Knowing about Knowing

Dissociations between Perception and Action Systems over Evolution and during Development

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How does it know? This is not a question that we ask about inanimate objects, unless we are speaking metaphorically. When we ask about how or what "it" knows, we are referring to animate objects or living things. But even here, we must be careful. We don't usually ask about what a plant knows, even though botanists may talk about the behavior of plants, their capacity to dupe pollinators or avoid the jaws of herbivores. We usually ask about what animals know. Here, however, is where the controversy begins. Some would claim that animals not only have knowledge of the world, but know what they know. Others would deny this claim for *some* animals, while acknowledging the possibility that perhaps chimpanzees know what they know. Yet others would deny this claim for *all* animals, and extend it further to many humans, including infants and adults with certain kinds of brain damage.

I will address two problems in this essay, the first conceptual and the second practical. Conceptually, what does it mean to know? What does it mean to know how a tool works or what someone else believes? Methodologically,

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how can we know what non-linguistic organisms know? This is a question that hinges on the competence–performance distinction, and I will show how we must tread cautiously in interpreting data from different kinds of experimental procedures. I address both questions by focusing on non-linguistic animals, pre-linguistic infants, and young children, with brief mention of patients with brain deficits. In Part I, the bulk of the chapter, I focus on folk physics, on what organisms know about the physical world. In Part II, I briefly turn to folk psychology, exploring what organisms know about themselves and others.

In a nutshell, the argument I will attempt to defend runs as follows. First, appropriate action depends on appropriate knowledge of the world. When an organism knows what it knows, its actions are different from an organism that locked out of its library of knowledge. Individuals who are locked out may act appropriately in some contexts, but when things go wrong, they will often lack the capacity to break with tradition. Actions win, beliefs lose. Second, recent studies of nonhuman primates and human infants implicate a dissociation between knowledge that is revealed through perception and knowledge revealed through action, a phenomenon paralleling work on brain-damaged patients (Goodale et al., 1991) and especially, human infants (Baillargeon, 1995; Hood et al., 2000; Munakata et al., 1997; Spelke, 1994; Spelke et al., 1995). Whereas the primate's perceptual system appears to interpret events in the world appropriately, generating the correct expectations, their action system often fails, leading to perseverative errors—a signature of an improperly functioning inhibitory mechanism. I suggest, on the basis of this dissociation, that both nonhuman primates and young human children are vulnerable to creating habitual action sequences (Graybiel, 1998), or what I refer to as *modular macros* (Hauser, in preparation). In the sense of Fodor's (1983) original position regarding perceptual modules, I argue that there are parallel action modules, systems that operate in an encapsulated manner, immune to counter-evidence (for somewhat similar views, especially in terms of procedural knowledge, see Dienes and Perner [1999]). More specifically, modular macros are fast, automatic, unconscious, action sequences. Each macro represents an adaptive solution to a recurrent problem in the organism's environment. Because of their structure and adaptive significance, they are immune to counter-evidence and difficult to break down once they have been created. If a macro misfires, creating an error, a different system must evaluate the error and correct the problematic subroutine. Animals and young children generally lack the capacity to reorganize their macros. Consequently, although they may know, at some level, that they are making an error, they may not know why, or have the resources to fix things. Although much of the evidence in favor of a dissociation comes from studies of folk physics, work in folk psychology suggests parallel processes, especially among human children. I conclude with a few remarks on how a dissection of what organisms know represents a first, albeit critical step in understanding how a sense of self evolved, and how the legacies from our past have shaped our unique individual life histories.

I. FOLK PHYSICS

Piaget (1954) proposed that children construct an understanding of the world by acting upon it. Action underlies much of the child's developing knowledge of the world. This view of development emerged from an extraordinary range of studies. All of Piaget's studies, however, had one thing in common: every task involved reaching for and acting on an object. Consider a classic experiment. Show a 6-month-old child a novel toy and she will reach for and play with it. Present the same toy, but now place an opaque screen between her and the toy. The game stops. No more reaching. Out of sight is out of mind. This result led Piaget to conclude that the capacity to keep an object in mind—object permanence—emerges some time around the child's first birthday, largely as a result of her sensorimotor experiences with objects. Between the ages of 1 and 2 years, when children will reach for a hidden object, you can play a different game with them. Show a child two opaque screens, A and B, and hide the toy behind A. Once she successfully and repeatedly retrieves the toy behind A, switch sides, hiding it behind screen B. Although the hiding game is the same, and although she appears to understand object permanence, she searches behind A, not B. This search error reoccurs over many tries. This error occurs in all infants, independent of socioeconomic background or culture. It is an error that reveals a signature of the developing mind. For 40 years, developmental psychologists have been battling over the cause of this signature (Baillargeon and DeVos, 1991; Baillargeon et al., 1990; Baillargeon et al., 1985; Diamond et al., 1994; Harris, 1986; Marcovitch and Zelazo, 1999; Smith et al., 1999; Wellman et al., 1986; Zelazo et al., 1998).

When the field of developmental psychology works well, it is like the science of anatomy or medicine. It dissects a problem into its component parts, and then attempts to extract the broken bits. Descriptively, the child's error is to repetitively search at the previously rewarded location, screen A. What components are broken? A fragile memory, one that fails to encode the new location until it has been hidden in the same place over many trials? A disconnect between the system that guides reaching and the system that stores the knowledge of the object's location? A deficit in following a simple rule such as "search behind the screen where the object was last seen"? An inability to take risks, searching in a location that has never been reinforced (B) as opposed to an area that has consistently been reinforced (A)? This kind of anatomical dissection is important because it helps isolate the locus of control, and the component or components that must develop for the child to gain control.

If repeatedly searching behind A builds up a strong association between screen A and the reward, then this problem reduces to the case of an arbitrary, but overlearned response—a learned habit. If this explanation is correct, then the more trials a child has with A before switching to B, the harder it will be for her to make the switch. This is correct, but with an interesting twist. If an experimenter repeatedly hides and then reveals the object behind A, but doesn't allow the child to search until the switch trial to B, the child shows no deficit, searching right away at B. This shows that actively searching is crucial; simply observing, passively, is not. To show whether the child's control problem stems from difficulties inhibiting a reaching response or from difficulties associated with representing—keeping in mind—one location and not the other, several researchers have presented three or more hiding locations. If the child can't inhibit either the response to A or the representation of the object at A, then when she fails to find the object at A, she should next search at random behind the other hiding locations. In contrast, if the error arises because of a reaching bias (targeted at A), then when she fails to find the object at A, she should only look at the correct location next. Results show that after failing at location A, children look behind B next. The locus of control is the reaching system. When infants and children fail to switch, it is because the reaching response to A dominates. No matter how loudly the representational system is yelling "Behind B," it is effectively silenced by the reaching system that yells louder, and first "Reach for A." Infants at this age reach ballistically. They appear to be guided by a modular macro for action that is immune to counter-evidence.

During his studies of the A-not-B problem, Piaget made an intriguing observation that has been reported by other developmentalists. Sometimes, a child will look toward the B screen while reaching toward the A screen. It's as if the child's visual system says one thing while her action or reaching system says something completely different. When infants look at B but reach behind A, it appears that their eyes know where the toy is but their hands don't. These observations led to the suggestion that the Piagetian framework of child cognitive development was flawed. Since Piaget derived all of his insights from watching children act on the world, he was measuring the development of the action system and how it is guided by what the child knows. What he failed to provide was a full account of the child's knowledge, independently of her ability to act on this knowledge. There are many things that we all know, but that we fail to use in action; often, when we use it to act, we do so incompetently, at least when contrasted with the depth of our knowledge. Similarly, there are many things that we act upon with supreme competence, but lack almost complete access to the knowledge that drives such actions. All professional baseball players can catch a fly ball, but most likely none can explain how they coordinate their catching hand with the physics of a flying ball. Upon considering the possibility that a child might know more about the physical world than her actions reveal, developmentalists turned to a different approach, one targeted at the child's eyes and attentional system. The logic of this approach—the expectancy violation looking time technique—parallels the logic of a magic show: when we perceive something that violates our understanding of the physical world, such as levitation or knives moving through human flesh without causing injury, we stare. Our attentional systems attempt to uncover the violation. The amount of time spent looking therefore becomes a measure of whether a violation has been detected.

Baillargeon (Baillargeon and DeVos, 1991; Baillargeon et al., 1990; Baillargeon et al., 1985) was one of the first to take on the possible dissociation between reaching and looking in the context of the child's developing understanding of object permanence. Starting with 4- to 5-month-old infants—individuals who, by Piagetian standards, are months away from grasping object permanence—she first allowed subjects to play with a ball and then showed them a rotating panel. Next, she placed the ball on one side of the rotating panel and then concealed the ball and part of the panel with an opaque screen; now, when the experimenter rotated the panel, the child only saw its tip. In one test condition, the experimenter rotated the panel 120 degrees, giving the correct impression that the panel had stopped at the apex of the concealed ball. In a second test condition, the experimenter rotated the panel 180 degrees, giving the impression that the panel had rotated (magically) through the ball. Infants looked longer in the second condition. Although the ball was out of sight, the infants continued to represent the ball's spatial location. This must be the case given differences in looking. To detect the violation—the magic—children must remember that the ball lies in the path of the rotating panel and must block its path upon rotation. When the panel apparently rotates through the ball, this represents a violation of solidity, one of the core principles of object-hood (Spelke, 1994). Although some details of this experiment have been criticized (Bogartz et al., 1997), leading to a healthy exchange of new experiments and controls (Baillargeon, 1995), the general finding, replicated in other labs, holds: infants are equipped with an understanding of object permanence well before they can act upon such knowledge.

The distinction between looking and reaching emerges elsewhere in development. Spelke and colleagues (Spelke et al., 1992) showed that 6-month-old infants look longer at a ball that drops behind a screen and then appears, magically, below a solid table, than at the same ball falling and landing on top of the table. However, when the same kind of task is run almost two years later, but with active searching substituted for looking, these toddlers fail to appreciate that a solid ball can't pass through a solid table (Hood et al., 2000). An experimenter shows a toddler a table with one cup on top and one below. The experimenter sets up a screen, hiding the table and cups, drops a ball, removes the screen, and asks the child to search. Repeatedly, the child searches in the cup that is under the table, and repeatedly, the child never finds the ball. How can a mere 6-month-old baby know that a solid ball can't travel through a solid table, while a 2-year-old thinks that this kind of physical event is not only possible, but the way the world works? Why doesn't the toddler's search error kick in, guiding her to look in the cup on top of the table? Like the A-not-B error, the toddler's action system has kicked in, ballistically, immune to counter-evidence. The toddler has developed a modular macro.

Much of the work on the infant's developing knowledge of the physical world finds direct parallels with nonhuman primates, a field that owes much to the work of Adele Diamond (Diamond, 1988, 1990; Diamond and Goldman-Rakic, 1989). For example, like young infants, many nonhuman primate species show the A-not-B-error, and in other contexts, commonly fail to solve problems due to inhibitory difficulties. For example, Diamond ran a comparative study of developing rhesus monkeys and human children, as well as an exploration of the underlying neurobiology by using a lesion technique. The task involved showing subjects an object (food for monkeys, toys for infants), and then placing it inside a transparent box with only a single open side on any given trial. The subject's task was to find the opening and reach inside to retrieve the object. Infant rhesus monkeys, 2- to 4-months old, reach straight ahead on every trial; this action works when the front face is open, but fails on all other trials. Older rhesus monkeys find the opening first, and then reach in to grab the food. Adult rhesus monkeys with lesions in dorsolateral prefrontal cortex are like infants, reaching straight ahead on every trial. These data suggest that a mature prefrontal cortex is essential. Diamond's parallel studies with human infants revealed that prior to 7–9 months, subjects repeatedly reach straight ahead; older infants find the opening and then reach in to grab the toy. Young rhesus and human infants are incapable of controlling the straight-ahead reach even though they never obtain the object inside the box. This can't be a memory problem. The toy is always in view. It can't be a motivational problem: these young infants reach straight ahead dozens of times, with no success. Again, the modular macro has fired. These two primate infants are on autopilot. These perseverative errors have parallels in other tasks with primates and, importantly, also show the signature of a dissociation. I first describe two tasks that reveal striking errors of action, and then turn to matched experiments that show success using perception, but no action.

In collaboration with Hood (Hood et al., 1999), who first ran these experiments with children (see below), we presented cotton-top tamarin monkeys (*Saguinus oedipus*) with a vertical frame, open in the middle, and with three short pipes (A, B, and C) on top and three boxes (1, 2, and 3) lined up below. While subjects watched, the experimenter attached an opaque S-shaped tube from pipe-C to box-1 and dropped a piece of food down the C-pipe. On their first try, the tamarins looked in box 3, the box directly beneath the release point. No food. Since there was no connection between pipe-C and box-3, and since the tamarins never saw food drop into this open space, nothing about their perceptual experiences would give them this result. After they opened box-3 and found nothing, they then opened box-2 and finally box-1, where they found the food. When the experimenter ran another trial, keeping the tube in the same C-1 configuration, the tamarins typically repeated the same error, searching first in box-3, then 2, then 1. Some individuals repeated this error 20–30 times. Eventually, some tamarins picked box-1 on the first try. At this point, the experimenter moved the tube from pipe-B to box-3. The tamarins searched in box-2, the box beneath the release point. The tamarins repeated this error over and over again. In fact, when the experimenter replaced the opaque tube with a transparent one, allowing the tamarins to see the food fall, they searched in the correct box, and did so consistently. But when the experimenter put the opaque tube back in, placing it in the same position as the transparent tube on the previous trial, the tamarins bounced back to their original error, searching in the box beneath the release point.

The tamarins appear to have a remarkable gravity bias, one that causes search error after search error. It is puzzling that they don't use their failed attempts to find food beneath the release point to try some other strategy. It is even more puzzling that they don't try the most obvious solution to this problem and pick the box *associated* with the tube. Without even looking at the food's release point, the correct response on every trial is always the box associated with the tube. Since we know that animals as evolutionarily distant as worms and humans use associations to solve problems in the world, why don't the tamarins fall back on this simple strategy? Perhaps tamarins, unlike their distant relatives the worms, are too smart for their own good. Instead of using the simplest strategy for finding food, they are overthinking the problem. Or perhaps this kind of gravity bias pays off most of the time, falling victim to the exceptional cases when a warped experimental mind sets up something equivalent to a Rube Goldberg contraption.

To show that there is not just something odd about the tubes apparatus, or about tamarins, consider two additional experiments. If gravity is really the problem, as opposed to some other factor such as that there are no tubes in the real world, then removing gravity should change the patterns of search. An experimenter presented the tamarins with the same apparatus, but set it up horizontally as opposed to vertically (Hauser et al., 2001). When the experimenter rolled the food down the tube, the tamarins showed a marked improvement in their search patterns, and most importantly, did not show the equivalent of the gravity bias. This shows that when there is no effect of gravity, tamarins can find a piece of food that has been invisibly displaced within a tube. Tubes are not the problem. But since the same tamarins were run on the horizontal test after they were run on the vertical test, perhaps this experience helped them. We can't distinguish between tamarins learning about tubes and tamarins learning about objects that move out of view. To explore these possibilities, an experimenter tested the tamarins on a vertical setup that was identical to the original experiment, but replaced the tube with a hidden ramp, a flat piece of plastic concealed by an opaque screen. Although these animals had hundreds of trials with the vertical tubes apparatus, and many trials with the horizontal tubes, they once again failed on the vertical ramps, and with the same repetitive errors. They first picked the box beneath the release point, then the middle, and then the correct box. And then they started all over again on consecutive trials. Like infants committing the A-not-B-error, the tamarins' response bias is the result of a modular macro. Unlike the A-not-B error, however, this response bias is not the result of an arbitrarily learned action sequence. Rather, reaching for the location beneath the release point appears to be the result of a long evolutionary history, one dominated by the physics of life on earth. Gravity dominates. Things that fall typically do fall straight down. The mind of a tamarin has been designed with this knowledge. Although we can't say for sure whether they have an innate expectation about falling objects, it is a reasonable starting assumption, one that could potentially be tested by rearing tamarins in a gravity-free environment, or perhaps less technologically challenging, in an environment where they never saw objects fall.

A second experiment shows that tamarins are not the only species to form such expectations about falling objects. Following up on the table experiments described above with infants and toddlers, we (Hauser, 2001) ran a comparable series of experiments with free-ranging rhesus monkeys, the same species used by Diamond in her box experiments. An experimenter showed a rhesus monkey a table, and then placed one box on top and one box directly below. The experimenter then hid the boxes and table from view with a screen, dropped an apple over the two aligned boxes so that it fell out of view, removed the screen and allowed the subject to approach. Consistently, the rhesus monkeys searched in the box below the table, and consistently, of course, they failed to find the apple. How could rhesus monkeys, who have survived for approximately 20–30-million years on earth, not *know* this? They can't really think that if you jump off a branch from the top of a tree that you will just effortlessly drop *through* the branches to the spot directly below on the ground. They can't. But the results are strikingly consistent. In parallel with the tamarin results, if the experimenter now turns the problem on its side, removing gravity, and then rolling an apple toward two concealed boxes placed in a straight line, rhesus always pick the near box. Here, they seem to *know* that when you roll an apple towards two boxes, that the closest box will stop or contain the apple. Rhesus know that an apple can't roll *through* a box. But they somehow think that an apple can drop through a box and then a table and into a box below.

Tamarins, rhesus and many other animals have some understanding of physical principles. The fact that they fail when searching for invisibly displaced falling objects is actually more, rather than less evidence for what they know or understand. What is striking about their error with falling objects is how consistent it is. Their search patterns reveal an immunity to counter-evidence. This kind of immunity is precisely what one would predict if the animal's expectations are based on theory-like principles. This proposal is similar to what Carey (Carey, 1985) and Keil (Keil, 1994) have proposed for conceptual change in child development (see also Gopnik and Meltzoff, [1997]), an argument that mirrors Kuhnian (Kuhn, 1970) views of scientific change. Like scientists with a pet theory, both tamarins and rhesus appear to hold a theory of falling objects, one that they adhere to even in the face of evidence that the theory is wrong and requires modification. We can say that it is a theory about falling objects rather than objects or moving objects in general, because both tamarins and rhesus have the right theory when it comes to making predictions about objects that move along the horizontal plane. And what gives this story even greater support is the fact that the searching pattern for tamarins and rhesus are similar even though each species lives in a different environment and has been designed to solve somewhat different ecological and social problems. Importantly, tamarins are highly arboreal animals, spending most of their time high up in the canopy. Rhesus, in contrast, are largely terrestrial, spending most of their time on the ground. Tamarins have therefore had little experience watching objects roll on a flat surface, and although they have presumably seen objects such as fruit falling, they are unlikely to track and search for such objects on the ground. Rhesus have presumably seen numerous objects moving on the ground and falling from trees, and most likely have searched for falling objects since they spend more time on the ground than do tamarins. Nonetheless, both species show a strong gravity bias.

Although tamarins and rhesus may be like children and scientists in terms of holding a theory about falling objects, they differ in one critical way: most children and scientists eventually give in, acknowledging at some level that their own theory no longer accounts for the data, and that a new theory is necessary. They engage in conceptual change. It is possible, of course, that if an experimenter had tested the tamarins or rhesus over thousands of trials that, eventually, they would select the correct box. But then it would be necessary to distinguish between theory change and mere training or shaping. The main point here is that for many animals, the action system spontaneously dominates, thereby blocking the kind of conceptual change that is required for deepening one's knowledge of the physical world. Such rigidity may be especially common when the expectations or theories that underlie their behavior represent statistical regularities of the world. Gravity is one such regularity.

Humans up to the age of three years are no different from adult tamarins with respect to performance on the tubes task (Hood, 1995). Over dozens of trials, these relatively old children searched for the ball in the gravity box, the box directly below the release point. Is their error due to a problem of memory, inattention, motivation, or sheer puzzlement over the nature of tubes? We can easily rule out the first three. The memory requirements are slim: from release to search is only a few seconds, well within the child's capacity for recall. On every trial, the child is deeply focused on the task, attending to the experimenter dropping the ball and then immediately running over to search. And the child is highly motivated, moving to find the ball, looking in the gravity box, then the middle box and finally, the correct one, only to start the same steps all over again on the tenth, twentieth, and thirtieth trial. Moreover, even if the child does have problems of memory, attention, and motivation, these three factors can't explain the systematic and biased pattern of search to the gravity box. To explore the possibility that children are just flummoxed by the tube, possibly ignoring it and just assuming that things fall straight down, Hood (1998) re-ran the experiment, but with an interesting twist. The experimenter presented the tubes apparatus on a video monitor, dropped the ball down the tube, and then asked the child to point to the box with the ball. As in the original experiment, children pointed to the gravity box and did so over dozens of trials. In a second condition, the experimenter inverted the apparatus, with the tube's opening on the bottom and boxes above. Due to a concealed suction pump above, when the experimenter let go of the ball, it was sucked up into the tube. No gravity. No error. Children point to the correct box, the one attached to the tube. Children have no problem with tubes. They have problems with gravity. What gets in the way is their modular macro, their tendency to assume that when things fall, they fall straight down. The action system then takes this assumption as true, and causes the child to reach, ballistically, for the gravity box.

One might have assumed, initially at least, that the tamarin's gravity bias was due to sheer stupidity. Such stupidity should have been replaced over evolution by the acquisition of language and other useful mental capacities. However, this same stupid, primitive system rears itself in child development, even in children who can speak and comment on their failed attempts to retrieve the ball.

What these studies show about our species is that we start life as folk physicists, individuals endowed with a core set of principles for understanding the physical world. This folk physics effectively guides perceiving in all children, independent of their culture. Sometimes it does the right thing, and sometimes it doesn't. Because children lack the resources to change this initial system, it takes years for them to develop an understanding of the physical world that is sufficiently rich and accurate that it can guide adaptive actions.

Back to the argument against Piaget, and his focus on action as a measure of knowledge. Recall that both the table and tubes tasks are action tasks. To succeed, and provide evidence that they know about invisible displacements as well as the core principles of object knowledge, subjects must retrieve the object from the appropriate location. In an attempt to explore whether the action system's dominance might cover up knowledge at another level, Santos and I (Santos and Hauser, 2002) re-ran the table task with free-ranging rhesus monkeys, this time using the violation of expectancy looking time method. Show rhesus monkeys the table and box set up as before. Hide the display, show them an apple, and then drop it behind the screen. Remove the screen and show them the contents of the boxes. In one trial, the apple appears in the top box and the bottom box is empty (the correct outcome), while in a second trial the apple appears in the bottom box and the top box is empty (the incorrect outcome); we create this low-tech magic by pre-loading the boxes before we set up, and then dropping the apple into a hidden pouch behind the screen. On the basis of results from the searching experiments, rhesus should expect the apple to land in the bottom box—this is where they search. Consequently, for a rhesus monkey watching this show, the violation arises when the apple appears in the top box. Rhesus should therefore look longer in trial one than in trial two. In fact, they do the opposite. When the apple appears in the bottom box, rhesus monkeys look longer than when the apple appears in the top box. What these results suggest is that rhesus monkeys know that the apple can't travel through the top box, and then through the table, into the bottom box. This is a violation of the solidity principle, *sensu* Spelke (1994), and it draws their attention. But what rhesus know as revealed by their eyes is inconsistent with what they know as revealed by their reaching behavior. Perceptual know-how appears to be different from action know-how. This distinction is not due to an immature brain as we only tested adult animals. It may, however, be due to *evolutionary* immaturity in the sense that the macaque brain, in contrast to the human brain, lacks the requisite circuitry for connecting perception and action know-how in some contexts.

To date, my students and I have run a variety of experiments on rhesus monkeys and tamarins, using both reaching and looking tasks. In some contexts, perhaps especially situations involving contact mechanics (Santos, in preparation), we find a dissociation between perception and action, with looking measures suggesting accurate knowledge of the physical world, while action measures suggest inaccurate or incomplete knowledge of the physical world. For example, while tamarins tested on the vertical tubes task search in the incorrect location for an invisibly displaced object, they correctly predict the object's location on the basis of looking-time data (Hauser, in preparation). In contrast, studies of numerical representation using looking time and search measures converge on the same limits or capacities (Hauser and Carey, in press; Hauser et al., 2000; Hauser et al., 1996). Thus, rather than a domain-general dissociation between perception and action, there appears to be a domain-specific dissociation. Fleshing out the details of these dissociations is am important goal for future research.

The fact that some animals may have perceptual know-how without action know-how raises a puzzle: individuals may perceive a situation correctly without being able to correctly act on the same situation. Neither tamarins nor rhesus appear capable of reaching into the library of knowledge that guides looking in order to use it to guide reaching, at least with respect to certain aspects of their folk physics. Based on their behavior, the information appears to be sealed off in a section of the brain. This kind of inaccessibility is directly relevant to Fodor's modularity thesis. As Fodor originally postulated, modules are automatic, fast, effortless, susceptible to breakdown from damage, task or content-specific, and, critically, informationally *encapsulated* from other parts of the brain. Thus, when we perceive a visual illusion such as the Mueller-Lyer line illusion, we can't turn them off even when we convince ourselves by measuring the lengths of the lines. The information tucked away in our visual systems—the parts responsible for seeing the lengths as equal—is encapsulated. No matter how sure we are about the dimensions of these lines, we can't convince our visual systems to change their opinion. The action system seems to be constructed in a similar fashion. When there are statistical regularities that map onto habitual motor responses, the brain constructs modular macros, designed to implement adaptive responses. Due to their long evolutionary history, such responses are immune to counter-evidence. For animals, such immunity blocks conceptual change, blocking access to knowledge that is available to perception and prediction, but not action. For human infants, such immunity represents one barrier that must be overcome in order to engage in conceptual change. As the inhibitory veil is lifted in the young child, knowledge of the world is enriched.

II. FOLK PSYCHOLOGY

In the previous section I alluded to the fact that the dissociation between perception and action is domain-specific. What about the domain of folk psychology? Although far less empirical work on animals has targeted this question, there is a general sense in which inhibitory problems play an equally powerful role in constraining conceptual change in the domain of folk psychology as they do in the domain of folk physics. The bulk of this work stems from studies of normally developing children and contrasting patterns among autistics, with only a smattering of evidence from nonhuman primates. The goal of this section is therefore to build the argument from studies of human children, and then show how the conceptual and methodological conclusions from this work bear on studies of nonhuman primates.

A telltale sign that the child has grasped the richness of other minds is that he or she appreciates the logical possibility of false beliefs, that a person can believe something that is false because they missed a key event or because another person told them something incorrect. To test young children's understanding of false beliefs, developmental psychologists have used what is now famously referred to as the "Sally–Ann" task, named after the two puppets that appeared in Wimmer and Perner's (1983) original experiments. Though there are literally hundreds of variants of this task, each designed to pick apart why children either fail or succeed, the core narrative runs as follows. A child watches as Sally and Ann play with a ball. Sally then puts the ball in a basket and leaves the room. While Sally is away, Ann takes the ball out of the basket and places it in a box. Sally then returns to the room. The experimenter now asks the child "Where will Sally look for the ball?" The classic result is that 3-year-olds point and say the box, while 4- to 5-year olds point and say the basket. The older children understand that Sally has a false belief. She must believe that her ball is in the basket because this is where she left it and she didn't see Ann move it to the box. Since she didn't see Ann move the ball, she can't know or believe that it is in the box. In the absence of a critical perceptual event—seeing—Sally lacks a critical mental state: knowing or believing. From their third to their fifth birthdays, children undergo a conceptual revolution. Faced with the fact that Sally searches in the basket and not the box, 3-year-olds are handed a piece of counter-evidence. They predicted that Sally would look in the box. Their prediction was wrong. Over a period of one year, their conceptual system changes, as they grasp a critical fact about human minds: sometimes we believe things that others don't. This was the party line until the mid-1990s.

In the last section I mentioned the idea that what children or nonhuman primates appear to know as revealed by their eyes may well be different from what they know as revealed by their actions. Now reconsider the Sally–Ann test. In the classic version, the experimenter asks the child to point or say where Sally will look for the ball. These are actions. What are her eyes doing before or during her actions? To address this question, Clements and Perner (1994) ran a Sally-Ann test with 3- and 4-year olds, but in addition to asking them to point and say where Sally would look, they also filmed their eyes. Like Piaget's child who looked to the B-screen but reached behind the A-screen in the A-not-B task, 3-year-olds looked to the basket and then pointed or said "the box"; 4-year-olds, predictably, looked and pointed/said "the basket." Three-year-olds appear to have knowledge of what others know or believe, but they can't access it, consciously. It's as if they have a hunch, but lack sufficient confidence to bet on it. In fact, in a follow-up study by Ruffman and colleagues (Ruffman et al., 2001), when children were asked to bet on where Sally will search, the amount they bet matched their pointing and verbal responses, but not the direction of their looks. Three-year-olds have implicit knowledge of others' beliefs. They lack explicit knowledge (Dienes and Perner, 1999). They don't really know what they know. What blocks access to this knowledge?

In the A-not-B error, I suggested that action dominates perception and perhaps even beliefs; reaching wins over thinking about the object's location. The infant might know that the object is hidden behind B. But since she has been reaching with success behind A, the action system pulls its trump card. Perhaps the same explanation holds for the Sally–Ann test. Though the children tested have never seen the puppet show before, and the experimenter only tests them a single time, the task itself involves a common, one might even say habitual, response: pointing. When we point, we point to where something is. But to demonstrate an understanding of Sally's false belief, the child must point to the basket. She must point to where the ball is not. To succeed, she must inhibit the natural, habitual tendency to point to where something is (the box), pointing instead to where something is not. In a series of experiments, Carlson, Moses and their colleagues (Carlson and Moses, 2001; Carlson et al., 1998) ran 3-year-olds on the classic Sally–Ann test, but instead of pointing, asked them to place a sticker on the relevant container. They succeeded, putting the sticker on the box. Unlike pointing, sticker-ing is not a habitual response. We can place stickers wherever we like.

What these simple and clever experiments show is that young children may understand false beliefs, but the original task prevented them from displaying their abilities. The classic Sally–Ann test is insensitive to the child's developing control problems. Unlike older children, 3-year-olds are more vulnerable to inhibitory problems (Carlson and Moses, 2001; Carlson et al., 1998; Leslie, 2000; Leslie and Polizzi, 1998; Russell, 1997; Russell et al., 1994; Zelazo and Frye, 1997). Like reaching for the ball behind screen A or searching for a dropped ball in the cup below the table, young children lack the level of control needed to reveal their conceptual understanding of Sally's false beliefs.

Some humans never acquire an understanding of other minds. As Baron-Cohen (Baron-Cohen, 1995) has put it, they have mindblindness. Unlike certain kinds of neurological deficits that can be repaired through therapy or by the natural compensatory abilities of the brain, these individuals are locked into a surreal social world, one in which others' beliefs are terra incognito. These individuals are autistic. This clinical label is itself somewhat controversial in that the syndrome is characterized by a highly variable etiology, ranging from severely retarded to high-functioning individuals. What appears to be characteristic of all autistics is that they have poor social, communicative, and imaginative abilities, as well as difficulties with planning. Given these descriptions, the critical question is whether the deficit is domain-specific, restricted to the individual's folk psychology, or whether it is domain-general, a reflection of general intelligence and control, the province of the frontal lobes?

Consider the following statistics (Baron-Cohen, 2000). First, autism is highly heritable. Second, autism is much more common in males than in females; the ratios run from as low as 4:1 to as high as 40:1. Third, high-functioning autistics often have brilliant careers in the physical and mathematical sciences, but never in the humanities or social sciences. Fourth, the non-autistic parents of autistic children are disproportionately represented in jobs that involve little to no social interactions, including engineering and mathematics. Fifth, autistics are not vulnerable to visual illusions such as the Ebbinghaus-Titchner illusion: when shown two identically sized circles, one surrounded by larger circles and the other by smaller circles, normal individuals perceive a difference in size between the two inner circles while autistics do not (Happe, 1996). One explanation for this effect is that autistics tend to perceive the world at a local level, ignoring contextual or global information; non-autistics see things in context, looking at the relationship between objects in a scene. Thus, when asked to describe a scene involving two or more characters, autistics tend to overlook the relationship between the characters and how one individual's actions might influence what the other does; rather, they focus on each individual or object in the scene. They are blind to relationships. Taken together, these observations raise an intriguing idea, one developed by Baron-Cohen: if autistics have a genetic deficit that leads to a selective impairment of their folk psychology, might they have not only a selective sparing of their folk physics, but a selective enhancement?

A first step into the problems raised by the statistics summarized above is to explore how autistics fare on classic theory of mind tests. In contrast to normal children of the same age, autistic children tend to look away when someone looks at them, tend not to look where someone is looking, do not understand that seeing is a proxy for knowing, and do poorly when asked about the emotions expressed in someone's eyes (Baron-Cohen, 2000; Frith, 1989, 1991; Happe and Frith, 1996). Two simple tasks reveal this deficit. Show an autistic child a picture of two girls standing in front of an open box, with one girl looking in and the other looking straight ahead. When asked "Which girl knows what is in the box?", autistics are as likely to say the girl looking in the box as the girl looking straight ahead. In a second task, an experimenter shows a picture of a cartoon character whose eyes are looking up and to the left, in the direction of a Hershey candy bar; in the other corners are different candy bars, labeled by name. When asked "Which candy bar does Johnny want?," autistics are as likely to say "Hershey bar" as they are to say the names of the other three candies. However, when asked "Which candy bar is Johnny looking at?," they answer "Hershey bar." What this study shows is that perception is spared in autistics, while folk psychology is impaired. "Looking" refers to Johnny's perception, "want" to his beliefs and desires.

Two further contrasts help make the case that autistics have a selective impairment in their folk psychology. If general intelligence is necessary for understanding what others believe, then children with the Down syndrome, who suffer from extreme mental retardation and low IQs, should perform as poorly on the Sally–Ann test as do autistics. They don't. Down syndrome children correctly attribute false beliefs, while autistics do not. Are autistics like normal 3-year-olds who also fail false belief tasks? Not quite. Recall that normal 3-year-olds seem to have some understanding of false beliefs as revealed by their eyes, as opposed to their pointing or verbal responses. Thus far, the looking version of the Sally–Ann test has not been explored in autistics. What Leslie and his colleagues have run, however, is a simplified Sally–Ann test that 3-year-olds pass (Leslie, 2000; Leslie and Polizzi, 1998). Instead of asking children "Where will Sally look for the ball?," an experimenter asks "Where will Sally look first for the ball?" Simply inserting "first" into the sentence causes 3-year-olds to answer "the basket." Perhaps this extra word causes the child to think about where Sally placed the ball first, as opposed to considering what Sally believes. To control for this possibility, Leslie and colleagues ran the same experiment, but this time, Sally stayed in the room while watching Ann move the ball to the box. When the experimenter asked three year olds "Where will Sally look first for the ball?", they answered "the box." Though this change helped 3-year-olds understand Sally's beliefs, it did not help autistic children. Leslie argues that the simplified Sally–Ann test reduces some of the inhibitory burden for 3-year-olds, thereby allowing their folk psychology to surface. For autistics, in contrast, lifting the inhibitory burden is irrelevant, because the difficulty lies in their folk psychology, the fact that they just don't have an understanding of others' beliefs; see, however, Zelazo and colleagues (2002).

I previously mentioned that autistics often have brilliant careers in the physical and mathematical sciences, and often have non-autistic parents who have similar professions. Baron-Cohen (1999) used these observations to ask whether the selective deficit in folk psychology might not be paired with a selective sparing or even an enhancement of folk physics. In one study, he tested three high-functioning autistics on three tasks, one for folk physics, one for folk psychology, and one for planning and inhibitory control. Of the three subjects, one was a professor of mathematics who had won the Field medal, equivalent in prestige to the Nobel prize; the other two were accomplished university students in physics and computer science, respectively. To test for their folk psychology, Baron-Cohen presented a "Reading the Mind in the Eyes" task. Subjects looked at a picture of a person's eyes and picked one of four adjectives to describe the person's emotional state. All three autistics scored well below normal adults. The folk physics test involved questions about the functioning of mechanical/physical devices. For example, the experimenter first showed a picture of a balanced scale, with one small box on one side and two larger boxes on the other, and then asked "Which box is the heaviest?" All three autistics scored well above normal adults. For the planning and inhibitory control task, Baron-Cohen presented the Tower of Hanoi. In this task, an experimenter sets up a stack of rings around a center peg, ordered from largest to smallest; next to this peg are two others, both empty. The experimenter asks the subject to place the rings in the same order (large to small) on the empty far peg by making the fewest moves to the center peg. Here too, the three autistics far surpassed normal adults, completing the task in lightening speed. These results show that there can be a significant impairment in folk psychology without a corresponding impairment in other domains, or in other forms of reasoning, planning or control. The bottom line appears to be that autism looks like a highly selective deficit of folk psychology.

Studies of autism also bear on another aspect of inhibitory control. Many autistics appear to be uninhibited copy cats, parroting precisely what others say and do, as if it were a reflex. They have what clinicians refer to as echolalia. Autistics are also prone to repetitive rocking, in addition to obsessional thoughts and actions. But autistics are not alone in this deficit, as evidenced by patients with obsessive-compulsive disorder. Is the inhibitory problem the same, however, in these two clinical populations? More specifically, is the inhibitory problem domain-specific or domain-general? To address this question, Baron-Cohen and colleagues (Baron-Cohen and Wheelwright, 1999) explored the content of obsessions in high-functioning autistics. Given the impairment in folk psychology, autistics should have few if any thoughts in this domain. Further, given that autistics seem to escape from the challenges that the social world presents by focusing on the physical world, they should have most of their thoughts focused on folk physics. If the inhibitory problem is domain-specific, then autistics should have obsessional thoughts about the physical world. To test this logic, Baron-Cohen used parental reports of obsessions in autistic children, as well as children with Tourette's syndrome, a disorder associated with facial or body tics—an apparent failure of inhibition at the motor level. In contrast to the children with Tourette's, whose obsessions focused largely on motor and sensory events (touching, smelling objects), autistics' obsessions focused on folk physics, with almost no observations in the domain of folk psychology. These studies show that the problem of inhibitory control is domain-specific.

In parallel with the child's conceptual revolutions in the domain of folk physics, the child undergoes comparable changes in the domain of folk psychology. Each child is endowed with core building blocks in each of these domains, naïve theories that help them make roughly correct predictions of the world. From birth to at least 6 or 7 years old, these initial theories are refined, and in some cases, radically transformed. During the reorganization period, the child is vulnerable to significant errors. The child's theory runs like a macro, immune to counter-evidence. No matter how much experience, and how much painstaking teaching she receives, the child will simply not learn. Learning, or more accurately, conceptual change, arises when the counter-evidence is so significant that a new theory is required. It is at this tipping point that critical experiences move the child to a new level of understanding. In the domain of folk psychology, there is a transformation at around 4 to 5 years. For the first time, what others believe, intend, desire, and want is transparent or at least, translucent. With this knowledge, children can not only understand how their own actions will influence what others believe and how they feel, but can think about how they would feel if someone did something to them. Their folk psychological expertise enables them to model the world, running mini-simulations of how their actions influence their own lives and those with whom they interact. For the autistic child, this world is a deep mystery.

How might we characterize the folk psychology of different nonhuman primates? Since many of the insights into the child's developing theory of mind come from tasks that use language, we can not use the same sorts of approaches with animals. The pioneering work in this area was Premack and Woodruff's (Premack and Woodruff, 1978) classic experiment on chimpanzees, research that led to the birth of the term "theory of mind." In the original task, a chimpanzee saw food placed in one of two boxes. In one condition, a cooperative trainer entered the room and if the chimpanzee indicated the box with food, the trainer opened it and shared the food. In a second condition, a non-cooperative trainer entered the room and if the chimpanzee indicated the box with food, the trainer opened it and took all the food for himself. The prediction was that if chimpanzees have a theory of mind, recognizing that only they know where the food is because the other trainers did not see the food placement, then they should indicate the food box to the cooperative trainer but not to the non-cooperative trainer. Results suggested that chimpanzees could make this distinction, using their knowledge of what each trainer knows and believes to guide their own behavior, pointing to the food box with the cooperative trainer and the non-food box with the non-cooperative trainer. This, together with a suite of other studies, led Premack (Premack, 1986; Premack and Premack, 2002) to conclude that the chimpanzee has a theory of mind, albeit not as well developed as in humans.

Since Premack's pioneering work, there have been an increasing number of studies exploring mental state attribution in primates. Unfortunately, the field is in a state of complete chaos (Cheney and Seyfarth, 1990; Hauser, 2000; Heyes, 1998; Tomasello and Call, 1997). Starting with Premack's original work, some have argued that these data fail to provide evidence for a theory of mind because the chimpanzees' ability to discriminate between trainers only emerged after dozens of trials, suggesting that they learned a discrimination based on behavior as opposed to beliefs. More recent work with chimpanzees (Call and Tomasello, 1999; Povinelli and Eddy, 1996) reveals a suite of failures, using a variety of tasks and subjects, as well as targeting different building blocks to a theory of mind, such as an understanding of the seeing–knowing distinction. All of these studies, however, share two potential problems in common: they ask whether chimpanzees have a theory of human minds as opposed to other chimpanzee minds, and they require inhibitory control, a capacity that is weak even among chimpanzees (Boysen et al., 1996; Boysen et al., 1999). For example, in Premack and Woodruff's original experiment, the chimpanzee must inhibit pointing to the box where the food is in order to deceive the non-cooperative trainer, pointing instead to the box without food. In Povinelli's experiments, where chimpanzees are required to beg for food from one of two trainers, they must inhibit begging from one individual on each trial. In the initial training, chimpanzees were required to beg from a trainer who was sitting facing and looking forward. In subsequent conditions, two trainers are present, each positioned in a slightly different way, designed to assess whether the chimpanzees are using the seeing–knowing proxy or some physical feature, perhaps linked to the original training condition. This design therefore creates an inhibitory problem as the chimpanzee must inhibit begging from a trainer who might be seated in such a way that it appears like the original condition (e.g., torso facing the chimpanzee, but eyes closed or head averted backwards), but in fact is inappropriate because this trainer is looking away.

The most recent work in this area appears to solve both problems raised above. Hare and colleagues (2000; 2001) asked whether chimpanzees know about other chimpanzees' beliefs, using a competitive task that requires some level of inhibitory control, but perhaps less than the others because of the absence of training. In the first experiment, an experimenter placed a subordinate chimpanzee on one side of a center arena and a dominant on the opposite side; the center arena included two opaque screens with two bananas located in view of the chimpanzees, in between the screens. When the chimpanzees' doors were opened, allowing them access to the center arena, the dominant marched out and grabbed the bananas. The subordinate didn't move, even when he was given a head start. Subordinates clearly know that they can't compete for food with a dominant; they exert some level of inhibitory control by staying put as opposed to rushing out. In the second condition, an experimenter placed one banana in between the screens and the other banana behind one screen, in view of the subordinate but not the dominant. If the subordinate recognizes what the dominant can see, and therefore understands what the dominant knows, then when the doors open, the subordinate should make a bee-line for the concealed banana. This is precisely what happens: the subordinate grabs the concealed banana while the dominant grabs the center banana. In a final condition, an experimenter concealed one banana on the dominant's side and placed the other centrally between the screens. Here, the dominant can see both bananas while the subordinate can only see the center banana. Each animal does the right thing: the subordinate stays put while the dominant first grabs the banana in view, and then grabs the concealed banana. This makes sense because there is no contest over the concealed banana. The subordinate can't know about this piece. These results, together with several other experiments involving critical controls, has led Hare and colleagues to conclude that chimpanzees know what other chimpanzees know. Comparable experiments with monkeys have all failed to provide evidence for mental state attribution, suggesting a phylogenetic difference in the evolution of this capacity.

At present it is difficult to say with confidence whether chimpanzees are endowed with a folk psychology that is as well developed as in humans, and if so, whether we are referring to an adult human or a child of age three, four or five years. The studies by Hare and colleagues are encouraging, especially since the design of their experiment bypasses some of the inhibitory problems inherent in the earlier approaches; given the results on children, avoiding problems of inhibitory control is crucial. What we also lack thus far are tests that explore whether chimpanzees, or other animals, have knowledge of others that is implicit as opposed to explicit, or more descriptively, knowledge that is available to perception but not action. Given the success of looking-time methods in studies of folk physics, it is time to implement such approaches in the domain of folk psychology (Hauser, 1998).

III. WHEN "I" KNOWS

Over the last twenty or so years, there have been an increasing number of cases reported where a patient exhibits a dissociation between perception and action. Such dissociations suggest that the knowledge that mediates perception may in fact be different from the kind of knowledge mediating action. Consequently, one challenge confronting the neurosciences is to establish the circuitry underlying these different kinds of knowledge, what some consider to be a distinction between implicit and explicit knowledge. The study by Goodale and colleagues (1991) provides a useful starting point. The patient studies showed preserved capacities with respect to action, with deficits in perception. In the classic test, the patient is presented with a slot (resembling a slot in a mail box) positioned at different angles. In the action task, the patient's task is to take a letter-sized piece of paper and place it into the slot. The patient performs at the level of normal subjects. In the perception task, the patient must hold the letter with an orientation that matches the orientation of the slot (i.e., the orientation that would be appropriate should the patient be required to put the letter in the slot). Here, and on several other variants of the task, the patient fails. The patient suffers from a deficit that causes a decoupling between the knowledge mediating action and the knowledge mediating perceptual judgments. In contrast, work by Damasio and colleagues (Damasio et al., 1982; Tranel and Damasio, 1985, 1993) on patients with prosopagnosia suggests that these individuals may have some implicit or covert level of knowledge about facial identity, but no explicit or overt knowledge. Evidence for implicit/covert knowledge comes from skin-conductance data (showing differentiating of familiar and unfamiliar faces), as well as experiments where prosopagnosic patients are introduced to nurses associated with different emotional valences (e.g., one is very pleasant and never tests the patient, one is neutral, and one is negative, always running tests, never allowing breaks, and so on) and subsequently show differentiated responses to each without any explicit recognition. Cases such as these, together with others including work on blindsight (Weiskrantz, 1986), and patients with prefrontal damage (Bechara et al., 1997), suggest that brain damage can cause a disconnect between different systems of knowledge.

What I have argued in this essay is that normal animals and developing human children show similar kinds of dissociations. In the case of children, the dissociation appears to reflect the immaturity of the brain, while in animals, a different explanation must be invoked. In particular, because all of the studies conducted on this topic have thus far been run with adult animals, it is not possible to invoke the standard arguments used thus far with infants and young children (Diamond et al., 1994; Diamond and Goldman-Rakic, 1989; Munakata et al., 1997). Rather, what seems to account for the primate data is that within particular domains of knowledge, circuitry connecting up perception and action systems are either weak or not there at all. Although we know relatively little about such circuitry, I believe the data speak to an additional factor æ perhaps in parallel with developmental studies, it appears that for many if not most animals, the action system largely dominates the perception system. More specifically, whenever animals are confronted with statistical regularities, such that selection favors fast, automatic, and unconscious responses, animals will create what I have called modular macros. These action sequences are immune to counter-evidence. More particularly, they are immune to perceptual evidence that might, if allowed to surface, cause the animal to change what it believes about the world. This conclusion has both conceptual and practical implications. Conceptually, these results suggest that future work in the neurosciences might profitably look for circuitry connecting between perception and action systems, not in a domain-general sense, but in a domain-specific sense. I reiterate this point here because in some domains, the perceptual and action tasks reveal comparable underlying knowledge; number is one such domain. Given convergence in some domains and divergence in others, it should be possible to isolate why these differences emerge both in evolution (comparisons across species) and within development (e.g., perhaps some individuals acquire certain habits early in development, that sets the stage for a dominant action system).

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Methodologically, work on animals must follow the lead of those working in infant cognitive development, recognizing the fact that inhibitory problems may get in the way of revealing conceptual knowledge. Said more starkly, what an animal does may not capture what it knows. This is an old problem, but one that must be reemphasized today given our increasing understanding of the psychology and neurobiology of control.

In conclusion, what makes each individual unique, is his or her knowledge, what he or she knows and how he or she experiences it. As a species, we evolved a perhaps unique sense of self, one that allows us to reflect upon what we know, to use such knowledge to generate expectations about others who are similar to and different from us, and to recognize that in many ways, each of us has a unique view of the world. That said, many of the ways in which we view the world is derived from an ancient stock, innate knowledge that we share with our primate ancestors because we evolved in the face of comparable social and ecological pressures, problems that have reoccurred over and over again.

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