1. Interactions are critical.*** Thelen et al.'s target article presents an interesting and potentially fruitful approach to deepen our knowledge about the neural basis of cognitive behavior. By focussing on the motor aspects of cognition, the authors are able to bring insights to bear on the problem of cognitive development that are drawn from motor development. This approach also succeeds in elevating motor control from the mire of control theory, thus blending motor activity with the so-called "higher" functions.

However, the field dynamics contain extraneous parameters that are unnecessary for the prediction of dynamics described in



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

the target article. Under the proper parameter settings, the cooperative (interactive) term in the field equations will lead to phase transitions that generate the desired dynamics. By emphasizing the dynamics arising from the interaction term (sect. 4.1.3), the task input (sect. 4.2.1), $S—{task}(x, t)$, and the memory input (sect. 4.2.1), $S—{mem} (x, t)$, become redundant.

The interaction term introduces instabilities into the system so that the choice of A versus B arises from small perturbations of the dynamic field (sect. 4.1.1), $u(x, t)$. These instabilities are analogous to those in the visual system that have been suggested as an underlying mechanism for certain visual hallucinations (Ermentrout & Cowan 1979). In the present case, the instabilities lead to cooperativity that represents movement direction probabilities in the space of motor activity.

By encapsulating the cognitive choice of the hidden toy's location in terms of motor representations, the authors are able to tie their formalism to population codes (Georgopoulos 1996). The connection would be more complete if the memory were embedded in neural structures of the cerebral cortex. If the theory were truly "embodied," then $S— {mem} (x, t) $ inputs might be contained in the interaction term and represent synaptic interactions.

***2. Critical fields persevere.*** Analysis and simulations can be used to study pattern formation on the one-dimensional domain that represents the movement space. The following analysis shows the existence of instabilities that yield a phase transition in the configuration space of the field $u(x, t)$. Results of simulations are presented that yield the behavior near the critical point of the phase transition between disordered and fixed behavior. Because the movement space is represented as the direction of reach from the sitting child, the domain encompasses a circle. The fixed behavior would appear as oscillations in the field value around the circle. High field values over a particular value of $x$ would represent a high probability of a reach in that direction.

To find the natural wavelength generated by the interaction term our starting point is the field equation (3), $\tau \{\dot{u}\}(x, t)$ = $ − u(x, t) + S(x, t) + g(u(x))$. The interaction term is a convolution of a threshold function of the field, $f(u(x))$ with the interaction kernel. To simplify matters for the analysis (we use the exact expression in the simulation) , we can expand the threshold function so that, $f(u(x)) = u(x) + t\cdots$. To seek instabilities in the absence of perturbations ($S(x, t=O$) , we assume an oscillatory solution, $u(x, t)=e^{\lambda t}e^{ikx}$ and see whether it yields solutions to the field equations. Instabilities decay if the growth factor $\lambda$ is negative for all wave numbers $k$ otherwise oscillations exist.

The result is that oscillations appear on the order of the system size. That is, a single reach direction will appear as a region of high field activity, and the reach will be frozen in that direction. Thus the frozen phase spontaneously generates the task input, $S—{task} (x, t)$. However, with sufficient noise in the system, the instability is overwhelmed and there would be no preferred direction so that no reaching is manifest. If the noise, or the interaction kernal, is properly tuned *so* that the *system* is near the transition, then spontaneous reaching occurs in random directions. A simulation confirms these analytic results, as shown in Figure 1. In this simulation, the field $u(x)$ is discretized into a set of interacting units, and the noise is set so that the field solution is near the critical point. Two perturbations are introduced: a cue to target A, $S— {task}(x)$, and a memory of target B that is encoded as a slight increase in the interaction kernel in the vicinity of target B. In this simulation, the memory does not fade.

Spontaneous switching between the two targets is seen in part A of the figure. The simulation also shows another feature of field behavior near the critical point: high susceptibility. The region of high field activity is the result of very small perturbations, so small that no result would he seen in higher noise conditions. Part B of the figure shows the result of an average over many trials for three different intensities of the cue. It is interesting that there can be stronger memory behavior with a small cue (dashed trace) than with no cue at all (dotted trace).

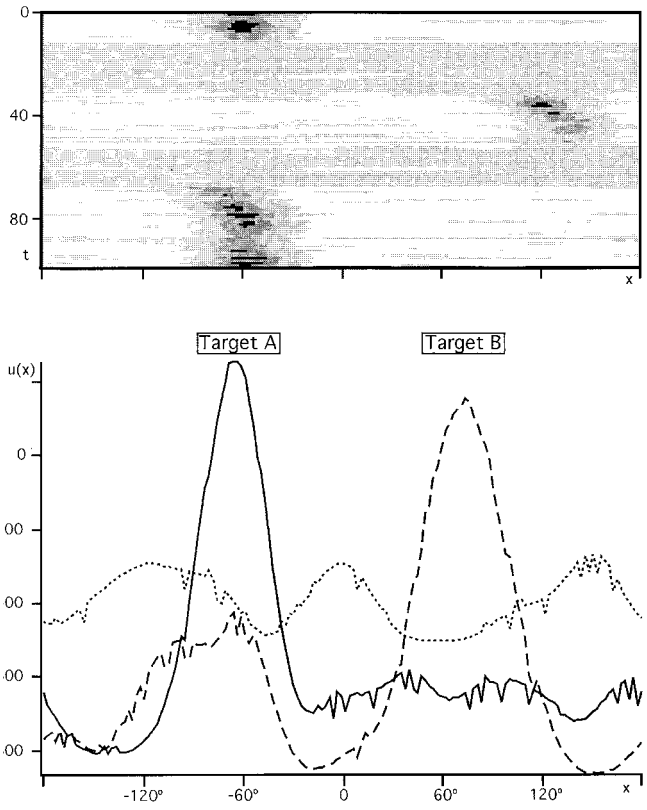



Figure 1 (Roberts). Simulation of field dynamics near the critical point. The cue input is located at target A (−60$^{o}$) and the interaction is slightly increased at target B (70$^{o}$) to represent memory effects. (A) Dynamical field amplitude of $u(x,t)$. Grey scale represents amplitude from lowest (white) to highest (black). Random switching takes place between target A ($t \in$[1,13] and $t \in$ [67,100], target B ($t \in$[32,54]), and non-cooperative dynamics ($t \in$[14,31]) and ($t \in$[55,66]). (B) Average of the dynamical field amplitude over $t \in$[1,4000] for simulations with no cue input (dotted trace), moderate cue input (dashed trace), and large cue input (solid trace).

***3. Critical connections to the brain?*** The conclusion we may draw from this exercise is that undecided movements crystallize into a decision as the system balances on the edge of a critical point. In the target article, the weak link to embodiment is in the physiological connection to motor population coding (Georgopoulos 1996). The memory term could be embedded in the interaction term with synaptic interaction yielding the form of the interaction kernel and perhaps synaptic plasticity regulating the strengths of the synapses. This is not unrealistic, given the known cortical interactions of local excitation and lateral inhibition. However, embodiment of the theory would require a kind of "kinetopy" that has not been found in the motor cortex. In their attempt at embodying cognition, the authors must be careful not to "disembody" motor control.


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

## Understanding A-not-B errors as a function of object representation and deficits in attention rather than motor memories

Ted Ruffman

*Department of Experimental Psychology, University of Sussex, Brighton, East Sussex BN1 9QG, United Kingdom.* **tedr@biols.susx.ac.uk**
**www.biols.susx.ac.uk/groups/ep.html**

**Abstract:** In this commentary, I raise several points. First, I argue that non-search tasks show that the A-not-B task is about object representation, even if perseveration can occur without objects. Second, I provide an alternative interpretation for the finding that changing body posture reduces A-not-B errors. Third, I provide an alternative interpretation for the finding of convergence in reaching behavior in two-target tasks. Fourth, I suggest attention deficits can explain the A-not-B error on their own with no necessity for motor memories.

Thelen et al.'s model of the A-not-B error is laudable in its attempt to weave together a number of factors interacting together in a dynamic fashion over time. There is probably much sense in this way of thinking. Yet I believe there are a number of inconsistencies in their account and ambiguities in interpretation that raise important questions.

We found that 8- to 12-month-old infants showed surprise (longer looking times) when on the B trial of an A-not-B task, the object was drawn from the A location in comparison to the B location (Ahmed & Ruffman 1998). Infants showed this pattern at delays of 6 and 15 seconds, even when they erred by searching at A on search tasks at an average delay of 4.5 seconds. Thelen et al. argue that looking tasks are not necessarily better at revealing core knowledge than search tasks. I agree, in that looking tasks may reveal a different form of knowledge – graded representations or largely implicit knowledge. Yet I think Thelen et al. are grossly mistaken to claim that "whether infants do better at looking than reaching is somewhat of a side issue" (sect. 2.2.4, para. 2). This claim seems related to another of their claims, that the A-not-B task is not about objects (i.e., that the error can occur even when only lids are used). Even if search errors can occur without objects, it is my view that looking tasks are crucial to understanding the A-not-B task. If the task was not about objects, then why would infants show surprise when the object is drawn from the A location in comparison to the B location? Motor memory is not relevant because the index of understanding (looking time) is not a repeated motor behavior. The only plausible reason is that infants were surprised because they thought the object was at B. In other words, the infant's representation of the object's location matters.

Two other concerns revolve around evidence that Thelen et al. use to support the notion of motor memories. First, they cite Smith et al.'s (1999b) finding that changing the infant's body posture before trial P1 makes it likely that infants will search correctly at B. Thelen at al.'s interpretation of this finding is that the infant's bodily memory was disrupted. However, changes in body posture are almost certainly confounded with changes in attention. Whereas it is known that allowing babies to retain their position on the mother's lap leads to A-not-B errors, moving the baby from a seated to a standing position will plausibly interrupt this breakdown in attention and refocus the baby on the object and the position of interest (B). Second, Thelen et al. claim that reaches tend to converge across trials in two-target reaching tasks in comparison to one-target reaching tasks. They cite this as evidence for motor memories in two-target tasks. One problem with such claims is that they are apparently made on the basis of different numbers of trials in each task. Figure 5 shows five reaches for the one-target task and eight reaches for the two-target task. Visual inspection of this figure suggests that convergence is only attained towards the end of the reaching trials in the two-target task. In other words, there are insufficient numbers of trials in the one-target task to know whether convergence would also be attained in this task. Further, Thelen et al. argue that infants who commit A-not-B errors tend to have higher convergence and that the convergence reflects a motor memory that results in the errors. There is another way of construing this data. Infants who commit A-not-B errors have converging reaches because their visual attention becomes firmly fixed on the A location around the time they reach. Convergence results because reaching is guided by an increasingly stable fixation of gaze (on A) that guides the hand along the same path to the target location. In this view, it is attention rather than a motor memory that causes the error. Convergence is simply a by-product of attention.

Attention has particular strength in explaining a vast array of experimental findings. It is typically conceptualized as an ancillary deficit, that is, as a factor that masks the child's understanding of object permanence. Although Thelen et al. clearly do not think of attention in these terms it is easy to imagine that attention on its own, with no input from motor memories, can account for A-not-B errors. They cite three influences on search errors. Thelen et al. acknowledge that the first two influences, (1) task input (e.g., the layout of the hiding locations, the desirability of the object), and (2) specific inputs (e.g., tapping on lids), could plausibly be seen as affecting attention. The third factor is motor memory: repeated reaches to A create a motor memory that makes A reaches more likely. Yet reaches to A can also plausibly be seen as making A more salient and attention grabbing. For instance, Mandler (1987) presented octagons for a brief rime to adults, and then presented them with pairs of octagons (one old, one new). Whether they were asked which octagon was brighter, which was darker, or which they liked more, participants tended to select the old one. It is as if the old stimulus is simply more salient than the new stimulus (see also Seamon et al. 1997 and Bornstein 1989 for a review). Appeals to motor memories or inhibitory deficits (Diamond et al. 1994; Markoovitch & Zelazo 1999) are unnecessary.

Improvements in attention have plausibility in that, like inhibition, they have been linked to frontal lobe development. Thelen et al. point out that frontal lobe development is gradual. The same is true for improvements in attention. For instance, A-not-B errors are probabilistic and need a steadily increasing delay to occur as the baby grows older (Diamond 1986) This delay can be seen as taxing attention (Harris 1987).

## Does cognitive development move beyond sensorimotor intelligence?

Catherine Sophian

*Department of Psychology, University of Hawaii, Honolulu, HI 96822.*
**csophian@hawaii.edu** **www.hawaii.edu/~csophian/**

**Abstract:** Thelen et al.'s account of cognition as the dynamic interaction of processes of perceiving, reaching, and remembering within a movement planning field is a useful articulation of the Piagetian concept of sensorimotor cognition. The claim that the same kind of analysis applies to all kinds of cognition at all ages, however, is questioned in light of the distinction between sensorimotor and symbolic cognition.

The model of embodied cognition put forward by Thelen et al. makes several important contributions to thinking about cognitive development. It underscores the futility of trying to draw a sharp distinction between cognitive and "merely" perceptual capabilities and, more particularly, of trying to understand cognitive-developmental achievements while isolating them from perceptual processes. It recognizes that cognitive performance is always multiply determined and gives us a coherent way of thinking about the developmental ramifications of that fact. And it calls attention to the importance of physical and motor developments in making possible new experiences that in turn lead to cognitive advances. Although the social context in which development occurs is not emphasized here, the model's emphasis on the importance of bodily experience is clearly compatible with a social contextualist per-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

spective inasmuch as our existence as embodied beings is fundamentally a social one.

These powerful developmental insights, however, contrast with the surprisingly non-developmental stance Thelen et al. take in extrapolating from their model to broad conclusions about cognition at all ages: "the same kind of analysis can be applied to any task at any age. . . . There is no time and no task when such dynamics cease and some other mode of processing kicks in" (Introduction). The kind of interpretation that is offered for infants' responses to hidden object problems – that they are not a function of the presence or absence of an "object concept" but derive from the dynamic interaction of processes of perceiving, reaching, and remembering within a movement planning field – is quite compatible with Piaget's characterization of infant cognition as "sensorimotor." Indeed, the dynamical model can be viewed as a useful articulation of the Piagetian assertion that infant intelligence is inseparable from action. But in claiming that all of cognition is like this, Thelen et al. clearly part company not only with Piaget, who contrasts the sensorimotor intelligence of infants with qualitatively different forms of knowing at older ages, but with any account of development that incorporates representational change.

The kinds of findings Thelen et al. set out to make sense of – evidence that performance can be facilitated or impeded by changing the particulars of a task in any number of ways – are by no means unique to research on infant cognition. The field of developmental psychology is replete with demonstrations that children who fail a standard task can succeed on a modified one, and that further variants of the modified task may again give rise to systematic errors. Such findings indicate that the cognitive performances of older children, like those of infants on hidden-object tasks, are multiply determined. All cognition is embodied at least in the sense that we can never put to use what we know without bringing that knowledge into interaction with processes of perception, remembering, and so on. But that does not mean that cognitive processes and their relation to perception and action must be fundamentally the same at all ages.

To assert that no knowledge structures exist apart from action is to hold that all cognition, at all ages, is sensorimotor in character. But for Piaget, what was fascinating about infant behavior was the cumulation of sometimes subtle changes until a fundamentally new "mode of processing" became available with the advent of language and symbolic functioning. The A-not-B error was of interest precisely because it showed that what appeared to be representational intelligence, the ability to keep in mind an object that was no longer visible, was in fact still intimately linked to processes of perception and action. However, in Piaget's view, behaviors such as deferred imitation and the use of language could not be accounted for in the same way. These behaviors were taken to indicate a non-sensorimotor form of cognition because they provided evidence of knowledge in circumstances far removed from those in which that knowledge had been acquired. In effect, Piaget took the decoupling of a remembered behavior from its original context as evidence for knowledge structures that were decoupled from perception and action in a way that earlier ones had not been.

Piaget's emphasis on deferred imitation and language as hallmarks of the emergence of a qualitatively new form of cognition suggests that they would be good candidates for a test of Thelen et al.'s assertions as to the generality of the kind of analysis they developed for the A-not-B error. Could a model of the same kind as the one presented here account for imitation of observed behavior at a time and place far removed from the original episode, in the absence of any immediate imitation (and hence, of any motoric remembering)? Could it account for language acquisition and the ability to talk about things that are far removed in time and space? It would take demonstrations such as these to substantiate the claim that processes of "knowing" in older children and adults are no different from and no less tied to perceiving and acting, than those of infants.

The viability of an embodied account of cognition, however, need not hinge on the demonstration that symbolic behaviors can be modeled in the same way as sensorimotor ones. If Piaget is correct that symbolic functioning grows out of sensorimotor activity, then even symbolic cognition is embodied in the sense that it "arises from bodily interactions with the world [and] depends on the kinds of experiences that come from having a body with particular perceptual and motor capabilities." Further, even symbolic knowledge can only be put to use in the context of dynamically interacting processes of perceiving, acting, and remembering. The multiply determined character of all cognitive performances makes it clear that, while symbolic knowledge may not be reducible to the mesh of dynamically interacting processes of perceiving, acting, and remembering, it is still inextricably entwined with them.

## The essence of cognitive development

John P. Spencer

*Department of Psychology, University of Iowa, Iowa City, IA 52242.*
**john-spencer@uiowa.edu**
**www.psychology.uiowa.edu/Faculty/spencer/spencer.html**

**Abstract:** Psychologists have long debated the underlying cause of infants' perseverative reaching. Thelen et al. explain the error in terms of general processes that make goal-directed actions to remembered locations. The context- and experience-dependent nature of their model implies that there is no single cause of the A-not-B error, and, more generally, no core essence to cognitive development.

The Piagetian A-not-B error is one of the most studied phenomena in developmental psychology. Nevertheless, as Thelen et al.'s target article illustrates, there is little consensus as to what this odd error infants make really means. Does the error indicate that 8–10-month-old infants fail to represent objects as independent of their own actions – that infants do not possess the object concept? Does the error index an immature prefrontal cortex? Does the error indicate that young infants encode locations egocentrically? Although there are a myriad of explanations, Thelen and colleagues point out that none of these explanations capture the full picture of infants' perseverative reaching. None of these accounts explains all the data. Why is this the case? One limitation of previous explanations is that they try to explain the essence of the A-not-B error – the single developmental change uniquely revealed by the presence, and later absence, of the error. Prom this perspective, the error provides a special window into a period in development (Piagetian Stage IV) captured in the context of a special task.

The dynamic field theory presented in the target article pursues different goals: to explain the continuous, time-dependent integration of action-related cues – both external and internal – that influence how infants perform in the A-not-B task. The field theory, in short, explains the error by explaining how the general processes that makes goal-directed actions in infancy work, that is, how processes that operate in many tasks and at many points in development lead to perseverative reaching in some circumstances, but "correct" reaching in others. At its core, then, the field theory makes a profound point – there is no one essence to the A-not-B error, no unique developmental change to be discovered by studying this phenomenon. Instead, the error appears and disappears as a function of experience- and task-specific events that play out over multiple time scales – real-time (seconds within a trial), learning time (trial by trial), developmental time. This has important implications for our understanding of A-not-B-type errors.

The field theory predicts that A-not-B-type errors are not specific to a period in development or to a particular task. Recent data from a series of forthcoming articles bear this point out. For example, my colleagues and I have demonstrated that 2-year-olds – children well past the age when infants acquire object permanence, have a "mature" prefrontal cortex, and encode locations


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

allocentrically – make A-not-B-type errors in a sandbox task (Spencer et al., submitted). The trial-to-trial structure of this task was identical to the canonical A-not-B task reported in the target article; however, rather than hiding a toy in a brown box covered by brown lids, we hid a toy in a 5-foot long sandbox. There were several A trials in which a toy was hidden at A, covered up, and, after a delay, children were allowed to search for the object. Next, we hid a toy at a "B" location 8–10 inches away from A. Two-year-old responses on the B trials were significantly biased in the A direction. Indeed, some 2-year-olds reached back to the A location on the B trials. Thus, 2-year-olds made the A-not-B error.

More recently, we have used a different – "spaceship" task – to demonstrate that many of the same factors that influence infants' perseverative reaching affect children's and adult's ability to remember the location of a hidden object. For example, we have shown that location memory decays systematically over 5–20 sec delays with both children (3-, 6-, 11-year-olds) and adults (Hund & Spencer, submitted; Schutte & Spencer, submitted a,b) However, as predicted by the dynamic field theory, memory decay is not a global characteristic of the memory system but depends on the specific information integrated in memory. We have also demonstrated that location memory can be biased toward a longer-term memory of locations repeatedly moved to on previous trials (Schutte & Spencer, submitted; Spencer & Hund, submitted b). Specifically, if people reach more often to one remembered location – "biased" location – than others, memory responses are distorted in the direction of the biased target.

Finally, we have begun to capture developmental changes in location memory abilities between 3 years and adulthood using the concepts of the dynamic field theory (Spencer & Schöner, in preparation) As in the target article, developmental modifications of cooperativity are central to our account. Although this parallel is exciting, it is important to emphasize that changes in cooperativity do not reflect the core change that turns a perseverating infant into a less-perseverating child or adult. This is just one piece of a general processes account. And changes in cooperativity can be realized in many ways (see the target article for further discussion of this point).

But if there is no essence to the A-not-B error, are we left with the daunting alternative that everything matters? The formalization provided by the dynamic field theory answers with a resounding "No." Specific things matter in specific ways at specific times. This is what makes the dynamic field theory both flexible – it explains the context-specific nature of the A-not-B error – and testable. So what is the essence of the A-not-B error, or, perhaps more generally, the essence of developmental changes in children's ability to plan and remember actions to retrieve important objects in the world? Development in this case (and perhaps in all cases?) is not in an essential change isolated to a particular point in development. Rather, development lies in the complex, but lawful interactions that play out across different time scales and in different contexts. And Thelen and colleagues have provided a roadmap toward realizing this new, non-essentialist vision of what developmental change is all about.

## *Plus ça change . . .* : Jost, Piaget, and the dynamics of embodiment

J. E. R. Staddon,[a] A. Machado,[b] and O. Lourenço[c]

[a]*Department of Psychology: Experimental, Duke University, Durham, NC 27705;* [b]*Department of Psychology, Indiana University, Bloomington, IN 47405;* [c]*Faculdade de Psicologia e de Ciências da Educação, Alameda da Universidade, 1500 Lisbon, Portugal.* **staddon@psyc.duke.edu amachado@indiana.edu d1506944@fc.ul.pt www.psych.duke.edu/jers/labgroup.html**

**Abstract:** The "A-not-B" error is consistent with an old memory principle, Jost's Law. Quantitative properties of the effect can be explained by a dynamic model for habituation that is also consistent with Jost. Piaget was well aware of the resemblance between adult memory errors and the "A-not-B" effect and, contrary to their assertions, Thelen et al.'s analysis of the object concept is much the same as his, though couched in different language.

Critic John Horgan recently commented discouragingly about progress in psychology: "Theories of human nature never really die; they just go in and out of fashion" (1999, pp. 6–7). We are extremely sympathetic to the general theme of Thelen et al.'s system-theory approach to developmental psychology. But we now draw attention to two ways in which it conforms to Horgan' s critique. First, the theory ignores an older and simpler approach to memory reversals; and second, the theory is not as different from Piaget's as its authors contend.

***Old memories.*** Who has not had the experience of moving something – a book or a file, say – to a new location, then going away on holiday and, returning, looking fruitlessly for the object in its old location. This is an everyday example at a well-established but frequently forgotten memory principle, Jost's Law: "Given two associations at the same strength, but of different ages, the older falls off less rapidly in a given length of time" (Hovland 1951, p. 649; after Jost 1837). Before the vacation, memory-trace strength for the new location was higher than for the old, but after a delay, because of Jost's Law, the two are reversed. The older "association" (we must forgive Hovland the unfashionable language) decays more slowly than the newer. It seems to have gone unnoticed that the A-not-B error is strikingly consistent with Jost's Law, albeit on a short time scale. What are the implications of this idea?

***A simple dynamic model.*** To answer this question requires a dynamic model that instantiates Jost's Law. Just such a model was proposed by one of us to account for the properties of habituation (Staddon, in press; Staddon & Higa 1996; 1999), and it is illustrated in Figure 1. Each occurrence of an event is input to a cascade of thresholded-input integrators whose output represents the strength of memory for the event. The light line in Figure 1 shows the response of the system to a series of inputs: memory strength increases with each stimulus presentation and then decays after the series. The important point for Jost's Law is that the memory trace decays rapidly at first and then more slowly, allowing the situation illustrated by the heavy line in Figure 1, which shows the effect of a single presentation of object B. The trace stimulated by B is initially higher than the older light-line trace for A, but soon falls below it. If the infant's reaching is controlled by the stronger trace, clearly he will respond correctly – choose B – to the left of the vertical line at the point of intersection of the two curves. But later, to the right of the line, he will show the A-not-B error because the older trace is now stronger than the newer one.

This model implies a number of quantitative relations:

1. The more occurrences of event A, the stronger the A-not-B error; in the limit, if A is presented only once, there should be no error.
2. The error should not occur at short delays (i.e., to the left of the vertical line in Fig. 1).
3. The point of transition from correct to incorrect reaching should decrease with the number of A presentations.
4. The delay after presentation of "B" necessary before the A-not-B error will occur should also depend on the time between B



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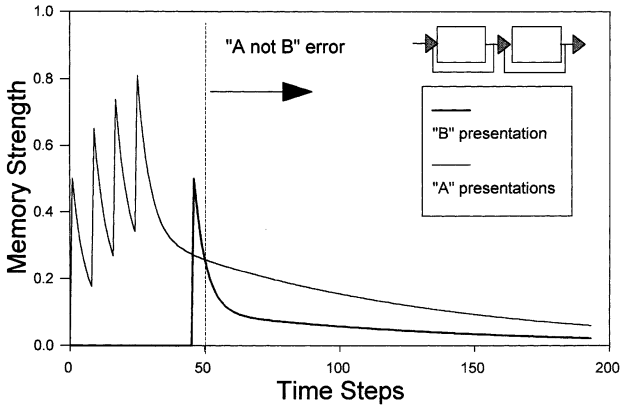



Figure 1 (Staddon et al.). The A-not-B effect predicted by memory decay. *Light line:* Effect on the trace of four presentations of object A. *Heavy line:* Effect of a single presentation of object B. The "A" trace is stronger to the right of the vertical line, leading to the error. The traces were generated by a two-stage, feedforward (S-type) cascade model of the type discussed by Staddon and Higa (1996). Parameters for the two integrators: $a_1 = .8, a_2 = .99$, $b_1 = .4, b_2 = .1$.

and the last "A" presentation: the longer the A-B time, the longer the delay after B necessary to get the effect. This prediction is illustrated in Figure 2.

5. The transition time (i.e., the A-B delay that allows correct responding) should also depend on the spacing of A. Specifically, as the spacing of A increases while the number of A presentations and the A-B interval remains constant, the transition time should decrease, attain a minimum, and then increase. This prediction follows from the rate-sensitive property of habituation, the fact that habituation may be more persistent following spaced than massed training, even though the level of habituation is greater with massed training.

Thelen et al. describe a series of studies consistent with the first three of these predictions: (1) "All A-not-B studies involve some, often unspecified, number of *training trials*" (sect. 2.2.1); (2) "infants do not err at short delays," sect. 1.1, para. 4; and (3) In this case, time of transition has not been measured, but probability of choosing A has: "Smith, Thelen, and their associates have shown conclusively that commission of location errors with the B cue is

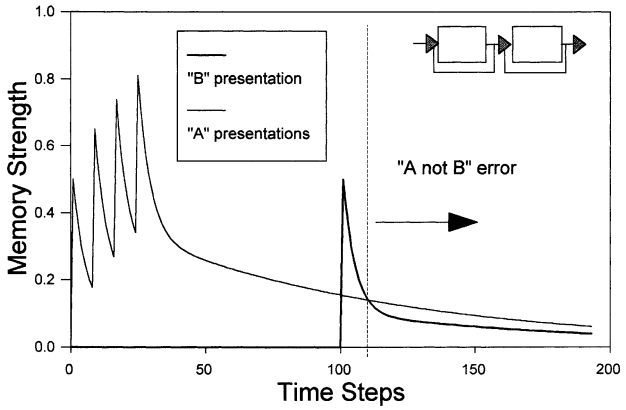



Figure 2 (Staddon et al.). The effect of A-B delay on choice delay. Notice that the time between the occurrence of "B" and the time of transition indicated by the vertical line is longer when B is delayed (compare with Fig 1). The longer the time between the last "A" presentation and the first "B" presentation, the longer the delay between "B" presentation and choice before the A-not-B error will occur.

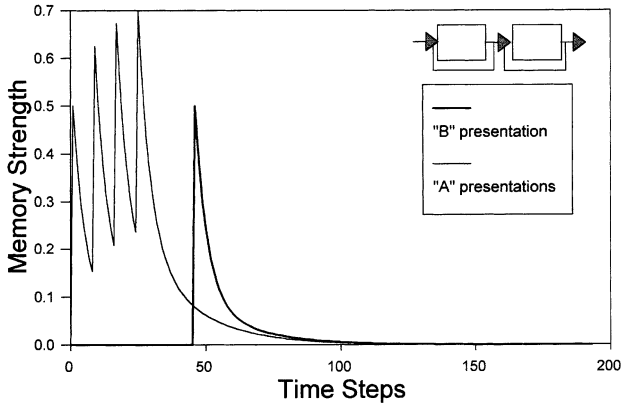



Figure 3 (Staddon et al.). The effect on the A and B traces of reducing the time constant of the slower integrator, $a_2$ to .95. The A-not-B error is eliminated.

strictly a function of the number of prior reaches to A" (sect. 2.2.5). But no one seems to have looked systematically at the effect of A-presentation spacing and A-B delay on the time of transition from correct to incorrect reaching – points 4 and 5. Parametric experiments are tough to do with infants, but these may be worth a try.

An obvious inference from this analysis is that the developmental changes indexed by the A-not-B error correspond to, even if they are not entirely determined by, progressive changes in the memory system. One possible change is shown in Figure 3. Simply decreasing the value of the slower integrator, A2, from 0.99 to 0.95 can abolish the error. No doubt other parameter changes would produce similar results, but the idea that the trace changes in such a way that different events become more clearly separated looks like the easiest way to duplicate the disappearance of the A-not-B error as the infant ages. Moreover, the change illustrated in Figure 3 implies correlated effects. For example, young infants should recover from habituation more slowly than older infants, another testable idea.

This is a dynamic analysis only. It makes no predictions about non-temporal manipulations such as the similarity, salience, and valence of the A and B objects and their locations. Moreover, the analysis is restricted to the acquisition of motor habits, which we (and Piaget) presume to be a large part of what is going on when infants engage in this simple task. We do not doubt that other, "higher" processes are also developing, so that the motor-learning component becomes less important as the child matures. But it never vanishes entirely, as Jost's Law testifies.

***Piaget redivivus: The object concept.*** And Thelen et al. agree: "it is incorrect however, to assume that perseverative reaching responses are unique to a particular stage in infancy" (sect. 2.2.6). But they go on to conclude:

> Such diverse context effects pose a serious challenge to Piaget's original interpretation. If the A-not-B error is a true measure of the status of infants' representations of objects, how can it be that what they know depends on so many seemingly irrelevant factors? . . . The contemporary consensus is that Piaget's account is incorrect, . . . we agree with some of our colleagues that the A-not-B error is not about an object concept per se. (sect. 1, last para., sect. 1.1)

Thelen et al.'s overall arguments and conclusions express two serious misunderstandings of Piaget: that contextual influences are incompatible with his theory, and that the A-not-B task measures the child's object concept. As to context, we note that Piaget was quite aware of it. For example, he himself had evidence suggesting the type of object (e.g., people vs. toys) clearly makes a difference in the construction of the object concept. He also knew that even older children may revert to earlier reactions: for example, at 2 years and 4 months, "Lucienne, hearing a noise in my [Piaget's] office, says to me (*we are together in the garden*): 'That is papa up there'." Piaget 1954, p. 59, our emphasis). He was also aware that even adults on


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occasion fall prey to the A-not-B error: "I take my clothes brush out of the small bag in which it is usually kept and place it on a table; afterward when I want to use it I look for in its bag and cannot understand its disappearance. I see my pipe on my desk, put it in my pocket, then hunt for it on the desk" (p. 60). Finally, Piaget discovered the A-not-B error by exploring the influence of specific contextual changes on the way children search for hidden objects. Far from ignoring contextual effects, he exploited them.

The second thing to note is that contextual effects can be explained and predicted on the basis of Piaget's interpretation of the A-not-B error. For example, Thelen et al. report that small changes, such as varying the number of reaches to A, can abolish the effect. This is perfectly consistent with Piaget, who argued that there is a period during which the child's search for hidden objects depends more on previous actions than on the displacements of the object. In other words, before it becomes something external to, and independent of, the self, the object is something at the disposal of the infant's actions. Lucienne, at 15-months-of-age, is in the garden. She has just greeted her father, Piaget, when her mother asks: "Where is papa?" (Piaget 1954, p. 59). Although Piaget stands one meter away, in full view of the child, Lucienne looks at the window. Piaget explained his daughter's residual behavior by appealing to *habit strength,* the number of times in the past Lucienne had looked at the window and seen her father.

In summary, not only was Piaget well aware of context effects, but he also explained them by appealing to the same mechanisms invoked by Thelen et al. Although he used a less technical language, Piaget would certainly have agreed with Thelen et al.'s frequent claim that the A-not-B error emerges from the coupled dynamics of looking, planning, remembering, deciding, and reaching.

But if Piaget knew about many of the findings that Thelen et al. now hold against his theory of the construction of the object concept – and even explained them by invoking similar mechanisms – why then did he not concede that these findings undermine his theory? The answer is related to Thelen et al.'s second misunderstanding: that the A-not-B task *measures* the child's object concept. Thelen et al. attribute to Piaget the view that to have the object concept is to have "some causal structure [presumably in the brain] that generates a thought or a behavior." According to this view, a child will either have this structure in the brain, in which case she will not err in the A-not-B task, or she will lack it, in which case she will err. Therefore, the child's performance can be used to measure the presence or absence of this internal structure, the physical embodiment of the object concept.

But this was not Piaget's view. Nothing could be further removed from his epistemological and constructivist assumptions than the idea that to have a concept is to have a physical embodiment of it in the form of an internal causal structure, a functional entity, or even a representation that is distinct from action, operations, and interactions (Lourenço & Machado 1996). For Piaget, to *know* is to act and operate upon reality, and to have the object concept is to act in distinct ways toward objects, to search for them when hidden, to take note of their displacements and their temporal order whether the displacements are visible or invisible, and the like. In Piaget's view, therefore, the A-not-B error is not a measure of the object concept but a *criterion* of the child's level of understanding of objects as external, permanent realities. Differently stated, the object concept does not refer to an entity that is separate from, and causally related to, the child's acts, but to formal aspects of these acts (i.e., not their force or duration but their relatedness to the object's displacements, their order, etc. Incidentally, this explains why the object concept is, for Piaget, inseparable from the concepts of space, time, and causality). Accordingly, Piaget's theory could not be closer to an embodied view of cognition, and it is simply wrong to attribute to him the notion that the child searches for hidden objects because of a "disembodied belief in the permanence of objects" (Thelen et al. sect. 7.1).

Piaget argued that to have a concept is to act and operate upon reality in distinct and organized ways. It follows that whatever factors influence the dynamics of action will also necessarily influence the criteria for the object concept. Piaget did not study these factors systematically, but he did elaborate on their significance for his theory. In particular, he discussed *memory* errors, *spatial localization* errors, and difficulties with *object formation.* This may come as a surprise to Thelen et al., but Piaget concluded that although these three accounts may seem incompatible at first sight, they are actually the same explanation approached from different viewpoints:

> To exist as object is to be ordered in space, for the elaboration of space is precisely the objectification of perceived images. A reality which merely remains at [the] disposal of the action without being situated in objective displacement groups is therefore not an object; it is only a potential act. (Piaget 1954, p. 44)

Thelen et al. close their paper with the following comments:

> Finally, does this model have anything to say to Piaget's issue: *when* do infants acquire the object concept? We believe this question is ill-posed and cannot be answered because there is no such thing as an "object concept" in the sense of some *causal* structure that generates a thought or a behavior. There is *only* "knowledge" of objects as embedded in the immediate circumstances and the history of perceiving and acting in similar circumstances. (sect. 7.1, penultimate para. Emphasis added)

But the question is as ill-posed for Piaget as it is for Thelen et al. The issue is not *when* the object concept is acquired, but *how* it is acquired. For Piaget, the sequence of transformations in the child's understanding of the object concept was something to be explained, not to be invoked as an explanation. Moreover, to state that "there is only 'knowledge' of objects as embedded in the immediate circumstances and the history of perceiving and acting in similar circumstances" is obviously correct, but only in the same sense that there is only paint in the Mona Lisa. What we want to know is how the painting was put together. What we – and Piaget – want to know about the object concept is how *it* is put together during ontogeny. The lesson is clear: If one challenges Piaget's theory but simultaneously overlooks its epistemological and constructivist underpinnings, then one simply misses the target.

ACKNOWLEDGMENTS

Machado and Staddon were supported by grants from the National Institute of Mental Health to Indiana and Duke Universities, respectively.

## Dynamic comparison of the development of combinatory manipulations between chimpanzee and human infants

Hideko Takeshita

*School of Human Cultures, The University of Shiga Prefecture, Hikone, Shiga 522–8533, Japan.* **hideko@shc.usp.ac.jp**

**Abstract:** I present my observations of combinatory manipulations by three infant chimpanzees in a series of test tasks. Common characteristics of motor patterns were observed across the tasks between both infant chimpanzees and 1-year-old infants. Based on the results, I point out that comparative approach can illuminate Thelen et al.'s arguments.

The target article by Thelen et al. is highly stimulating. Their argument seems convincing especially for those who explore the phylogeny of cognition in primates. I would like to present some related data for further discussion from the viewpoint of comparative developmental psychology.

I have made systematic observation on three infant chimpanzees from ages 2 to 4 who participated in a series of diagnostic tests of combinatory manipulations (Takeshita 1999). The tasks were piling up blocks, seriating nesting cups, and inserting an object into a corresponding hole of a plate or a box. These tasks were originally devised for developmental diagnosis of human infants. To summarize, the chimpanzee infants tested displayed combina-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

tory manipulations comparable to those of 1-year-old human infants.

I did not compare only final achievements such as success or failure failure in piling up blocks, seriating cups, or inserting an object into a corresponding hole, but also actions performed by both species. In infant chimpanzees and 1-year-old infants common motor characteristics were observed across the tasks, namely "repetition" of actions, "adjustment" of actions, "reversal" of actions, and "shift" of attention.

Starting with putting a block on another block, the subjects repeated the action of placing it on the top of the blocks they had lined up. They adjusted the actions using their hands functionally asymmetrically to successfully build "a tower." They put a cup into another one and also repeated the action of putting a cup into the seriated structure of cups, with the "pot" strategy (Greenfield 1991; Johnson-Pynn et al. 1999), which seems typical of both infant chimpanzees and 1-year-old human infants. It happened that the cup they picked up was bigger than the one inserted just before. They tended to take back the cup just after they had made a nonseriated structure. Then, they tried to make a seriated structure, showing a variety of manipulations to adjust to making a seriated structure, that is, only repeating the previous behavior of insertion, putting the other side of the cup on the seriated structure, transferring the cup between hands, and so on.

Taking back a cup from the nonseriated structure is "reversal of action." "Shift of attention" also occurred in this problem solving situation. After taking back a cup from a seriated structure, they left it on the table and picked up another cup from the structure and put it back again into it or into another cup on the table. Picking up another cup is considered as "shift of attention towards objects" and putting it into another cup is considered a "shift of attention towards locations." "Shift of attention towards locations" also happened when the subject put a disc on a wrong hole of a plate or a box, along with adjustment behaviors for putting the disc into the wrong hole again.

Phenomena similar to the A-not-B error could be observed in one of the combinatory manipulation tasks mentioned above.

In the last test, the subjects were presented a disc and a plate with a circular hole, a triangular hole, and a square hole, which were lined up on the plate. The circular hole was the fit for the disc to be inserted. At the beginning of a trial, an experimenter put the disc in the circular hole and encouraged the subject to take it off. After the subject took the disk off, the experimenter encouraged the subject to put the disc in one of the holes again. If the subject succeeded in putting the disc in the circular hole again, the experimenter took the plate to rotate it. The circular hole was located on the left if it had previously been the right, and vice versa, after the rotation. The experimenter encouraged the subject to put the disc in one of the holes again.

Generally, human infants can put the disc in the circular hole after they take it off the hole at around 12–14 months. Most infants can put the disc in the circular hole even after the rotation of the plate, from 24 months onward. Between 15–23 months, infants put the disc in the foursquare hole at first, and after the failure, they take it off to put it in the circular hole anew. This succession of "A-not-B error and the B-not-A correction" seems to be a precursor for the direct combination of a disc and a circular hole by older infants and was observed in my two chimpanzees who are 4 years old. In contrast, the 2-year-old chimpanzee adhered to the "A-not-B error" and only repeatedly put the disc into the square hole, manipulating the adjustment for success. Performance by the 2-year-old subject seemed less mature than that by the 4-year-old subjects.

What I would like to point out here is that the comparative approach seems fruitful for illuminating how "the coupled dynamics of the ordinary processes of goal directed actions" generates human cognition. Combinatory manipulations are prerequisites for tool use and the development has been considered one of the conditions for Piagetian stage. Combinatory manipulations include more complex actions and contexts than reaching in that the dynamic field model can be applied and tested in the tasks of combinatory manipulations in both chimpanzee and human infants.

I expect that the delay in acquisition of combinatory manipulation by chimpanzee infants, as compared to human infants, can be understood as a reflection of the history of dynamic interaction among morphological, postural, locomotor, manual, and perceptual development in their social and physical environment. Chimpanzees can provide us with abundant useful behaviors for comparing behavioral deve1opment to deepen understanding of the ontogeny and phylogeny of embodied cognition.

## Next step, synergetics?

Wolfgang Tschacher and Ulrich M. Junghan

*Department of Social and Community Psychiatry, University of Bern,CH-3010 Bern, Switzerland.* **{tschacher; junghan}@spk.unibe.ch**
**www.upd.unibe.ch/**

**Abstract:** Thelen et al. offer an inspiring behavior-based theory of a longstanding cognitive problem. They demonstrate how joining traditions, old (the Gestaltist field theory) and new (dynamical systems theory) may open up the path towards embodied cognition. We discuss possible next steps. Self-organization theory (synergetics) could be used to address the formation of gaze/reach attractors and their optimality, given environmental control parameters. Finally, some clinical applications of the field model are advocated.

Thelen et al. do a very nice job in removing cognitivist terminology from their account of the A-not-B task. Doing this, they can substitute for the intentionalistic connotations of concepts such as "knowledge," or "object concept," their own behavior-based theory. We recognize three main ingredients in their approach: (1) A dynamical systems reformulation (2) of a behaviorist view with the Gestalt inspired addition (3) of Köhler's (1920) field theory and Lewin's (1936) topological psychology. In this way Thelen et al. create interesting links between today's dynamical science and the scientific psychology of the time before the cognitivist era, thereby pointing out a path to a future embodied and situated cognition.

***Attractors.*** Thelen et al.'s models rest on the generation of attractors in a potential landscape, the dynamic field that defines the resultant motor activations of the infant. Seen from the perspective of synergetics (Haken 2000), this kind of modeling is equivalent to macroscopic or phenomenological synergetics. The observable gaze/reach attractors are described together with the constraints given by environmental control parameters (the various inputs of the A-not-B system). Thus, in the dynamic motor field, the spatial (cues and tasks) and temporal (learning history) environments of the infant become integrated, that is, the spatial properties are "represented" and the temporal history is "stored" (these terms are put in quotes because the quality of "representation" and "memory storage" here clearly differs from the traditional usage in the symbolic information processing view). The A-not-B error enters the scene as soon as a phase transition from one attractor (reaching/gazing towards A) to the other (reaching/gazing toward B) is afforded by the task environment. Younger children tend to suffer more from the hysteresis effect, in that they perseverate to explore A even though they "should" rather be motivated to explore B.

We found the model presented in the article impressive, especially its capacity to provide new research hypotheses and predictions. Maybe this heuristic capacity is generated mainly by the strict formality of modeling which forces one to consider all variables of the task situation. Apart from this, the model does not go far beyond the phenomenology of the task. The field seems to basically map all input; it is actually the self-excitation of the medium (the cooperativity between adjacent sites in the field) that makes the difference between generating either the error ($h$ is low; little


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

cooperativity) or the mature response ($h$ is large). One problem then comes to mind, namely, our (perseverative?) concern about the explanatory status of this simulation model (and many other simulation models throughout psychology): Is this model a descriptive instrument helping to come to grips with the phenomenology of the A-not-B situation, or is it more than this, an explanatory model of the A-not-B task and similar problems?

***Complexity.*** What would be the properties of a complete (explanatory) model? Thelen et al. have proposed a model that nicely accounts for the continuous aspects of A-not-B decisions. The functioning of the model has the desired analogic, "seamless" nature, mirroring both the blurred dividing lines between perception, memory, and action and the gradual changes in cognitive development. What else is needed, now that this psychological theory has been launched from a dynamical foundation? We see a quite general goal lying ahead – in terms of the A-not-B task, one may ask, what are the origins of the point attractors at A and B? The dynamic motor field is a low-dimensional mapping of a high-dimensional (i.e., complex) problem. The number of degrees of freedom of any acting infant is obviously much larger than just reaching and looking towards A or B. A profound reduction of this complexity must have taken place before, enabling the infant to exhibit just the two highly coordinated motor actions. Some organization or, to avoid the obvious pitfall of a homunculus, self-organization must have occurred in the high-dimensional phase space of the A-not-B system before we can even think of a conceptualization at the level of the motor field. Thus, a further realm within dynamical systems theory becomes instrumental to address the collapse of complexity prior to modeling at the (macroscopic) level of attractors. If we use complexity theory or synergetios (Haken et al. 1985), an understanding of the optimality of movement coordination may come within reach. Formu1ating optimality in the framework of self-organization theory may explain cognitive intentionality, complementing the macroscopic field theory offered by Thelen et al. From a psychological standpoint, this would address the motivational questions concerning the valences active in the A-not-B task (why "hiding" a "toy" arouses the child's "curiosity" and makes him "want" to explore the wells at A or B). But to date, this theoretical formulation (and empirical validation) of a concept of optimality is still in its beginnings (Swenson & Turvey 1991; Tschacher 1997; Tschacher & Dauwalder 1999).

***Applications.*** Given the heuristic value of the model presented, which applications to other fields of cognitive science and psychology may ensue? Once one has introspectively encountered the eerie flavor of dissociative experiences (such as looking for things in all the places you "know" are the wrong ones), one may suspect that A-not-B situations are not at all rare in everyday life. It should be fun to pursue this more closely, but applications to psychotherapy and to the treatment of chronic mentally disordered individuals might be even more worth the effort. Some individuals with chronic schizophrenia, for example, suffer from massive perseveration problems in their daily lives. Viewed from the very down-to-earth perspective of community psychiatry, the problem is often to design environments for living, working, and recreation that help to maintain persons with mental disorders outside the psychiatric hospital. The task for an ecological psychologist or environmental designer in this respect is to disambiguate cues for action by creating heterogeneous fields and thus allowing for larger delays between intention and action. Further strategies might be to increase target distinctiveness, and allocate salience/valence to items in the living and working environments (e.g., Velligan & Bow-Thomas 2000). The complementary task of the behavior therapist would be to train cognitive differentiation and consider the impact of body-memory, and maybe even to find routes towards enhancing cooperativity. Pharmacotherapeutic approaches may exist that have an impact on the excitability of the neural medium. The open questions abound, as always, but we envisage a possible benefit from a step-by-step translation of Thelen et al.'s field model into clinical hypotheses.

## The social dynamics of embodied cognition

S. Stavros Valenti[a] and Thomas A. Stoffregen[b]

[a]*Department of Psychology, Hofstra University, Hempstead, NY 11549;* [b]*Department of Psychology, University of Cincinnati, Cincinnati, OH 45221–0376.* **psyssv@hofstra.edu** **stoffrta@email.uc.edu**

**www.uc.edu/~stoffrta/psl/index.html**

**Abstract:** Reaching in the A-not-B situation is not the product of a single person, but rather of a person-person system. We argue that models of embodied cognition distributed over persons may be necessary to capture the essential qualities of evolving behaviors, even those as simple as perseverative reaching.

Thelen, Schöner, Scheier, and Smith approach the deep problem of the emergence of infant cognition in a principled and coherent manner, employing the theory and tools of dynamic systems analysis. We whole-heartedly agree with this approach, and with the need for theories of embodied cognition. Our concern is that models that incorporate only the dynamics of the infant may be qualitatively unable to capture the essence of reaching in the A-not-B situation.

Students of the A-not-B situation tend to assume that the behavior results from processes within the child. Thelen et al. propose that reaching emerges from the dynamics of the body's nervous system situated in a task. However, reaching to A in the A-not-B situation is not the product of a single person. It occurs solely in the context of a person-person dyad. The data reviewed by Thelen et al. make this clear (sect. 2.2.1). For example, infants must be trained to exhibit the characteristic reaching patterns and even after training, the adult must provide cues. In addition, aspects of the experimenter's gaze in coordination with the infant's may influence how the infant scans the scene and how reaching evolves over time (this could be tested). Finally, the "error" occurs even when there is no object, that is, when there is solely the social interaction between the child and the adult (Smith et al. 1999b). We interpret this as indicating that A-not-B reaching is less about the recovery of objects and more about the social interaction between the child and the adult. This might be tested by having the objects be presented mechanically, rather than by an adult.

The model of Thelen of al. succeeds in part by making a number of assumptions about how the nervous system remembers and plans actions. However, there is ample evidence from a range of studies that at least some of the memory and planning in a developing behavior are distributed across agents. This idea is captured nicely in developmental research on "apprenticeships in thinking" (e.g., Rogoff 1990), and in analyses of distributed cognition and representation in social groups (e.g., Hutchins 1995). For this reason, we think it would be a boon to dynamical models of infant cognition to include social processes, even at the most basic levels of movement and gaze coordination.

Models need not be limited to the dynamics of individuals (Baron et al. 1994). Some researchers have begun to study the dynamic basis of social coordination. For example, Schmidt et al. (1990) showed that the dynamics of limb coordination between two people are similar to those found within a single person. This is true even when dyadic coordination is unintentional (Schmidt & O'Brien 1997), as in the A-not-B situation. The analyses in these studies make minimal assumptions about neural organization, planning, and so on: Coordination is dependent primarily upon perceptual information.

One social task in infancy that we think could profitably be modeled this way is nursing. Kaye and colleagues (e.g., Kaye 1982; Kaye & Wells 1980) have found coordination between sucking by the infant and jiggling of the baby by the mother. This person-person coordination seems to constitute a coadaptation that yields more efficient nursing. A model of nursing should show how this novel behavioral form emerges from the coordinated dynamics of infant and mother.



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

The emergence of novel behavioral forms in social interactions can be seen in research that does not use the formalism of dynamic systems modeling. Consider the work of Dunn and Shatz (1989) on how 3-year-olds learn to interrupt conversations. At this age children show major development in understanding and acting on the intentions, thoughts, and feelings of others. To examine the emergence of this competence, Dunn and Shatz coded child interruptions over the course of the third year. Over time, more new and relevant interruptions occurred, when the younger child was the non-addressed referent of the previous turn. What accounts for this developmental change? Surely, no one is consciously training the children to intrude properly. But these children have something in common with their older siblings and mothers: They all attend and respond selectively to novelty. When the young child intrudes with new and relevant information, the likelihood of being responded to is extremely high, at least 70%. We believe that a relatively constant attention of all speakers to new and relevant information provides support for the growth of socially appropriate patterns of intrusion. Behavioral novelty in this case can be explained economically as an epigenetic outcome of social coordination. In principle, this three-person interaction could be modeled dynamically, without reference to the nervous system of any of the individuals.

We conclude that behavior in the A-not-B situation emerges from the dynamics of person-person coordination, and not from the dynamics of any one person. The neural dynamics that might explain intra-personal coordination cannot explain inter-personal coordination, insofar as the latter relies on informational dynamics (Schmidt et al. 1990). Thus, there is a strong chance that the model proposed by Thelen et al. cannot explain behavior in the A-not-B situation. The dependence of social coordination upon informational dynamics may mean that neural dynamics models of individuals cannot be used as building blocks for models of person-person dynamics. If so, it will be necessary to begin directly with modeling of the latter. Thelen and Smith (1994, p. 327) acknowledged that human behavior is fundamentally social, taking the form of social interactions. Embodied cognition is an important concept, but it should not be assumed that cognition is embodied in individual nervous systems.

## Do adults make A-not-B errors in pointing?

Philippe Vindras[a] and Edouard Gentaz[b]

[a]*Faculty of Psychology and Educational Sciences, University of Geneva, 1205 Geneva, Switzerland;* [b]*Cognition and Development Laboratory UMR 8605 CNRS, University René-Descartes, Paris V 92774 Boulogne-Billancourt Cedex, France.* **philippe.vindras@pse.unige.ch**
**gentaz@psycho.univ-paris5.fr**

**Abstract:** We discuss the assumptions put forward by Thelen et al. about motor planning processes. We examine the results of an experiment inspired by the authors' contention that the motor plans of both infants and adults are continuous and graded. We wondered whether adults, in an adapted version of the A-not-B error paradigm, would point between the A and B targets, as in some degraded conditions of pointing (Ghez et al. 1990), or would make A-not-B errors. Unexpectedly, we observed that adults tended to shift the direction of their pointings to B away from A, and did not make any A-not-B errors.

In the target article, Thelen and her colleagues put forward an impressive formalization of a complex set of experimental data. This achievement rests on three bases (i) starting from a thorough analysis of the infants' A-not-B errors, (ii) they set out some experimentally-based assumptions about reaching, and, finally, (iii) evaluate a dynamic model built on these foundations. In this commentary, we will focus on the second step by discussing or testing three of the authors' contentions concerning motor planning in adult humans: (1) The hypothesis that actions are planned in movement parameters, which legitimate the use of the direction parameter in the model; (2) the hypothesis that plans are continuous and graded in nature, which is the basis for the continuous changes of the dynamic fields; and (3) the assumption that plans evolve under continuous perceptual specification of task and cue, which legitimate the crucial role of the motor memory in their model. Concerning the first one, we will remark only that the hypothesis is still a matter of debate (e.g., Desmurget & Prablanc 1997; Desmurget et al. 1997; Shadmehr 1993). We will mainly concentrate on the second hypothesis by presenting and discussing the results of an original exploratory experiment inspired by the target article authors' contention that "the processes that create perseverative responding in infancy are not special, but are the very same processes that lead . . . to correct and perseverative responding at any age" (sect. 3.2.6). Finally, we will comment on the third assumption by evoking some supporting experiments.

The main point we want to focus on is a potential inconsistency between the model of target article and the works of Ghez's team (Ghez et al. 1990; Henning et al. 1988b), which are widely quoted by Thelen et al. in support of the hypothesis that plans are continuous and graded in nature. On the one hand, the gist of the proposed model is to generate a spatial discontinuity between the A and B targets. It must be explained why infants reach toward the A or B target, and sometimes do not make any movement at all, but *never* reach to a point *between* A and B. On the other hand, Ghez and his colleagues demonstrated that adult subjects point to close targets by continuously specifying the direction and amplitude of their movements from a default value, which reflects subject's prior experience. They showed that when subjects had to produce their responses as soon as possible, or within a very short delay after the target was displayed, the produced movements were closer to the default value than when they pointed with a normal reaction time. Plainly, Ghez and his colleagues observed movements that consistently reached to points *between* the A and B targets. Using the analogic language of dynamics, their observations suggest that, during movement preparation, the locus of the maximal activation of the dynamic field would migrate from a mean default direction to the target direction. By contrast, the target article (Fig. 6) suggests that the dynamic field would only have two local maxima rigidly linked to the directions of the two targets.

This contrast could be owing to the subjects, who are adults in the experiments carried out by Ghez and his colleagues. Alternatively, in line with the contentions of the target article that the same motor assumptions should hold for infants and adults, the difference between the observations of the two teams could be ascribed to the discrepancies of the experimental conditions. Ghez and his colleagues have evidenced a tendency to point towards a mean default target by using various paradigms reducing the reaction time (Ghez et al. 1990; 1997; Henning et al. 1988b). By contrast, as underlined in the target article, the A-not-B errors increase with the delay between the target presentation and the movement outset and decrease when the B target is more attractive or more distinct from the A target. In sum, from Thelen et al.'s point of view, if the same motor assumptions hold for infants and adults, the later ones should also make A-not-B errors – and point either to the A or to the B target – when they have to do their movements in degraded conditions similar to that used in the infants' paradigm. Alternatively, the observations of Ghez and his colleagues suggest that, in such degraded conditions, adults should not make any A-not-B errors and that their pointings could be systematically biased towards a default direction close to the more frequent target. To test this alternative, we carried out an experiment in which adults pointed to targets A and B with a paradigm adapted from that used for the infants: The target A was five times more frequent than the target B; the two targets were similar and close together; the target B was occasionally made less attractive by being presented very briefly; the subjects had to wait 0, 3, or 6 seconds when the target B was presented. The results were quite unexpected: In these degraded conditions, the pointings to B tended to be biased *away* from A.


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

Thelen et al. stress the importance of apparently negligible details of experimental conditions. Consequently, we felt obliged to describe ours as precisely as possible. In darkness, twelve adult right-handed subjects were required to point on a digitizing table from a single starting point, located in their sagittal plan at about 25 cm from their trunk, to two targets A and B that were 20 cm further away and 2 cm left or right of the sagittal direction. Thus, the target directions were 11.4 deg. apart, with respect to the starting point, and about 3.4 deg. apart, with respect to subjects' eyes. Before the experiment, the subjects carried out 6 to 8 familiarizing trials comprising more pointings to A than to B. Then, they made 6 groups of 6 pointings (trials 1 to 36). In each group, the first five pointings were directed at target A, and the last one at target B. Finally they made 6 control pointings to B (trials 37 to 42). The 6 last pointings to B were done in the same conditions than all those to A: A laser spot indicated the target during 500 msec. The subjects pointed when they heard a sound stimulus, immediately after the spot disappeared. By means of a half-reflecting mirror, the subjects could not see their hands when they pointed, but could see it between trials (see Vindras et al. 1998 for a precise description of the apparatus and a similar procedure). The conditions of the six first pointings to B (trials 6, 12, 18, 24, 30, and 36) differed in two ways: The duration of the target presentation was either shorter (100 msec for half of the subjects, or 150 msec for the others) or longer (1,000 msec for all subjects); the sound stimulus which served as GO signal was presented immediately after the spot disappeared ("delay 0"), or after 3 seconds ("delay 3"), or after 6 seconds ("delay 6"). Every subject pointed in these 6 degraded conditions (2 duration × 3 delays). Their order was balanced between subjects, as well as the side of the target A and the value of the shorter duration. As targets A and B were both at 201 mm from the starting point, we only analyzed the influence of the duration and delay factors on movement direction, which was measured clockwise from sagittal direction. The results of a typical subject are displayed in Figure 1.

As most others, its pointings were tilted to the left (mean direction for all subjects: (−4.4 deg.). More important, its 6 first pointings to B (degraded conditions) were slightly more distant from the pointings to A (all subjects mean: 13.0 deg.) than the last pointings to B (control condition: 12.6 deg.). Besides, in keeping with one of the assumptions of Thelen and her colleagues, it can be seen that the direction of the pointing to A (upper empty triangles and curved line) continuously evolved throughout the experiment. A

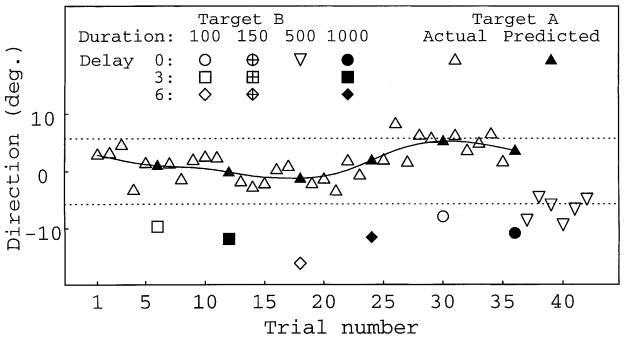



Figure 1 (Vindras & Gentaz). Raw results of one subject pointing to a right-hand target A (upper triangles) and a left-hand target B. The specific experimental conditions of each pointing to B (e.g., a 6 sec delay and a 100 msec target duration) is indicated by one of the symbols shown in the upper part of the panel (e.g., empty diamond for a pointing directed at target B with a duration of 100 msec and a delay of 0 sec). The direction of each target is shown by a dotted line. Smooth splines were used to underline the continuous evolution of the direction of the pointings to A (curved line) and estimate its value when the subject actually pointed to B (black triangles).

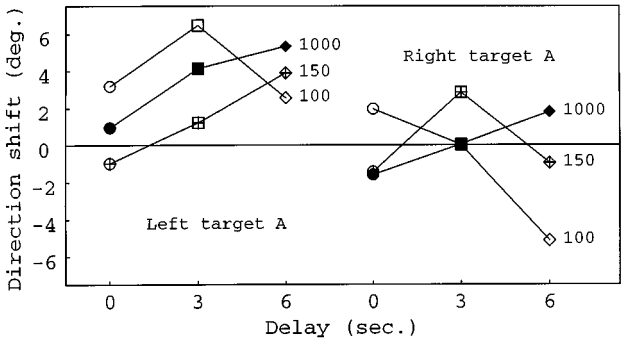



Figure 2 (Vindras & Gentaz). Averaged differences between the mean direction of the control pointings to B (delay 0 sec; duration 500 msec; righwards empty triangle in Fig. 1) and the directions of the pointings to B for three delays before the GO signal (abscissa) and three target durations (same symbols as in Fig. 1). The values of the direction of all pointings to B were first computed with respect to the estimate of the directions of simultaneous pontings to A (black triangles in Fig. 1). A positive direction indicate a rightwards shift of the direction of the first pointings to B with respect to the control trials, that is, a shift away from a left target A (left part of the panel) or towards a right target A.

significant linear trend ($p < .05$) was found for half of the subjects, and most of the others displayed consistent non-linear variations of direction. Despite this variability, an analysis of variance performed on the raw data showed that the quantitative delay factor was significant ($F(1,68) = 7.83$, $p < .01$). When the delay increased, the final positions associated to the target B moved away from the target A (when A was on the right side, the direction of the B pointing were multiplied by −1). To ascertain this finding, we used smooth splines with 6 degrees of freedom (curved line in Fig. 1; see Vindras & Viviani 1998) to estimate the directions of potential pointings to A (black triangles) when subjects actually pointed to B. We then subtract from these data the difference between the last estimated direction of the pointing to A and the mean direction of the 6 control pointings to B (trials 37 to 42).

When target A was on the left side, in all conditions but one with delay 0, the mean direction of the first pointings to B was more rightwards than the six control pointings. When target A was on the right side, the results were much less clear, but in the worst condition (delay 6 sec and target duration limited to 100 msec), the mean direction of the first pointings to B was 5.1 degrees more leftwards than that of the control pointings. Note however that the mean shift by subject was not significantly different from 0 ($t(11) = 1.93$, $p = .080$). A post hoc analysis showed that the shift was significant only when A was on the left side and when the delay was maximal ($t(5) = 3.12$, $p = .026$). Thus, the analysis of smoothed data partially confirmed the results obtained with raw data. It seems clear that the direction of the pointings to the less frequent target (B) tended to move away from the direction of the pointings to the most frequent target (A). However, further experiments would be needed to confirm that this shift increase, when the delay increases, or when target duration decreases or when target A is on the left side.

What do these results mean? At first sight, they seem to reject both of the two initial hypotheses. However, a closer examination suggests that they are more compatible with the Thelen et al.'s model than with the idea of an incomplete continuous specification of movement direction (Ghez et al. 1990). As for the second hypothesis, the difference between target directions is small enough for the movement direction to be continuously specified from a central default (Ghez et al. 1997). However, the mean difference between the directions of the 6 first pointings to B and the estimated directions of the pointings to A was larger (12.8 deg.) than the difference between target directions (11.4 deg.). To ex-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

plain why the postulated continuous specification of the direction should have reached beyond its target, we could only tentatively suggest a cognitive effect similar to that found in other studies (e.g., Gentilucci et al. 2000; Rossetti & Regnier 1995). As for Thelen et al.'s model, it is clear that our experiment did not provide a single instance of A-not-B error: The differences between the observed and estimated directions of the pointings to A and B ranged from −22.2 to −4.1 deg. when A was rightwards, and from 6.7 to 20.9 deg. when A was leftwards. However, it is possible that such errors could be found in adult subjects with longer delays, closer targets or shorter target duration. Moreover, the finding that the final positions of the pointing to B moved away from A could probably be easily integrated in Thelen et al.'s model as the effect of a slight inhibition surrounding the activation centered on A. Thus, the results of our exploratory experiment do not provide a direct support to the contentions of the target article – that is, A-not-B errors in adult subjects – but rather underlines the relevance of its formalization for the research in motor control.

In the same line of thinking, the present experiment and some others carried out recently provide strong supports to another Thelen et al.'s contention, namely that "plans evolve under continuous perceptual specification of task and cue." In the experiment presented above, 11 out of the 12 subjects displayed a significant linear trend for the amplitude and/or the direction of their pointings to A. Moreover, the eight significant linear trends of movement amplitude were all associated to a positive slope, that is, movement amplitude increased during the experiment. This observation was in line with the results of a previous experiment (Vindras & Viviani 1998). The reason for these consistent trends is not yet clear, but they could be related to the lack of visual information about the position of the pointing hand. This hypothesis is directly supported in a recent paper showing that pointing errors reflect biases in the perception of the initial hand position (Vindras et al. 1998). In this experiment, subjects were required both to point at various targets, and to localize their unseen hand on the starting positions by means of a laser spot which could be moved with a joystick. The amplitude and direction of the mean pointing errors were negatively correlated with the same two parameters of the localization errors, as if subjects computed the hand-target vector on the basis of a hand position close to their erroneous localization of the same hand on the same starting point. These experiments, as well as others (Ghilardi et al. 1995) stressed the high variability of the visuo-motor transformations required for pointing or reaching. The analogic language of dynamics emphasized in the target article appears to be a promising mathematical approach to model shifting interactions between the visual, the proprioceptive and the motor systems.

ACKNOWLEDGMENTS

This work was supported by grants from the University of Geneva and Fonds National Suisse (No. 31–5562098). We thank Prof. Paolo Viviani for his helpful critique of an earlier version of this commentary.

# Authors' Response

## So what's a modeler to do?

Esther Thelen,[a] Gregor Schöner,[b] Christian Scheier,[c] and Linda B. Smith[a]

[a]*Department of Psychology and Program in Cognitive Science, Indiana University, Bloomington, IN 47405;* [b]*Centre de Recherche en Neurosciences Cognitives, C.N.R.S., Marseille, Cedex 20, France;* [c]*Department of Biology, California Institute of Technology, Pasadena, CA 91125.*

**{thelene; smith4} @indiana.edu** **gregor@lnf.cnrs-mrs.fr**
**scheier@neuro.caltech.edu** **www.php.indiana.~gormlef.edu**

**Abstract:** We argue that mentalistic constructs like the "object concept" are not substitutes for process explanations of cognition, and that it is impossible to prove the existence of such constructs with behavioral tasks. We defend the field theory as an appropriate level for modeling embodiment. Finally, we discuss the model's biological plausibility and its extensions to other tasks and other species.

We are gratified that our target article elicited interest and comments from colleagues in so many different disciplines, including developmental psychology, cognitive science, philosophy, motor control, animal behavior, and neuroscience. We believe that this attests to the power of dynamic models to unite disparate levels of explanation, and ultimately, provide the integration of brain and behavior that does justice to real behavior in real organisms.

We are gratified, but also challenged, to respond to these wide-ranging remarks. We have organized our response to deal first with the more abstract conceptual issues raised by our target article. Then we discuss issues of implementation of the model in brain and body.

### R1. Concepts, object concepts, conscious concepts

#### *R1.1. The ghost of Piaget.*

Piaget's theory is richly layered, deep, and often dense. He produced a voluminous body of work, ranging widely through epistemology, logic, psychology, biology, evolution, and history of science. Within pages, his writing could shift from the most exquisitely detailed descriptions of infants and children ever written to highly abstract and formal logical structures. As is true of every great philosopher and theorist, Piaget has been continually interpreted, reinterpreted, and misinterpreted, each reader able to find within Piaget's corpus ample material to accept or reject.

The debate continues. Several generations of developmental psychologists can credit (or blame!) Piaget for discovering the A-not-B error and for weighting it with its epistemological burden. Is our dynamic model yet another violation of Piaget? Would he love it or hate it? Our commentators do not agree. **Staddon et al.** and **McCune** place us squarely in the tradition of Piaget. **Pelphrey & Reznick** disagree, believing our account is irrelevant to Piaget's questions. **Müller & Carpendale, Michel**, and **Sophian** state that our account is Piagetian in some respects, but critically different in other ways.

This debate over the "real" Piaget and his "true" legacy stems, in part, from an inherent ambiguity in Piaget's the-



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

ory. Consider the central issue raised by the commentators. **Pelphrey & Reznick**, **Michel**, and **Sophian** believe that there is a point in development when infants "have" an object concept, and that this concept really motivates behavior. "A person with no implicit belief in the permanence of objects," say **Pelphrey & Reznick**, "would have no reason for searching for missing keys." **Marcovitch & Zelazo** likewise state "A toddler who consciously represents the object at location 'B' and searches there despite an extensive history of searching at location 'A,' does so because he knows the location of the object . . . and this knowledge has consequences for behavior." Once the child has this belief, representation, concept, or knowledge of the constancy of objects, he or she cannot be fooled, but is sometimes "distracted." As several commentators mentioned, adults sometimes make perseverative errors, too, and for the same reasons: momentary distraction in the face of an unshakable belief that solid objects like keys still exist when they are out of sight.

This is a strong interpretation of Piaget's idea that this ability to represent hidden objects is a "qualitatively different" form of knowing than the sensorimotor intelligence of infants. If this is true, then our model either ignores or is indifferent to Piaget's real question. However adequate our sensorimotor explanation of the task, the argument goes, we only sidestep the real issue of when (and how) children acquire the object concept.

**Staddon et al., Müller & Carpendale,** and **McCune** read Piaget differently. According to them, Piaget never meant the mental structure of object permanence to have any causal role in behavior. Rather, "the object concept does not refer to an entity that is separate from, and causally related to, the child's acts, but to formal aspects of these acts," and that to have a concept means "to act and operate on reality in distinct and organized ways" (**Staddon et al.**). Thus, according to **Staddon et al.** Piaget did not want to know when infants gained an object concept, only how. **Müller & Carpendale** echo this point, claiming that the formally defined structures have reality only as they are embodied in activities. Nonetheless, they maintain we have ignored the issue of when or how infants gain an objective universe, one populated with permanent objects and realistic notions of time, space, and causality. **McCune** also points out our similarities with Piaget's sensorimotor origins of cognition, but for her, the critical transition is to " representational consciousness."

In sum, there is an active dispute over Piaget's meaning of a mental structure, well summarized by the following quote from Chapman (1988):

> Piaget's use of the term "structure" is characterized by a certain ambiguity. On the one hand "structure" may refer to the formal properties of a certain type of thinking. In this sense, concrete operational thinking is said to be characterized by the grouping structure. On the other hand, Piaget believed that cognitive structures of this kind possess a functional reality. They do not exist merely as a formal description in the mind of the observer, but as he once put it, they are "causally active" in the mind of the subject. (Piaget 1941, p. 217; Chapman 1988, p. 343)

But let us explore more fully the implications of either reading of Piaget. What does it mean to say that a concept has causal reality? Conversely, what is the real use of a mental structure with only formal properties? How can we tell?

### *R1.2. My concept made me do it*

The first interpretation is that at some point in development behavior is differently motivated. It ceases being sensorimotor and instead is driven by new, higher-level structures such as "conscious representation," or "representational consciousness," or "transcendent knowledge," or "intentionality." Thus, when infants are right on the task, their behavior is dominated by the higher-level structure, but when they are incorrect, they are working in another mode. There are several problems with this view.

**R1.2.1. Labels do not constitute an explanation.** The main difficulty here is that when the commentators give these putative mental structures a name, we believe they have substituted ill-defined constructs for explanation. In Table R1, we have compiled a list of terms used by the commentators in the context of their discussion of the deficiencies of the field model. On the surface, these phrases sound loaded with psychological meaning. But we find them unsatisfactory because they are underspecified and have little explanatory power.

For example, in the model of **Marcovitch & Zelazo,** the "conscious representational system" comes on line when the infant "becomes increasingly aware of his or her own conscious states," such that this system can deliberately override motor habits. What constitutes being aware of one's own conscious states? This and the other undefined constructs are invoked as explanations. Infants solve A-not-B when they "get" the "object concept" or "intentionality" or "representational intelligence" when they did not have it before.

The core issue is that these terms by themselves do not offer us any help in understanding the mechanisms and

*Table R1. "Psychological" constructs with underspecified meaning*

| Construct | Commentators |
|---|---|
| Conceptual understanding | Marcovitch & Zelazo |
| Consciousness | Müller & Carpendale |
| Conscious representation | Marcovitch & Zelazo |
| Representational consciousness | McCune |
| Representational intelligence | Sophian |
| Internal mental representation | McCune |
| Knowledge | Marcovitch & Zelazo |
| Conceptual knowledge | Marcovitch & Zelazo |
| Explicit knowledge | Marcovitch & Zelazo |
| Implicit knowledge | Ruffman |
| Core knowledge | Ruffman |
| Transcendent knowledge | Pelphrey & Reznick |
| Knowledge structure | Sophian; Pelphrey & Reznick |
| Object concept/permanence/ knowledge | Marcovitch & Zelazo; Ruffman Staddon et al.; Pelphrey & Reznick; Michel |
| Structure | Müller & Carpendale |
| Objectivity | Müller & Carpendale |
| Intentionality | Müller & Carpendale |
| Subpersonal level of explanantion | Müller & Carpendale |
| Psychological level of explanantion | Müller & Carpendale; Markman |
| Plausible psychological content | Pelphrey & Reznick |
| Central cognitive parameters | Lewis |



https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

processes involved in succeeding or failing at the A-not-B, nor do they have what it takes to become intentional or representational or knowledgeable. Moreover, there is a gap between invoking such constructs and specifying how they actually operate to motivate real life behavior. For instance, do infants really shift modes in mid-task: using intention, consciousness, and knowledge on the A trials, but sensorimotor structures on the B trials? How can a baby have "transcendent knowledge," but have it fail to be activated strongly enough to motivate behavior at one moment but not the next? What do the incomplete states of these constructs look like and what turns them on and off?

The problem, therefore, is how to get the constructs listed in Table 1 to do any work in real behavior. This is because these views are, at heart, profoundly dualistic, drawing a distinct line between behavior controlled by the construct (the real person) and that controlled by the mere bodily processes. This dualism is made especially explicit by **Müller & Carpendale** who call our explanation of the A-not-B error "subpersonal," as if the mechanisms of behavior cannot constitute a psychological explanation.

Lurking behind these comments, however, is the everyday feeling that we all know a lot of stuff, and that we use that knowledge, and at some point, so must babies. It is trivially obvious that we all develop sets of beliefs and notions based upon our experiences in life. But it is also not so obvious that our moment-to-moment behavior is dictated by our pre-existing beliefs. We may look for our car keys not based on abstract notions about object permanence but because we need to drive to the store, and we remember that we have keys in the house which are intimately tied to our experiences of the feel of keys in the ignition, starting the car, and driving away. At that moment, we may be as much motivated by hunger or obligation as by the abstract properties of objects. This is exactly the point of the field model: to show that internal states and external cues interact in a continual shifting balance.

And, so where are "representations" in all this? As **Newton** cogently argues, representations may be more than the shapes of the traditional computational paradigm. Representations in their strongest, original, and most meaningful sense are symbols that stand for what is represented and are distinct from the computational processes that operate on them. By this original definition, sensorimotor processes are decidedly not representations. More recently, however, the range of internal events considered to be representations has expanded. In this newer view, any dynamic internal event that is causally related to behavior is a representation. This is fine by us. If we all agree that there is only process, and if we all reject the dualist partition of knowledge distinct from process, then we are happily representationalists. But notice such a move takes all meaning from the term: a hurting knee becomes a representation of the fall that gave rise to it. It hardly seems worthwhile to ask whether a theory posits representations or not.

**R1.2.2. Formal structures are not explanations either.** The alternative position taken by **Staddon et al., Müller & Carpendale,** and **McCune** is that Piaget did not mean to assign causal force to his mental structures. Rather, they maintain, he was after a formal description of behavior. Thus, they say, Piaget did not advocate disembodied constructs, as mental structures are only a reality as they are applied to action.

Our treatment of Piaget indeed did not do justice to the dynamic, embodied, and emergent nature of his monumental theory, nor did we make clear enough the parallels between the dynamic approach and the essential Piaget. Indeed, toward the end of his career, Piaget himself became much intrigued with the parallels between theories of complexity and change in biological organisms (Chapman 1988; Piaget 1985).

But if Piaget meant for mental structures to be only formal descriptions, our foundational issue remains: where are the mechanisms that produce behavior? What does it mean to say that an infant is in "Stage IV" of object permanence? Is this a description of the statistical likelihood of acting in a particular way? In reality, this type of formal description does no more work than a causally active mental structure in accounting for both the regularities in behavior and the times and circumstances in which the expected responses do not occur. In either case, we do not have an adequate theory of performance. Although Piaget did not concern himself with these dynamics directly, the conditions under which a concept or a formal structure does or does not contribute to behavior must be considered. What we have tried to do in this model is to show that the dynamics of mistakes, or absent-mindedness, are one and the same as those of "correct" responding.

### *R1.3. The quest for the holy task*

A related problem arises when either mental constructs or formal structures are reified with explanatory power: how to demonstrate that the infant or child really has it. The mental structure or belief can only be tapped in a particular task. What, then, is *the* task that reveals the underlying knowledge? Who gets to choose? For instance, **Ruffman, McDonough,** and **Pelphrey & Reznick** all describe tasks designed to assess object permanence. **Ruffman** found that the same infants "knew" about the location of hidden objects when tested in a visual violation of expectation experiment at delays where they failed a manual search task. **McDonough** found that when no repetition or training was involved (just one trial) 7.5 month-old infants could remember the location of the hidden toy for 90 seconds, and search for it, a delay far longer than in traditional measures (e.g., Diamond 1985). **Pelphrey & Reznick** mention an experiment where after reaching incorrectly to one of the three wells, infants were able to reach correctly on a second try.

Who is right about object permanence? As we said in the target article, the question is ill-posed, because the answer is entirely constrained by the construction of the task and its evolution through time. This is true of Piaget's tasks as well, which are no more or less paradigmatic of the "real" object concept than anyone else's. **Michel** points out that Piaget was aware that perseveration could be only a habit. But in tasks that have a time dimension, how can Piaget (or anyone) say that the first or second wrong reach to A is motivated by an incomplete object concept, but the third, fourth, and fifth, and so on, are motor habits? These differences may be more in the mind of the experimenter than in the mind of the baby.

All of these experiments (and many others) are fascinating windows on infant visual attention (especially to events), motor processes, and memory. Moreover, we are confident in each case that the behavior could be adequately characterized by dynamics similar to those we used in the model


https://doi.org/10.1017/S0140525X01523912 Published online by Cambridge University Press

as events that capture visual attention, require a choice of where to reach and where to look, and interplay between the history of the system and the current input. We believe that such a level of explanation is sufficient. As researchers gather data from many situations where the parameters of the task are well-specified, we will accumulate a picture of how infants behave based on mechanisms not constructs. Our theoretical formulation, therefore, questions not just the A-not-B paradigm, but the issue of what empirical evidence is necessary and sufficient to demonstrate the existence of any presumed stable mental construct. The radical implication is that concepts do not exist isolated from the time- and context-dependent processes that produce behavior. Rather, knowledge is created in the moment of its manifestation.

#### *R1.4. Can dynamics do it?*

Despite our discomfort with the commentators' invocation of mental constructs, we recognize their collective concern that the model does not fully capture an important developmental transition. Something is manifestly different between adult and infant cognition: infants are more tied to their immediate perceptual world, while adults and older children can think about objects, people, and events when they are not present, mentally manipulate symbolic representations, and use previous experience to plan and decide in anticipation of action.

We believe that a form of the dynamic field model could indeed simulate these ontogenetic changes, and indeed take on the flavor of a more "cognitive model" as requested by **Markman** and **Lockman**. We wrote in the target article that one key developmental change was the ability to make decisions "off-line," not coupled to the immediacy of the environment, but using stored experience and planning in advance of acting. But the insight from the model is that these skills can develop from changes in one or all of the parameters. At the present time, we have insufficient data to choose among them.

1. Increasing the resting level $h$ is one mechanism that would act to prolong the stimulus in its absence and also contribute to the longer-term memory of the resulting action. One way this resting level might be raised is through more dense connectivity in the field resulting from overall experience-dependent strengthening. But experience might also strengthen the local excitatory interactions, leading to a sharpening of the kernel. In addition, the inhibitory connections might also be strengthened, producing the same result. Contrary to what **Lewis** assumed, the field does produce nonlinear outcomes, depending on the parameters and the connectivity. Continuous changes in the dynamics of the field produce discontinuities in function.

2. In the A-not-B simulations, we used the task input to reflect the layout of the targets, assuming that the immediate perceptual scene was sufficiently novel so as not to contain any information about the infants' history of viewing or acting in other, similar circumstances. But this is clearly an oversimplification. Objects and people are never neutral as to their possibilities: It is plausible that with repeated encounters with situations in everyday life, the task field becomes richly landscaped, acting to pre-shape the movement parameter field before the specific cue to act is added (Erlhagen & Schoner 2001). This comes close to **Glenberg et al.**'s suggestion for an affordance field. We would situate the affordances (the histories of perceiving and acting) in the task field to allow it to be integrated with a specific input.

3. Intentionality is a concern of **Latash, Marcovitch & Zelazo, Freeman,** and **Müller & Carpendale**. In the A-not-B simulation, the cue to act, the specific input, was externally generated. But the cue may be internal as well, elicited by the situation itself and/or by memories of similar situations. In this way, an intention is another input to the field and subject to the field dynamics. As intention to act may, for example, be swamped by a more potent new external cue, or a competing memory, giving rise to an episode of absent-mindedness. Likewise, we could imagine that with development, and the accumulation of a rich experiential history, the threshold for internally generated cues is lowered, and the intentional component becomes stronger and more likely to overcome immediate perception. The point is not to deny that behavior can be internally generated, but to constitute intentionality from the same cycle of perception-action-memory that generates persev-eration, inattention, and other realities of embodied mental life.

Clearly, more experimental work is crucial to elucidate the multiple developmental and task-related pathways through these transitions. The field theory provides a framework for conducting such experiments.

Indeed, **Spencer** provides an excellent example of how the field model can be extended to tasks with older children and adults. There are two important implications of this work. First, **Spencer** shows convincingly that A-not-B type behavior is not confined to infancy, but is part of the general processes of reaching to locations in space. Second, his metric data provide insights into possible mechanisms of developmental change. Third, he echoes our beliefs that in order to characterize what changes in development, we must understand how behavior evolved in the time scale of the task.

## R2. Embodied cognition and cognitive embodiment

Just as there is disagreement on whether the dynamic model is Piagetian or not, there is also some dispute on its degree of embodiment or disembodiment. **Markman, Lockman,** and **Berger** call for extensions of the model into more "cognitive" domains, while **Munakata et al., Mareschal,** and **Dounskaia & Stelmach** ask for more concrete embodiment in neurophysiology or biomechanics. We also consider the issues raised by **Freeman, Hailman, Lockman, McCollum,** and **Harter et al.** on the appropriate level of abstraction for modeling.

#### *R2.1. How cognitive is cognitive?*

Several commentators have suggested tasks that are more "cognitive" than the A-not-B as a challenge to the generality of the field model. For instance, **Berger** found infants perseverate in a difficult locomotor task even when their actual movements varied from trial to trial, suggesting that the memory – and the decision – may be a more general goal state than the particular movements. This is very similar to **Glenberg**'s invocation of an affordance field. In **Markman**'s example of consumer behavior, there is competition between the old buying habits and the sensory assault of the new packages on the shelves, with the resolution often in favor of the habit. For all of these cases, the underlying pro-



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cesses may well be captured by the dynamics of the neural field. Thus, while we have situated the A-not-B decision close to the sensorimotor surface, there is also no reason why a decision field could not be conceptualized more abstractly. The critical feature of the field is its metric properties that allow specification in terms of distance, interaction strength, and evolution over time of multiple inputs. We mentioned in the target article that the model lacks "what" information, clearly needed in a complex task like shopping. Such an extension of the model is a promising direction for the future.

The important point, however, is that at whatever the level of abstraction we conceptualize the inputs to the decision, the final evolution of that decision is at the sensorimotor surface if any action, including speech, will follow. The remembered and abstract inputs to the action must remain compatible with and *continuously coupled* to the motor decision field so that the inputs can be meshed together to specify the action. This is well-illustrated by **Lockman**'s suggestion to model imitation and pretend play. In both cases, children repeat actions they have seen, sometimes elaborating, combining, and embellishing them. In terms of the field model, some immediate cue – the actions, or the remembered activities, of other persons – would be the input to a motor planning field already pre-shaped by a history of similar, but perhaps not identical movements. The visual and/or internal cues could thus be combined in innovative and nonlinear ways with children's memories of possible movements such that in some cases the seen example would dominate and in others, children's associations with other related movements would also come into play. Thus, the ability to imitate would not be an all-or-none skill, but would depend on how similar the action to a remembered and performed one, how novel and salient the cue to remember the action, and so on.

Some interesting and testable hypotheses emerge from such an account: (1) Children cannot imitate movements that are truly novel, although they can combine already performed movements in novel ways. (2) Conversely, more familiar movements, such as "bye-bye" are more easily imitated. (3) The more novel and attention grabbing the model-to-be imitated, the more likely he or she will be imitated precisely. The specific input will dominate. (4) Conversely, when the stimulus for imitation or pretend play is only suggestive (for instance, providing objects like play telephones, dishes, tools, etc.), children's actions will be more combinatory and inventive, relying on stored memories of possible actions. In short, these cognitive activities remain a blend of competing and cooperative influences from the immediate situation and the history of acting in similar situations.

### *R2.2. How much body in embodied?*

Other commentators take us to task for not being embodied enough. First, it is important to stress again what we mean by embodiment. We have a theory of embodied behavior, where we consider the constraints of sensory and motor systems as well as the environmental and task conditions in which behavior emerges. This is different from a model of how the nervous system works or of the biomechanics of movement, although our model should be fully compatible with these mechanisms.

**Munakata et al.** and **Mareschal** ask us to provide a more neurally realistic model, claiming that our model is too abstract and therefore, under-constrained. We agree that constraints are a critical part of the modeling process. Under-constrained models can be too powerful, and thus difficult to evaluate empirically. Conversely, models with many free parameters require many, sometimes ad hoc assumptions. What are the appropriate constraints?

For example, **Mareschal** suggests we include connectivity constraints mirroring the dorsal versus ventral streams in the brain. However, this distinction is very general and in reality, the underlying neuroanatomical structures are extremely complex. (In his commentary **Cisek** sketches out this complexity.) In the dorsal stream, for instance, one would have to model the separate streams concerned with different classes of actions relevant to the A-not-B task such as area MIP (reaching), area AIP (grasping), and area LIP (looking), each of which has its own idiosyncratic representation of space and its own developmental pattern. As we documented in the target article, it is very difficult to disassociate the individual contributions of these and the other parallel and densely interconnected areas of the brain so that we can implement their actions in any well-specified way. We would also have to make untestable assumptions about changes in connectivity or the population characteristics of neurons to implement **Munakata et al.**'s suggestion. Thus, we think of the model as neurologically inspired and plausible, but not anatomically specific. Surely someday enough will be known about the neuroanatomical bases of these behaviors to use them as realistic constraints on the model, and we continue to welcome evidence in support of the model's biological plausibility. In the meantime, however, the model is very powerful at the behavioral level. We do not see much advantage in substituting parameters requiring more assumptions and less specificity at this point.

**R2.2.1. Neural networks to the rescue?** Both **Mareschal** and **Munakata et al.** have modeled infant object-related behavior with neural networks. Are neural networks inherently more "embodied"? First, our field dynamics is a neural net, and in particular, a type proposed by Amari as a model of cortex. Indeed, Amari was motivated by the homogeneity of cortical layers to formulate the network as a continuum of neurons. In fact, most other neural network models are a form of dynamic system and many well-known network architectures are cast in differential equations (Grossberg & Carpenter 1988). Neural networks per se are not more embodied than an equivalent dynamic systems mode.

Formulating a neural network model does not necessarily reveal more realistic constraints. This is because certain classes of neural net models are universal approximators and may approximate any input-output relationship (Hornick et al. 1989). The Amari dynamics are particularly appropriate for this task, we believe, because they capture the continuous and graded structure of A-not-B behavior, including metrics, local cooperation, and global competition/inhibition.

A second important issue is whether the "synaptic" learning mechanisms of connectionist networks are more "brain-like" than, for instance, the mechanisms that preshape the dynamic field. In reality, not much is known about the actual neural mechanisms at play in most tasks for which networks have been proposed. Note also that in the field model, the connectivity could, in principle, be elaborated in light of more extensive neurophysiological data.


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**R2.2.2. More on biological plausibility.** Given that the behavioral simulation alone is quite powerful, we were pleased that **Cisek** provided such a strong neurophysiological justification for our model. One notable feature of Cisek's "specification-selection" model is the borderless stream of processing where current information is continually tested against the history of potential actions. In particular, and in agreement with several experiments we cited in the target article, movement preparation does not emerge fully formed at the end of a series of "cognitive" decisions, but is progressively shaped and refined as the task parameters themselves become progressively refined. A second notable feature is that action decisions are not executed in one special part of the brain alone, but are the accumulated result of the cooperative dynamics of nearly the entire cortical processing stream as well as the basal ganglia. This idea is echoed by Leon and Shadlen (1998) in a review of the neurophysiology of decision-making.

> In fact, we know of no structure in the brain that contains an abstract representation of an interpretation or decision that is not either tied to an effector system or dependent on the continued presence of a sensory stimulus. Rather the decision process seems to emerge at the nexus of sensory and motor processing – where sensory data give rise to a plan to enact some particular behavior. (p. 669)

We believe that dynamics is the appropriate language to express this profound embodiment in both behavior and in neurophysiology.

**R2.2.3. Moving into motor control.** Another way that our model seems to be "under-embodied" is suggested by **Dounskaia & Stelmach**: our agnostic stance on the mechanisms of motor control that follow the location decision. We are keenly aware of this lack, and as in the case of the neuroanatomy, have chosen to stick with a well-specified behavioral model rather than stick our necks out with mechanisms that are not yet fully understood. Nonetheless, they point out the important role of infants' reaching skill in our account. Contrary to what **Diamond** asserts in her commentary, even at 12 months, infants are not fully skilled reachers (Thelen et al. 1996). Our data show that the period of 8–12 months is a transition between being really terrible from about 4–8 months and more adult-like reaching (Clearfield et al. 2000; Thelen et al. 1996). Indeed, in a longitudinal study, Clearfield et al. (2000) found that the emergence of consistent perseveration in the A-not-B task was associated with a dramatic improvement in reaching skills (at around 8 months). Remarkably, at 5 months, infants either refused to reach at all in the task, or were perfectly correct, reaching to A on the A trials and to B on the B trials, *even at a standard 3 second delay*. At 6 and 7 months, their reaches were random, and only at 8 months did they show true perseveration, that is, correct on A and wrong on the B trials. Clearfield et al. interpreted these results, consistent with the field model, as young infants simply not having enough control to produce repeatable movements for the motor memory to build.

It is highly plausible that some of infants' difficulties in motor control stem from their inability to stabilize the subordinate joint, the elbow, from the interactive torques generated by the primary joint, the shoulder. Indeed, recent evidence from a study by Galloway and Thelen (2000) showed that the shoulder joint was more stable throughout the first year, even in early, non-reaching movements. The elbow only gradually attained a smooth trajectory.

In addition to the possibility that infants need sufficient motor control to form stable memories, there is another way that the developmental status of reaching may contribute to perseveration. When the task is difficult, as we have argued, infants must focus their attention on the motor control so as to aim correctly toward the desired target. This heightened attention may also work to etch in the memory of the action, consistent also with what **Berger** reported on her locomotor task. Indeed, we have recently tested this possibility and found that when we called infants' attention to their reaching arms by having them wear gold lamé sleeves or pick up an unusually heavy object, they perseverated with fewer A trials than did control infants.

We appreciate **Latash**'s example of just this type of process in adults in an analogous situation, learning the difficult new skill of mirror writing. Of course, it is well-known that new skills require effortful attention. In the case of adults, effort is required not only to learn the new mappings, but also to compete with the existing, well-practiced habits acquired over a lifetime. Even if adults can maintain the cooperative mode, the model would predict a very strong preshape of the field based on long experience that would persist in the face of the new task demands. But again, we question Latash's need to invoke a separate process – intention – or instruction – that is over and above the parameters of the perception/action/memory dynamics.

We appreciate **Vindras & Gentaz**'s demonstration that, in adults, as in infants, the history of reaching has a profound effect in biasing the system on subsequent reaches. The exact nature of that bias, however, as they show, may evolve within the specifics of each task context. We described earlier how increasing infants' attention to their moving arms changes their tendencies to perseverate. Likewise, depriving people of vision of their hands may change the way movement history affects the current motor plan.

### *R2.3. So what is the right level of explanation?*

Too top down! (**Harter et al.; Mareschal; Munakata et al.; Roberts; Dounskaia & Stelmach; Tschacher & Junghan**)! Too bottom up! (**Michel; Ruffman; McCune; Marcovitch & Zelazo; Sophian; Müller & Carpendale; Lewis**). Just right! (**Hailman; Lockman; Newton**). What's a modeler to do?

We set out to model a well-researched behavioral phenomenon using abstract dynamics that, in relatively few parameters, captured the sum of the complex, time-and-age dependent processes that produced it. Regardless of the particular modeling architecture, which may be a matter of preference, we believe that the number one criterion by which to judge the success of the enterprise is the model's compatibility with a set of experimental facts, both to explain and predict. We are very happy with the model's strength in this regard, and especially in suggesting new experiments, which have only increased our confidence in the basic assumptions of the model.

At this point in the science of the human mind, the only access to its workings is through behavior. Scientists and philosophers seek to understand intentionality, consciousness, knowledge, and the nature of representation. But these mental processes can only be revealed as people do or say something. Likewise, the most detailed neuroana-



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tomical or neurophysiological models remain largely unconstrained until parameters are set from a precisely defined task. Imagine looking at a set of EEG recordings or PET scans and being asked to infer from them what the subject was doing or thinking at the time! Behavior is the final pathway integrating brain, body, and situation, and is, by itself, an appropriate level to model. Thus, while we welcome and rejoice in models that are neurologically realistic or that can offer any other level of explanation, we disagree with suggestions that they are, a priori, any more realistic or grounded or explanatory.

### *R2.4. Embodiment works*

The ultimate test of an "embodied" model is to have the "mental" events produce real behavior in a real device. By using our model to "act out" the A-not-B effect in an actual piece of hardware, we have convinced ourselves that the theoretical framework is indeed sufficient to generate this behavior. No additional "cognitive" tricks are needed. The model interfaces with sensory and motor systems with simple sensors and effectors themselves devoid of any intelligence.

We used a robot vehicle to enact the A-not-B behavior. The vehicle had two servo motors driving the two active wheels. The sensor system consisted of five weakly directional microphones mounted in a semi-circle. The robot mimicked the babies' acts of reaching toward A or B by moving about one meter toward a target. Targets were sound sources (loudspeakers), one each at the A and B locations, about 1.5 m apart. Both sources emitted a low volume beep permanently to represent acoustically the perceptual layout of the task, analogous to the task input in the simulations. A trial consisted of increasing the volume of one of the speakers, A or B, for a few seconds to provide the specific cue. The vehicle solved the dynamic field equations defined over an allocentric direction in space on its onboard computer. The motor planning field received input from each microphone tuned to its specific direction in space (see Bicho et al. 2000, for the dynamic system architecture used here). After a fixed delay, the location of a peak in this field was fed into a dynamical system controlling the heading direction of the vehicle as an attractor, attracting toward the target direction. The dynamical system directly controlled the two servo motors of the vehicle.

With the same sequence of 6 A trials and 2 B trials as in the experiments of Smith et al. (1999), the robot oriented and moved toward the most recently activated sound source following a delay when the field was operated in the strongly cooperative regime. Similar to an older infant, the robot was correct on all trials. However, in the input driven regime with low resting levels of $h$, the robot committed A-not-B errors, driving toward the A location on the B trials after correctly navigating to A on the A trials. As in the simulations, this was caused by the activation accumulated in the memory field preactivating the A site of the motor planning field. Sometimes the robot, like the infants, committed spontaneous errors on the first A trials when the activation at A was, by chance, too weak to completely compete with the B side.

Thus, the implementation of the dynamic field model demonstrated that no additional concepts beyond the most simple sensor and effector systems are needed to generate both A and B behavior.

## R3. Is this just the same old theory?

### *R3.1. Jost's Law*

Our attention is drawn by **Staddon et al.** to an old, and still relevant memory theory, Jost's Law, and their recent adaptation of this law applied to habituation. The model shares many properties with the current field model, including competition between recent and longer-lasting memories and an explanation for the delay effect. They also point out an important manipulation for both models to test the memory decay, varying the time between A presentations and also between A presentations and the B test trial. We have tried some of these manipulations, but they are difficult as Staddon et al. note, because infants get bored, distracted, and fussy if the delay is much longer than a few seconds. Despite the interest of this model, it is incomplete as a substitute for the field theory, as the authors note. It does not deal with the parametric effects of the inputs. Also, there are several predictions of the Staddon et al. model that are inconsistent with the A-not-B data. First, they propose that memory decays faster with increasing age. This would predict more spontaneous errors (reaching for B on the A trials) in older infants, while the opposite is true. And second, neither A nor B activations are true memories since both are strongly transient, becoming even more transient with increasing age. Thus, it is difficult to understand how the ability to memorize a single action or location would increase as children get older. In the field model, the decay time for the motor field memory is long, presumably building up an experiential history of that action over quite a long time scale. Indeed, this may be the mechanism by which infants learn appropriate affordances for action. Nonetheless, it would be important to empirically determine the time scale dynamics of both the specific input and the field memory.

### *R3.2. It's not A-not-B and if it were, my simpler model is better*

For many years, **Diamond** has been testing babies using an A-not-B task. Because of her stature as an expert in this field, her commentary warrants careful and full consideration. We find her commentary both puzzling and troubling.

**R3.2.1. Will the real A-not-B please stand up?** In section R1.3, we pointed out the pitfalls of reifying any task as the privileged window on the contents of mind. We argued that while all tasks constitute phenomena-to-be-explained, experimenters can slide into a logical tar pit when they claim that performance on any single version reveals knowledge that is divorced from the constituent dynamics of the task itself. For years **Diamond** (e.g., Diamond et al. 1997) has claimed that her version of A-not-B measured the developmental status of infants' dorsolateral prefrontal cortices, and especially their abilities to hold the representation of the hidden toy in mind and to inhibit their prepotent responses to reach to A. We argued, based on our theory, that her finding of a neat 2 second-a-month increase in the tolerated delay was an artifact of the particular testing procedure and that her inferences from these procedures are unwarranted.

The heart of **Diamond**'s response is that, based on procedural differences, our version of the A-not-B task is not the "real" one, and that, therefore, our model has "fatal


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Table R2. *Response to Diamond's Table 1*

| Procedural Element | Accusation | Response |
|---|---|---|
| (a) Discriminability of the covers from the background surface. | "Most A-not-B researchers go out of their way to make the covers discriminable from the background surface" | Studies using nondistinct covers and backgrounds: Evans & Gratch 1972; Gratch et al. 1974; Gratch & Landers 1971; Munakata 1999; Fox et al. 1979; Bell & Fox 1997. Studies manipulating covers and background, Bremner 1978; Goldfield & Dickerson 1981; Butterworth et al. 1982, and Diedrich et al., in press. |
| (b) Distance between covers. | Distance is less than half that in most A-not-B studies. | Distance between the edges of the covers is nearly identical. The distances used have been highly variable between studies, and are likely, by our model, to be predicted to matter. The next step is to experimentally manipulate distance and to fit the model's predictions to infants' responses. |
| (c) Illumination of the room. | Low illumination in our studies. | In all studies but one (specially arranged to measure infants reach trajectories), we have used brightly lit rooms. |
| (d) Presence of distraction during delay. | No distraction in our studies. | As Diamond herself notes, some studies have used distraction, some haven't. This is true for us as well. We distracted infants in the control conditions of the posture shift studies (Smith et al. 1999). There was no difference in preservation rate. |
| (e) Amount of reaching experience at A. | "training trials, 50 to 500% more real A-not-B studies." | See Diedrich et al. (in press) for an extensive discussion of A-trial procedures. Note, Butterworth & Jarrett (1982), Butterworth (1976); Bremner & Bryant (1977), and Bjork & Cummings (1984), used 7 A trials (including their "warm-up" trials) as compared to our 6. |
| (f) Rule for determining when to switch to the B location. | B trial administered after a set number of A trials rather than after a criterion of successful reaches to A. | *Both* procedures have been widely used in the literature. See discussion in text. |
| (g) Criterion for determining whether reach is correct or not. | "Painstaking frame by frame analyses." | Smith et al. (1999) showed that every reach mattered in influencing subsequent reaches, even a second reach to B on an A trial increased the likelihood of a reach to B on subsequent trials. On those few (less than 10% of trials on which the first lid reached to was not obvious, the lid contacted first was considered "correct", but *all reaches* were recorded, counted, and shown to matter. |
| (h) Infant's rationale for reaching (This is the inference made the experimenter, the procedural matter is actually the reaching target). | "Reaches were for visible lids" . . . . "no toy was hidden". . . . "when a toy was hidden, infants were allowed to have it on each trial." | Yes, in the interest of scientifically understanding the processes involved, we have experimentally manipulated the reaching target. |

flaws." We agree completely with Diamond that the procedures used to test the baby matter profoundly. Indeed, that is a major point of our theory: performance depends on principled changes in task parameters that we have experimentally tested in systematic ways. The puzzling part of Diamond's commentary, however, is that at the same time she claims our model has "fatal flaws" because of task conditions such as the distance and distinctiveness of the targets or the room lighting, she also states that her version, and others', are apparently immune to these procedural issues. Indeed, she later said in her commentary, "The A-not-B error is so robust that, despite marked variability [in] task administration, virtually every lab finds this behavior."

**R3.2.2. Robust for her, ephemeral for us?** There is a deep and abiding problem in one person defining who is doing "real" A-not-B and who is doing "fake" A-not-B. **Diamond** seems to have drawn a rather arbitrary line between acceptable procedures and those she has dismissed as "fake." Moreover, our reading of the literature is quite different from hers, and so we offer our Table R2 in response to her Table 1.

But more important, we have proposed a theoretical



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umbrella that attempts to explain everyone's version of infants' reaching to targets – whether toys are hidden or not, whether infants are trained or not, whether the targets are close or far away or distinctive or not. We need not judge whether A-not-B is robust, real, or fake, because we are beginning to understand the precise conditions that produce it. We believe this is a theoretical advance.

**R3.2.3. Reaching still matters.** The critical theoretical and empirical challenge to **Diamond**'s work is our unequivocal statement that the number of reaches to the A side matters, every single reach. If true, her multiple reversal procedure produces random responding, and cannot be used to measure prefrontal maturation. We cannot find anywhere reports of parametric test that supports her contention that "varying the number of A trials within a small range has no effect whatsoever on the A-not-B error." In her unpublished dissertation (Diamond 1983), the number of correct reaches to A she required the baby to perform varied from one to three. She reported no difference in the subsequent number of reaches to A as a function of whether the infant had reached correctly to A one or two times previously (her Tables 8 and 9). This is expected: once infants reach correctly to A they are likely to continue to do so. However, in reporting the percentage of correct reaches on the reversal trials, that is to the B side, Diamond only reports the cumulative "one or more" previously correct reaches (her Table 10). Thus, we do not know whether infants were indeed just as likely to perseverate after one correct reach to A as after three correct reaches.

Indeed, we find **Diamond**'s commentary to be internally contradictory on just this point. On the one hand, Diamond claims that reaching to A does not matter, and that the error is just as robust after one reach to A as after four (her Table 1). Moreover, she says that infants do not have to reach at all to perseverate, that just watching the experimenter hide a toy will elicit perseveration. We find this statement intriguing, although contrary to Diamond's previous data and conclusions in her own cited dissertation: "Reaching seems to matter; watching, by this analysis seems not to matter" (Diamond 1983, p. 73). Our efforts to replicate this effect – perseveration with just looking – have so far been unsuccessful.

But having made the strong claim in the first part of her commentary that number of reaches is irrelevant, later on, **Diamond** appears to change her mind. Although we are, by her account, not doing real A-not-B, she claims her 1983 explanation is valid anyway. "There are no data that Thelen et al. present which cannot easily be accounted for by the theory presented in the early 1980s." In defending her own theory, she says: "Yet, it follows straightforwardly that anything that increases the strength of the prepotent tendency that must be inhibited (*as would increasing the number of reaches to A*) [Emphasis is ours] or that make less distinct the information that must be held in mind (e.g., reducing the distinctiveness of the targets) should make errors more likely."

This is very confusing. **Diamond**'s theory predicts that number of reaches to A should matter, but her elaborate procedure for testing infants does not control for the number of reaches to either A or B. Likewise, if her theory accounts for the effects of distinctive targets, salience of the cue, and so forth, these parameters should be systematically controlled for in her procedures, but they have not been. Readers are referred to Diamond (1983; 1985) and Diamond et al. (1997) to glimpse these complex procedures which have varied, in the same experimental session with the same baby: delay, distinctiveness of targets, location of the targets (left, right, or centered), number of reaches to criterion before switching, objects or food hidden, landmarks, and whether the infant was looking or reaching. The accumulated history of these manipulations surely matters according to our theory, and according to Diamond's alternative, they should matter as well.

Finally, what about the prefrontal cortex in all this? As we said in our target article, there is abundant evidence that in humans and other primates, prefrontal cortex is involved in these decision-to-act circuits. But as **Hailman** has pointed out, it does not take a prefrontal cortex to attain a target location, nor to perseverate. Despite the analogies, the jury is still out. For one, most of the lesioned monkeys in the experiments in **Diamond**'s Table 2 were not tested with the same protocol as were the human infants. Indeed, the correct human infant experiments to assess the presumptive delays have not been done. What are required are parametric experiments at each age, such that separate groups of infants are given only one reversal at every delay.

## R4. Beyond infant reaching

In our target article, we formulated the field theory to explain a particular infant task and its variants. But, as we wrote, the theory is a more general model of motor planning. In this section, we respond to the commentators' extensions of the model to different species and to different tasks.

### *R4.1. Other tasks, other species*

We are asked by **Hailman** to consider not just the proximate mechanisms of A-not-B behavior, but also its ultimate causation through natural selection. He reports that perseverative behavior has been described in many species, including such species as parrots, where prefrontal involvement is not an issue. We agree with Hailman that A-not-B errors in infants may have some selective contribution, but perhaps more indirectly than in his account.

It is useful to recall that perseveration in infants emerges from a very particular set of experimental contexts – ambiguous targets, delay, repeated reaches to one target. It seems implausible that there would be any selective advantage for 8-month-olds to perseverate under such special conditions, and we assume that **Hailman** is not suggesting such a scenario. However, we do believe that it is adaptive for humans, and other animals, to remember cues in the environment that are no longer visible, and then to remember what they have just done and where they have just done it. Indeed, we think that, in order to learn adaptive skills, infants must have processes by which their movements and the consequences of their movements are stored and used to influence subsequent behavior. As the A-not-B error is emergent from these basic mechanisms, it surely has a phylogenetic origin.

We thank **Takeshita** as well for providing a nice example of perseveration in both human infants and chimps in another problem-solving task. As we understand his experiment, after several successes in placing a disk into a round hole, young infants and chimps repeated their efforts at the same location even though the action was now inappropri-


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ate as the correct target had been rotated to the other side. No objects were hidden – the targets were visible – and no delay was reported. This suggests a powerful action-location memory that swamped the relatively weaker visual cue of the round hole.

**Valenti & Stoffregen** say we have ignored an important part of the task context, the social dimension. We certainly agree with them on the critical role of the experimenter in setting up the task, attending to the infants' behavior, regulating the critical timing of presentation, comments to the infant, and so on. They make an excellent point about the essentially social nature of this procedure, although even when toys are not hidden, objects (lids) are involved so that the task is not entirely social, either. It would be ideal to be able to capture the social dimension in the model. In the meantime, despite many different infants and many different experimenters, the model does capture regularities and make confirmed predictions. Whatever variations there are in the social contribution, the infants' behaviors are robust.

### *R4.2. The field model and spatial cognition*

Fascinating parallels are raised by **Burns & Domjan** and **Newcombe** between accounts of spatial cognition in humans and rats and the field model. Of particular interest is the question of whether there are two systems involved, one for egocentric (or response) learning and the second for allocentric (or place/landmark) learning. The behavioral evidence in both species argues for highly opportunistic solutions: use external cues when they are available and salient and use allocentric responding when the external cues are missing or swamped by a strong response memory. The underlying neural systems may indeed be disassociable with lesions, but behaviorally, the system may work as the model predicts, as a continual competition between habit and cues, as Newcombe suggests. At the same time, it is also likely that these abilities to use response or place learning themselves have complex developmental histories. For instance, just as infants need a rather salient distinctive cue to pull them out of the response habit, so 15-day-old rat pups must rely on distinctive landmarks when tested in water maze. Remembering the correct target in the absence of salient cues only develops later, at 22 days of age, and likely with the development of the hippocampus (Nadel 1990).

### *R4.3. Perseveration and psychopathology*

We are shown by **Tschacher & Junghan** intriguing parallels between perseverative behavior in infants and similar "stuckness" seen in many kinds of mental illness. Indeed many kinds of maladaptive behaviors and thought patterns seem to reflect the inability to flexibly shift thoughts and actions to fit the dynamics of new and ever-changing situations. In an everyday example, a person who has built up relationship habits with his parents may not be able to shift to a new pattern with his spouse. In the language of the field model, the new task and specific inputs cannot compete with the old behavior habits. In some mental conditions, like obsessive-compulsive disorder, the old attractors may be so stable that they overwhelm behavior in many situations. Another lesson from the field model mentioned by Tschacher & Junghan is that there may be multiple routes for disrupting perseverative behavior in adults, just as we can manipulate perseveration in infants.

## R5. Technical issues

A number of commentators have made suggestions on technical issues concerning the model or possible extensions (**Berger; Freeman; Harter et al.; McCollum; Pelphrey & Reznick; Roberts**). Berger asks about the meaning of subthreshold activation in the field in view of the graded nature of perseverative effects in general. In our model, subthreshold activation determines the likelihood of a particular location in the field being selected by the read-out process and the corresponding parameter value being realized by the motor control system. Subthreshold activation predicts the rate of spontaneous errors and thus captures the graded nature of perseverative effects. In response to **Berger** and **McCollum** we must add, however, that the trivial mechanism for spontaneous error used in the current version of the model (reading out the location of maximal activation in the fluctuating field at a fixed time) is not satisfactory and more work must be done to understand how the "read-out" of the field can be made compatible with the fundamental stability of motor planning and control (reading out continuously in time). In our current work on a robotic implementation, we are no longer using this simplified read-out mechanism.

**Freeman** suggests that fluctuation-inducing noise should be replaced by internally generated chaos. Because noise must be taken into account in any case and is sufficient to model spontaneous errors, we have not explored more specific mechanisms for the generation of variability. But note that as requested by Freeman, the A-not-B decision is highly context sensitive in the field model. **Harter et al.** have searched for a "mixed" cooperative/non-cooperative regime, which they consider more appropriate to model the A-not-B effect. This is based at least in part on a misunderstanding. Within the regime that we call "cooperative" the dynamics is always bistable: there is the stable localized peak representing a "memory" of a previously stimulated location ("cooperative solution"); and there is a subthreshold (or "non-cooperative") solution in which the field is essentially input driven (the solution observed before specific input is applied). Such is the essence of memory: the state of the system depends on the recent history of the system. Computer memory is made from a bistable element (the flip-flop). In this bistable regime, whether or not memory is "set" by specific input does indeed, as suggested by Harter et al., depend (subtly) on the size of the different contributions: if specific input to too weak no peak in induced. The larger task input, the easier it is to induce such a peak. These dependencies are exactly how the task- and context-effects seen in real infants doing A-not-B are captured in the model.

It is another matter, whether a peak, once "set," remains stable forever or will decay at some time (or be "deleted"). As in any bistable system, spontaneous decay may occur in the presence of noise, and this alone will lead to a delay effect: the longer the delay the more chance for spontaneous decay of the self-stabilized peak. It is quite thinkable, however, that there would be additional mechanisms (like habituation) which would tend to weaken a "cooperative" solution over longer delays. In the absence of clear experimental constraints, we did not contemplate such additional mechanisms for this model.

**Roberts** shows that the input-free field can spontaneously generate standing activation waves and suggests



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this as an alternative account for task input. These (and a wealth of other possible solutions, see Amari 1977) are linked to boundary conditions (periodic in this case). Boundary conditions are not easily evaluated in models of the nervous system (e.g., Does the direction to reach define a 360 degree space with periodic boundary conditions, or is the range of reachable directions much more delimited without any special connections between the leftmost and rightmost direction?). This is one of the reasons we chose a form of the model that depends minimally on boundary conditions. Constraints can be represented by inputs in more directly testable and in graded form. Task input, for instance, may vary with experimental layout (as discussed in the target article) both in strength and in its metric contents (where the A and B locations are).

**Pelphrey & Reznick** state that model does not cover A-not-B errors induced by a single A trial. This is not true. The memory trace left by a single A trial is sufficient to induce the error. Indeed, the rate of spontaneous errors drops dramatically after a single successful A trial in the model.

## R6. Conclusion

In our target article, we provided a unifying theory to account for several decades of studies on the A-not-B error and its many variants. All of the commentators recognized that we were basically successful in this goal. The comments were of two flavors. Some amplified our drive toward embodiment by requesting more detailed mechanisms in the form of neurophysiology or motor control. Others, however, while not disputing our formulation, complained that we ignored "higher" cognitive processes. We both disagree and agree with this second point. On the one hand, if a full range of behavior can be accounted for with plausible processes close to the sensorimotor level, we believe this is a theoretical advance. On the other hand, the commentators are correct in noting that there are many human mental functions that are not specified in the present model.

We believe we have made two important contributions, over and above the particulars of the A-not-B error. First, we have raised questions about what inferences one can make about cognitive processes based on behavioral evidence. This is a major issue in the study of infants and young children for whom there is no verbal access to the workings of their minds, but it is also a deeper issue of the pervasive embodiment of everything that is considered cognitive at all. Second, we think that this model is a good starting point for a new theoretical framework in which to re-examine "higher" cognitive function, a framework in which time, context, task, and history are part and parcel of our understanding.

ACKNOWLEDGMENTS

We thank Patricia Bate, Patricia Buchanan, and Melissa Clearfield for their helpful comments on this response.

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**Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.**

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