8 Commentary and Discussion on Imitation and Human Development

8.1 Grasping Action
Paul L. Harris on Meltzoff

In a tightly argued chapter, Andrew Meltzoff not only lays out some influential findings concerning the development of imitation, he also seeks to answer a fundamental question about the origins of social cognition. How does the infant come to establish an equivalence between itself and others? His proposals echo a traditional philosophical argument about our knowledge of other minds—the argument from analogy—but he brings to that argument a wealth of empirical discoveries, many of them made in his own laboratory over the past quarter century. His thesis has ramifications for the infant's developing understanding of other people's attention, action, and emotion. In this brief set of comments, I focus on the action of reaching and grasping with a view to evaluating Meltzoff's argument.

Meltzoff's first claim is that there is an innate connection between the capacity to observe and encode the acts of another person and the capacity to execute those same acts. Developmental evidence for the existence of that innate connection comes from early imitation, by now a well-established phenomenon. As Meltzoff points out, there are many studies from different laboratories that testify to its existence. Moreover, it would be wrong to conclude that the phenomenon is very narrow in scope; it extends to a variety of gestures. However, I would insert one caveat. With one important exception that I discuss later, all of the findings on early imitation pertain to gestures that are not visible to the self. Neither a neonate nor I can see ourselves stick out our tongue, close our eyes, or purse our lips. Arguably, then, nature has provided the human species with an innate dictionary that specifies equivalences between facial gestures of the self and others because the construction of such a dictionary would be next to impossible on the basis of experience. So equipped, the human neonate enters the world ready to engage in the kind of expressive, face-to-face exchange with its caregivers that is so characteristic of our species.

In this account, it becomes plausible to speculate that such an innate dictionary is limited to invisible facial gestures and is not available for nonfacial gestures. We can, after all, observe our own hand movements. In principle, therefore, the neonate or young infant might come to link a given manual program with its visible result; for example, an infant might link the motor program for the opening and closing of his or her hand with the visible gesture that ensues. Then, on seeing a comparable visible gesture produced by another person, the infant might retrieve and execute the previously linked motor program. Indeed, to the extent that many manual gestures are directed at visible objects, and to the extent that the trajectory of the hand and the orientation of the fingers needs to be adjusted to the particular visible properties of the object, the capacity to visually monitor both grasping movements and the visible properties of the objects at which they are targeted is likely to be at a premium. Such fine-grained visual monitoring of the movements of the self might play a key role in establishing a larger repertoire of equivalences between the infant's own object-directed manual gestures and those produced by other people. In this view, the role of the innate dictionary of equivalence would be confined to a relatively narrow set of gestures, notably facial actions that cannot be subjected to visual guidance.

As Meltzoff points out, however, evidence is available suggesting that neonates not only imitate facial gestures, such as tongue protrusion and lip movements, they also imitate simple manual gestures such as opening and closing the hand (Meltzoff & Moore, 1977; Vinter, 1986). Consider the detailed report by Vinter (1986). She found that when 4-day-old infants watched an adult open and close her hand repeatedly, they were more likely to open and close their own hand—whether partially or fully—than in two control periods, one in which the model made a facial movement, namely tongue protrusion, or else remained still. Moreover, it was not the case that the infants became indiscriminately active when they saw the model open and close her hand—there was no comparable activation of tongue protrusion. Of course, we might insist that 4 days would be enough for neonates to pursue the alternative "visual monitoring" route that I sketched earlier, but that does not seem very plausible. Infants of this age are unlikely to spend time watching their manual gestures. In short, this study provides persuasive evidence that entries in an innate dictionary of self–other equivalence extend beyond invisible facial movements to include manual movements as well. The visual monitoring route may complement this innate foundation, but it is not its ultimate basis even for manual actions.

The next step in Meltzoff's argument is to propose that the child gradually builds up, on the basis of experience rather than an innate dictionary, links between the mental states that guide or accompany a given motor movement and the movements themselves. A couple of examples will be instructive. When a baby smiles, he or she presumably feels happy. That positive mental state will, by dint of repetition, come to be associated in memory with the facial expression that accompanies it. Subsequently, when a baby sees an adult smiling, the innate dictionary described earlier will provide a cross-reference to the baby's own facial expression; it will evoke a mental representation of that facial expression or trigger an actual, imitative movement. In either case, the associated mental state of feeling happy will be generated. Similarly, a baby who is able to make an accurate object-directed reach will come to associate the goal of grasping an object with the act of reaching. Seeing another person reach toward an object will evoke some mental representation of the infant's own object-directed reaches, or conceivably an actual reach, and that will in turn evoke a representation of the goal that guides such a movement.

Within this framework, Meltzoff describes the results of imitation experiments in which an infant watches an incomplete or unsuccessful action: a model reaches toward a target but fails to grasp it. Infants of 18 months and even 15 months appear to grasp the goal behind the action and instead of copying the failed gesture, execute it successfully. By implication, when an infant sees a model reach (albeit unsuccessfully) toward an object, that sight evokes some mental representation of the infant's own object-directed reaches, which in turn evokes a representation of the goal that guides such movements. Hence the infant imitates the model, not by slavishly copying the model's failed action, but by producing actions that do attain the model's apparent goal, including actions that the model did not even attempt.

What about younger infants? Meltzoff notes that results from two laboratories have shown that infants aged 12 months and younger do not produce this sophisticated goal-based pattern of imitation. In one study for example, 9-month-old infants imitated successful acts but not unsuccessful ones (Meltzoff, 1999b). How should we interpret the change in behavior from 9 to 18 months? Following Meltzoff's own analysis, it is tempting to conclude that 9-month-old infants have not yet established the appropriate connections between actions and goals that make such sophisticated imitation possible. More specifically, when they observe a model's unsuccessful action, they do not represent the goal of the action.

Other evidence, however, casts doubt on this interpretation. Consider recent findings by Woodward (1998). She reports that when infants of 6 and 9 months have seen an actor reach for a particular toy, they subsequently show greater dishabituation if the actor reached along the same path for a new toy than they do if the actor reached along a different path for the same toy. By implication, when the infants watched the initial reaching, they were more likely to encode the goal object that it was directed toward rather than the precise trajectory of the reach. These findings would lead us to expect that infants of 6 or 9 months would also attend to the goal of an action when they imitate, contrary to Meltzoff's findings.

How might we explain this inconsistency? One explanation might run as follows. An infant of 6 or 9 months can reach for objects. Therefore, in line with Meltzoff's proposals about the role of experience, an infant of this age will have established connections between its actions and its goals. Those connections help the young infant to interpret the actor's reaching as goal directed in Woodward's experiment. However, while the connections can serve interpretations of others' actions, they are not yet able to support the sophisticated imitative actions produced by 18-month-olds. Unfortunately, this defense of Meltzoff's account is weak at best. If his account is correct, essentially the same mental machinery is recruited whether the infant perceives the act of a model in terms of its goal or imitates the act of a model in terms of its goal. More generally, it is one of the strong predictions of Meltzoff's account that there is a tight link in development between the perception and interpretation of another's action and the imitation of another's action. Any developmental separation between perception and imitation causes trouble for the theory.

Here, then, is another line of defense. Arguably, it is simply harder to figure out the goal of a failed action—such as a reach that over- or undershoots the goal object—than to figure out the goal of an action that is brought to a successful completion. Meltzoff effectively asked whether infants could figure out the goal of unsuccessful actions, whereas Woodward asked whether infants could figure out the goal of a successfully completed action. Nine-month-olds can manage the latter but not the former, whereas 18-month-olds can manage both.

This account leads to a simple, testable prediction that is based on a merger of the methods introduced by Meltzoff and Woodward. The infant sees a model facing two toys, a toy bear positioned on a higher shelf and a cup positioned beneath it on a lower shelf. The model reaches up for the toy bear and grasps it. We then switch the positions of the two toys and give the infant an opportunity to imitate the model. The infant can either reproduce the model's movement by reaching up for the cup, or the infant can reproduce the model's goal by reaching down for the bear. If the same machinery serves action perception and action imitation, the pattern of imitation produced in this setup should parallel the pattern of habituation observed by Woodward (1998); it should reflect the goal rather than the movement by the actor.

Finally, we may consider a persuasive piece of evidence reported by Meltzoff. In the normal course of development, several things may happen concurrently, but it is difficult to infer any causal relationship from such concurrence. Training experiments are especially helpful in analyzing causal mechanisms. Brooks and Meltzoff (2002) have found that in the absence of any special training, infants are insensitive to whether or not an actor is blindfolded. So when an adult wearing a headband turned to look at something, 12-month-old infants turned to look in the same direction. Yet they also turned to follow the actor's "gaze" when the headband was replaced by a blindfold. By implication, they did not realize that the blindfolded adult could not see anything.

The preliminary results of a follow-up study by Meltzoff and Brooks indicate that when infants have experienced a blindfold themselves they behave differently. They no longer turn to follow a blindfolded adult. This is a compelling demonstration of the way in which a particular first-person experience can be attributed—by analogy with the self—to another person. Still, we can ask how general this analogical route is. To understand the mental experience that ensues from a blindfold, extrapolation from first-person experience with the blindfold seems the most direct route. True, one might slowly infer that restricted mental experience simply from observing the limited behavioral repertoire of an actor wearing a blindfold—first-person experience of wearing a blindfold is not critical. Still, that route into what the other is or is not experiencing seems tortuous, at best.

The advantages of analogizing from first-person experience is less clear-cut if we return to consider the act of reaching. Woodward has observed that 3-month-old infants do not show the type of goal encoding that is apparent with older infants. However, infants of this age are quite poor at reaching out to grasp an object themselves. Hence, their lack of goal encoding is just what one might expect in Meltzoff's account. Indeed, studies in progress at Woodward's laboratory underline the potentially didactic role of first-person experience. When the infants were fitted out with Velcro mittens that enabled them to "grasp" objects more successfully, they were subsequently able to encode the goal structure of an actor's reaching and grasping action (Woodward, 2002).

So, should we conclude that infants are attributing goals on the basis of their own goal-directed actions with the Velcro mittens? Possibly but not necessarily; when the infants wore the mittens, they had the experience of being an agent. However, they could also watch themselves—spectatorlike—as they made their successful reaches. If Meltzoff is right, the experience of being an agent is critical. Yet it is also possible that sustained visual monitoring of reaching is sufficient to teach infants to encode that action in terms of its goal. Experiments currently in progress at Woodward's laboratory should give us an answer. More generally, the simple act of reaching appears to be a wonderful vehicle for answering in some detail the question of whether our ability to execute a given action is inextricably woven into the way that we perceive it.

8.2 Do Babies Know What They Look Like? *Doppelgängers* and the Phenomenology of Infancy
Nicholas Humphrey on Meltzoff

When an infant imitates a face, is it possible that he can *see* the resemblance between his own face and the model's; that is to say, see it as a visual image, so he can compare what the two faces *look* like? To be able to picture oneself in any such literal sense is surely beyond the capacity even of most adults. So the suggestion that a baby might be doing it may seem absurd. Yet extraordinary data, of the kind Andrew Meltzoff has reported over the past 25 years, invite extraordinary hypotheses. And it is in this spirit that I want to introduce into the discussion a singular phenomenon: the illusion of the *doppelgänger*, or autoscopic hallucination, where a person does indeed *see* his or her own double.

The phenomenon, as it occurs in adults, is quite rare. It is sometimes experienced by healthy individuals, but is more common in those with epilepsy, and appears to be linked to right-hemisphere parietal lobe malfunction (Blanke et al., 2002; Krizek, 2000). Graham Reed has described the typical manifestation:

> Usually the *doppelgänger* apparition appears without warning and takes the form of a mirror-image of the viewer, facing him and just beyond arm's reach. It is life-sized, but very often only the face or the head and trunk are "seen." Details are very clear, but colors are either dull or absent. Generally the image is transparent; some people have described it as being "jelly-like" or as though projected onto glass. In most cases the double imitates the subject's movements and facial expressions in mirror-imagery, as though it were his reflection in a glass. (Reed, 1972, p. 54)

Sometimes, however, the subject may have a more detached perspective, as in this case:

> At first "B" usually "saw" his double only sideways, i.e. his profile, "but now I can see him from any possible position, from behind as well as from his front, just as if I was walking round him and choosing the position from which to look at him. He is absolutely identical with me in every detail of his features, expression of his face, his dress and movements." The "double" does everything the patient does in the given moment. (Lukianowicz, 1960, p. 985; see also 1958)

The fact that the human mind can create illusions of this kind, albeit when in a pathological state, would seem to imply that there must exist a "normal" capacity for modeling the body of a remarkable kind. Reed relates it to Sir Henry Head's notion of the "multimodal body schema"—"a plastic and isomorphic representation of one's body which must be incorporated in our nervous system if we are to account for our constant awareness of our posture and position in space"—and suggests that perhaps "the *doppelgänger* experience may be a displacement or projection of that internal model" (Reed, 1972, p. 55).

In volume 2, chapter 1 Meltzoff proposes an idea similar to Head's to explain normal infant imitation: his notion of "active intermodal mapping" (AIM). Thus he suggests that a baby, when imitating another person, maps a visual representation of the other person's body onto a proprioceptive representation of his own.

Now, it is surely possible that just the reverse of this could be happening in the case of the *doppelgänger*, so that the subject maps a proprioceptive representation of his own body onto a visual representation of another as-if person (although, in this case, it's himself!).

In the context of this book it hardly needs saying that mirror neurons might be just the ticket for creating such intermodal equivalences (and the suggestion of right-parietal involvement in creating the *doppelgänger* phenomenon fits nicely with the brain-imaging data reported by Decety and Chaminade vol. 1, ch. 4).

However, what interests me more than the mechanism of the *doppelgänger* is the question of what such a sophisticated mental construction might be good for. Does the *doppelgänger* have any functional utility? And if so, what and when?

It is true that in adulthood the *doppelgänger* is seemingly not good for anything (and in fact it is generally regarded by those experiencing it as a nuisance); moreover, it is experienced only by the very few. But could it be that the *doppelgänger* is primarily a phenomenon of early infancy? Could it even be that most babies experience their own bodies projected as an external visual image most of the time? *Doppelgängers* as *near-birth* experiences? I think the *doppelgänger* might in this case be a remarkably useful "teaching aid." Meltzoff writes: "Infants can imitate and recognize equivalences between observed and executed acts" (2002a, p. 35). My proposal is that a baby's experience of his visual double would give him a relatively easy means of doing just this. But more than this, his capacity to see himself, not so much as others see him, but *as he sees others*, would be an invaluable tool for entering other people's minds (as many, from Nietzsche on, have pointed out).[1] For it would mean that when, for example, a baby feels sad, angry, happy, and so on, he would be able to know just how he himself looks and so have a basis for inferring what other people are feeling when they look the same way (see Humphrey, 2002, pp. 94–99).

We live in interesting times for the understanding of cognitive development. I suspect we have only just begun to discover how strange—but wonderfully designed by nature—the phenomenology of infancy may be.

8.3 Construing Selves from Others
Wolfgang Prinz on Goldman

In volume 2, chapter 2 Goldman discusses two major competing accounts of mind reading, that is, theory theory and simulation theory, and examines the possible role of imitation in both of them. Different as they may be, the two accounts do share one crucial, common belief. They both believe that access to one's own mental states is easier and in a way more direct than access to knowledge about the mental states of others. This is

1. Nietzsche not only advanced a "simulation theory" of mind reading, but explicitly linked simulation to bodily imitation. "To understand another person, that is *to imitate his feelings in ourselves*, we ... produce the feeling in ourselves after the *effects* it exerts and displays on the other person by imitating with our own body the expression of his eyes, his voice, his walk, his bearing. Then a similar feeling arises in us in consequence of an ancient association between movement and sensation. We have brought our skill in understanding the feelings of others to a high state of perfection and in the presence of another person we are always almost involuntarily practicing this skill" (Nietzsche, 1881/1977, p. 156).

particularly true of simulation theory, but as Goldman points out, it also applies to some brands of theory theory, particularly the version defended by Meltzoff and his colleagues, (e.g., Meltzoff & Moore, 1995; Meltzoff, 1999a; see also Meltzoff, vol. 2, ch. 1). The common question here is, how do we understand other people's minds—and how do young infants come to develop such understanding? The common answer is that we do it by resorting to our own minds—and young infants come to do it that way, also. To our own mental states, it is believed, we have direct or privileged, access. Hence, when we set about understanding others' minds, we use our own mentality to either infer or simulate the mentality of others, proceeding thereby from intrapersonal to interpersonal understanding (Meltzoff, 1999a). This is what researchers sometimes call the "you-like-me" perspective; I understand you as being like me (both a being of the same kind and being in the same state as I am in).

The notion of privileged access to first-person knowledge is deeply rooted in both philosophy and folk psychology, and to many researchers it appears to be a self-evident intuition. However, as it is often the case, there are good reasons to distrust such intuitions. In fact, I find it difficult to believe in privileged access to first-person knowledge. I believe that organisms are made for understanding the world surrounding them, rather than for understanding themselves; that is, how their own bodies and their own minds work. For instance, it has long been known that veridical perception relies on mechanisms that subtract, from the total information available, any contributions that are due to the perceiver/actor. Perceivers cancel the proximal information related to themselves in order to perceive the distal world surrounding them (cf., e.g., Woodworth, 1938; Epstein, 1973). In a way, then, we are made to know the world at the expense of knowing ourselves. The mere fact that we use certain cognitive tools for understanding the world neither requires nor implies that we understand how these tools work, or, as Millikan puts it so elegantly in the next section, "merely having a mind is not the same as knowing about minds" (p. 185).

Hence, if there is any privilege at all, it lies in the access to knowledge about things and persons in the world, but not about one's own mental states. In other words, privileged access refers to knowledge about third-person events, not about first-person states.

If this is true, we should think of reversing the question. The problem now is how we understand our own minds and how young infants come to develop such understanding. As in theory theory, the answer might be that we do it by resorting to the categories folk psychology offers us for understanding other minds—categories that gradually develop in young infants.

However, unlike in theory theory, these categories are first applied to others and then to oneself.

Accordingly, rather than understanding others as like ourselves (i.e., passing the "like-me" test), we may perhaps come to understand ourselves as like others (passing the "like-you" test, as it were). This is what I suggest calling the me-like-you perspective; I understand myself as being like you (again, both a being of the same kind and being in the same state as you).

Is there a role for imitation in this perspective? There is, but not surprisingly the functions of imitation are quite different from those discussed by Goldman in the you-like-me perspective. The question now is how young infants come to construe themselves as agents in exactly the same way as they construe other people. Imitation games offer an obvious tool to support such construal of subjectivity and agency. In these games, the infant may from time to time copy the adult's actions, but the infant's actions are mirrored by the adult as well. As a consequence of being mirrored by somebody else, the infant comes to perceive her own actions through the other. It may be such attending to one's own actions through the mirror of somebody else that may counteract and eventually help to overcome the inbuilt mechanisms for canceling the perceiver/actor and her contributions to the world she is perceiving and acting upon. In a way, then, the infant is in the situation of a portrait painter, who can easily see and paint a portrait of other people, but needs a mirror to see and portray herself.

To conclude, let me mention two issues, both of which require further elaboration in the you-like-me perspective. First, there is of course still a long way to go from perceiving one's actions mirrored through someone else to construing oneself as an agent like the other. For instance, one needs to explain how an infant can distinguish between the other mirroring the infant and the other acting on his own account. Second, how do infants come to understand other minds in the first place? The categories offered by folk psychology may be part of the answer, but some other part will certainly have to be derived from basic mechanisms of perceiving actions and events.

8.4 Some Reflections on the Theory Theory–Simulation Theory Debate

Ruth Garrett Millikan on Goldman

Goldman tells us that the theory theory and the simulation theory are different theories concerning "how ordinary people go about the business of attributing mental states" (vol. 2, ch. 2, p. 80). This phrase is ambiguous in ways that may make a difference, I think, both to the controversy between the theory theorists and the simulation theorists, and to the question of what imitation might have to do with mind reading.

First, the question might be taken to concern the natural ontology of beliefs about mental states. What kind of structure does a belief about a mental state have? Supposing that having a belief about a mental state requires one to have a concept or thought of that kind of mental state, what sort of thing is a thought of a mental state? Is it just like the thought of any other sort of state, say, the state of being old, or the state of being sick, or the state of being wet? Suppose that the mental state to be thought about is an intentional state, and suppose that intentional states are mental representations. Is a representation of a representation *as* a representation (not just as a vehicle) just another ordinary representation but one that happens to have a representation as its object? Or does it require a completely different sort of mental act?

Second, the question might be taken to concern the ontogeny of the ability to have beliefs about mental states. What are the steps in the normal developmental process that lead to the capacity to think about mental states? Was there perhaps a certain cultural or historical process that resulted in humans acquiring the ability to think about their mental states, as there was a cultural or historical process that resulted in humans acquiring the ability to think about numbers or, at least, say, about negative numbers? Or does each child acquire the ability to think about mental states all on its own, a strong disposition to this having been built in, perhaps, by natural selection?

Third, the question might be taken to concern the natural epistemology of beliefs about mental states. How do people discover what mental states other people are in, or discover what states they themselves are in? Besides knowing what states other people are in, sometimes we can predict what states they will soon be in. And sometimes we can retrodict what states they must have been in previously to account for the emergence of their current states. Sometimes we can predict what states we ourselves will be in given certain future conditions. And sometimes we know something of the etiology of the states we are currently in or have been in the past.

Now the origin of the "theory theory" was a story about the first of these three matters. It was a story about what is involved in having any sort of thought or concept at all, not a story specifically about thoughts or concepts of mental states. What kind of structure does a belief about anything have? The story was that beliefs are mental representations, indeed were originally mental sentences (e.g., Sellars, 1963, ch. 5; Quine, 1960, ch. 1) and that they acquire their content, they get to be about what they are about, because of their inference relations to one another plus their connections to perceptual input and, some philosophers thought, also to motor output. The concept of a kind of mental state was the concept of that state and not another for the same reason any other concept was about whatever it was about—namely, because of its role in inference. This sort of theory of what a thought is was aptly called a "theory theory" of the nature of thought because it was exactly the same as the theory developed in the second two-fifths of the twentieth century concerning the meanings of theoretical terms in a scientific theory. As was forcefully pointed out by Hempel (1950, 1951), Sellars (1963) , Quine (1960, ch. 2) and others, having a concept and having a theory came to much the same thing in this analysis. Or, putting this differently, the difference between changing your beliefs and changing what your thoughts were about (changing your meanings) became moot, or at any rate highly problematic.

Goldman objects to the theory theory as applied to thoughts of mental states partly on the grounds that if it were true, no account could be given of the second problem mentioned earlier, which concerns the ontogeny of the ability to have beliefs about mental states. Goldman claims that a child could not discover a set of laws concerning mental states by observing the origins and progression of its own mental states because in the theory theory, to be able to think about its own mental states, it must already know what the psychological laws are that define those states. Note, however, that in the theory theory of concepts, this sort of problem is perfectly general, having nothing to do with the theory theory of thoughts of mental states in particular. If the laws of a theory define the concepts in the theory, it seems one could not reach any theory by performing simple inductions in order to derive its associated laws. The criticism, if valid, would challenge the whole of the most characteristic twentieth-century theory of what thoughts are.

Sellars and Quine had a way out of this dilemma. They assumed that we learn to think by being taught to speak. We are taught connections between sentences by our elders and we internalize them. Ordinary people do not develop theories of their own, but slowly learn traditional methods of thought handed down through the generations. The formation of genuinely new theories was another matter. Many, including Sellars—the original theory theorist about thoughts of mental states—were explicit about the use of models and analogy in the development of theories. Sellars (1963, ch. 5) thought the original model for the ordinary theory of thought was language, and suggested that the development of this new theory took

place originally during the history of ideas, not during evolutionary history. However, he thought individual children learned about the existence of mental states by being taught correct sentence connections and the conditions for uttering sentences about mental states by their elders and then internalizing these sentences and connections.

Sellars was also explicit about how a person introspects their own current beliefs and desires, and about how they learn to do this. That is, he also had a theory about part of the third question noted earlier. How do people discover what mental states they are in? It is not done (as Goldman suggests the theory theorist must hold) by observing one's behavior, but by catching oneself in the state of being disposed candidly to express a certain thought and then prefacing that expression with "I believe" or "I want" or whatever (Sellars, 1975). The fact that an entity is first discovered merely as a theoretical entity does not preclude the possibility that one can later learn how to observe it directly, how to make judgments about it directly from experience without inference. In contrast to this, no simulation theorist has, to my knowledge, developed a clear theory of how one knows what one's own thoughts are, whether they are really one's own, or merely thoughts that one has simulated in the pretend guise of another. What the theory theorist clearly has on his side concerning questions two and three above is the clear understanding that merely having a mind is not the same as knowing about minds, nor is having mental states the same as knowing one has mental states. The general capacity to think of mental states needs to be explained, including the capacity to know what particular mental states one is currently in.

Now I am not disposed to accept the classical theory theorist's view either of the nature of thoughts generally or of the nature of thoughts about thoughts. Nor, of course, am I disposed to accept their view of how children learn to think about thoughts. But it seems to me that a critique of the classical theory theory needs to go considerably deeper than Goldman's current analysis. Most important, to oppose the theory theorist on his original ground, one would need to develop a different theory than the classical twentieth-century theory theory, either of what it is to think about or have a concept of anything—say, of dogs, or of the state of being old, and so forth—or, alternatively, one would need to explain exactly why it is that mental states cannot be thought about in the same sort of way as any other states, and of how they are thought of instead. I myself think that the theory theory of thoughts and concepts is mistaken quite generally (for this, see Millikan, 2000). But moving closer to Goldman's view, there might also be a reason to suppose that there is something peculiar about concepts, not of mental states in general, but at least of *intentional* mental states, states that seem to be like inner representations.

Sellars assumed that our model for thoughts was words and sentences, not words and sentences classified by vehicle types, but rather as classified by "roles"; later terminology would have said by functional roles or inferential roles. He did not explain what it is to have a thought of a role, however. What would it be to think of a thought as being a mental sentence that plays a certain role in inference and as typically stimulated by such and such sensory input? Would a full-blooded theory theorist have to say that this would involve having a mental name for the sentence, say, "Tobermory," and then believing a whole host of psychological laws about which other mental sentences, such as those named, "Samantha" and "Melissa" and "Xavier," when these are believed, generally lead to belief in the sentence Tobermory, and which mental sentences, when believed along with Tobermory, generally produce in their wake still further sentences, for example, those named "Tobias" and "Melek" and "Dildar," and so forth? One way or another, it seems to me, the theory theorist would need to concede that our ability to think of the inferential role a mental sentence plays must ride piggyback on our own dispositions to make inferences with just such a mental sentence, not on an entirely independent and prior knowledge of what these dispositions are.

Putting this differently, suppose that thinking that someone holds a mental sentence, *p*, to be true involves thinking that they are likely to believe whatever *p* immediately implies. This supposition is definitional of the theory theory of what thought is if we spell out the theory theory assumption that believing that someone holds *p* true involves having a more or less correct theory of what it actually is to hold *p* true. Now ask what is it like to know what *p* immediately implies? The obvious answer would seem to be that this knowledge must somehow rest quite immediately on one's having *oneself* a set of inference dispositions with regard to the thought that *p*—not, in the first instance, a set of beliefs about laws of thought, but a set of dispositions to obey laws of thought.

Something like this principle would seem to generalize to any theory of what it is to have a thought of a thought if it is assumed that thoughts are mental representations. In order to believe that John plays the trumpet, I certainly don't need to be able to play the trumpet myself. But in order to believe that John believes that it is raining, I do need to be able, if not to believe that it is raining, at least to entertain the thought that it is raining. Surely, thinking of a representation, not just as a vehicle, but as something

having a known intentional content, requires that I be able to think of or entertain that intentional content myself.

Returning to simulation, if we suppose that merely thinking about a certain content or entertaining it involves harboring a representation of that content that is processed offline, that is, it is not connected with dispositions to act, as in the case of online beliefs, and if we refer to this sort of offline processing as simulation of belief, then it seems to follow that any mental representation theorist will have to agree that the ability to simulate beliefs one does not have oneself must lie behind the ability to attribute beliefs to others.

Note, however, that it does not follow that one might not also simply remember from experience what kind of conclusions one has usually reached from what kinds of experiences or from what kinds of prior beliefs, or remember having been told what kinds of conclusions another has reached, thus concluding what another may think without currently engaging in simulation. Nor does it follow that the ability to know what kinds of nonintentional mental states tend to have what kinds of outcomes, or what kinds of situations tend to cause what kinds of nonintentional states, depends on concurrent simulation of these states. Only thoughts of intentional mental states succumb to this argument.

Concerning predictions of future intentional states, I think it is important to recognize that prediction of people's future intentions often works backward by prediction first of their future actions. That is, regularity in actions is what we notice first about people, perhaps including ourselves. We know what people generally do in specific situations, or what people of a certain culture or class are likely to do, or what a certain individual is likely to do, all by simple induction. If we then think about these people's intentions, it is likely to be in order to explain the behavior we expect rather than to predict their behavior by first knowing their intentions. Thus we are disconcerted when we find certain people "unpredictable," for most people are quite predictable in many broad ways (although not usually in the details of exactly *how* they will do this or that). The idea that we use mind reading primarily to predict behavior seems to me quite mistaken. Mostly we use it only to explain behavior after the fact, or to explain behavior that has been predicted by simple induction from previous behavior patterns.

My suggestion, then, is that thinking about intentional mental states probably requires the capacity, at least, to entertain mental representations offline. But so does imagining of all kinds, and so does hypothetical thinking, and so does considering possibilities and hypotheses and so forth. Do all of these derive somehow from the ability to imitate? That seems doubtful. Does the ability to imitate require the ability to think off-line? Well, that might depend, as Goldman seems to agree, on how you define imitation. In neither case, however, it seems to me, is there evidence of any but the most indirect connections between imitation and mind reading *specifically*.

I feel compelled to add one more skeptical comment. I do not know where it has been shown that the phenomenon of mirror neurons, mentioned in Goldman's chapter and in many of the talks at the Royaumont Abbey meeting on imitation, needs to be interpreted as any different from the well-known phenomenon of efferent copy. To interpret it as a phenomenon of efferent copy, all one needs to do is to assume that efferent copy can predict perceptions of object-centered or aperspectival happenings as well as perspectival happenings. If so, efferent copy could predict, say, that a hand is seen in an object-centered way grasping a nut, disregarding how the hand is related to the subject's own body. Another part of efferent copy, connected, say, with the "where" (the dorsal) rather than the "what" (the ventral) system, might predict the relation of the grasping to the subject's body. If that were so, there would be no potential causation between seeing another grasping a nut and grasping it oneself. The causation would only be between intending to grasp a nut and the firing of neurons anticipating the seeing of the grasping of a nut. However, it may be that the neurologists have their own careful reasons for thinking that is not the way it happens, and I just have not heard them yet.[2]

2. Here is a response to this paragraph from neuroscientist Marco Iacoboni: "An efferent copy can occur only when an agent does an action. It is a carbon copy of a motor plan. If the individual is only observing, there is no efferent copy. Mirror neurons may represent the input of the forward model if their activity during action observation is the expression of a simulation of an efferent copy. But their activity during observation can't be the expression of a real efferent copy, because the observer is not moving.

Now, even if mirror neuron activity during action observation is a simulation of an efferent copy, and during execution is a real efferent copy, it seems to me that the most parsimonious account of this machinery is to link seeing and doing for the purpose of imitating (and other things too, of course). To postulate that in fact this neural activity is only useful for linking the intention to grasp (during execution) with anticipating the seeing of a grasping (during observation) seems to me unnecessarily complicated" (personal communication). ED.

8.5 Who Can Imitate Depends on How We Define Imitation
Thomas R. Zentall on Anisfeld

Anisfeld systematically reviews the literature on infant imitation and proposes that (1) the evidence for imitation in infants younger than 11 months involves behavior either that is not novel or that can be attributed to other causes (i.e., to an increase in general arousal or "priming"), (2) many studies have not included the appropriate no-demonstration (baseline) control, and (3) the best evidence for the representation of an observed behavior is deferred imitation.

With regard to the first point, Anisfeld does not accept as evidence of imitation the copying of any response that may have been previously made by the observer. Instead, he views such behavioral matching as priming (a more automatic, reflexive behavior), in which the observed behavior primes the memory for the earlier action or elicits a "recognition response." However, there is no practical way to ensure that a similar behavior has never occurred before. All behavior, including behavioral sequences, are likely to be similar to some previous behavior or behavioral sequence, and thus it is impossible to rule out response generalization. Furthermore, although imitation sounds more cognitive than priming, there is no objective way to distinguish between imitation and behavior that is primed by the memory of observing that behavior demonstrated by another.

On the other hand, why should it be necessary that copied behavior be novel? We tend to think of the value of imitation in terms of the acquisition of new behaviors. But observers may imitate a familiar functional behavior in a context in which it would not otherwise be as likely to occur. Such imitation would also have adaptive value.[3] The theoretical importance of imitative behavior is not its novelty but the fact that the behavior is often "invisible" to the one imitating. That is, for the observer, there is often the absence of a sensory match (often referred to as the problem of correspondence) between the behavior of the model and the self.

Rather than insisting that the imitated behavior be novel, a more useful means of assessing the behavior is to compare its probability of occurrence with that found under suitable control conditions. Anisfeld seems to agree because he proposes that an appropriate control is a baseline condition in which the target behavior is assessed in the absence of its observation. However, he challenges Meltzoff's (1988b; Heimann & Meltzoff, 1996) use

3. See also Byrne, vol. 1, ch. 9. ED.

of the observation of an alternative activity as an appropriate control and suggests that a baseline period of no demonstration is more appropriate. Anisfeld argues that in Meltzoff's experiments, the observation of different actions suppressed the observer's spontaneous inclination to perform the target actions. But in the absence of imitation why should observation of a different response suppress spontaneous behavior? The suppression of target behavior suggests that the observer recognizes that the observed and target behavior do not match. And the recognition that they do not match implies that the observer would also have the ability to recognize that they do match.

In fact, the two-action method, in which whatever behavior is demonstrated is considered the target behavior and the nonobserved behavior is considered the comparison or control behavior, has become a standard method for assessing imitation in animals (see Zentall, 2001). For example, when Japanese quail observe a demonstrator quail step on a treadle (a small, flat, elevated, platformlike device), and then the observers are given access to the treadle, they show a strong tendency to step on it. However, when a different group of quail observe a demonstrator peck at the treadle, and they are given access to the treadle, they show a strong tendency to peck at it (Akins & Zentall, 1996). Furthermore, although the quail have certainly pecked and stepped before participating in this experiment, we can affirm that prior to this experiment they have never stepped on or pecked at a treadle.

The version of the two-action method used by Akins and Zentall (1996) also controls for two important nonimitative factors that might affect copying a response. First, observation of the manipulation of an object may result in stimulus enhancement (attention drawn to the object by its movement). In the case of the two-action method, however, each of the two actions results in similar movement of the treadle. Second, it is also possible for the observer to learn through observation *how* the treadle moves (downward, with a spring that brings it back up) without regard to how that movement was accomplished by the demonstrator. Such a phenomenon, often referred to as object-movement reenactment or learning of affordances (see Akins et al., 2002) is controlled for using this two-action method because the treadle moves in the same manner regardless of the behavior (stepping or pecking) that moves it. Anisfeld notes that if the imitation condition is not significantly different from a baseline (no demonstration) condition, the demonstration of an alternative behavior is not needed. However, a baseline control does not allow for the possibility that the presence of demonstrator may actually distract the observer from the demonstrated response (a negative "mere presence" effect resulting perhaps from competition with or attraction to the demonstrator; see Zentall & Levine, 1972).

Anisfeld notes that in most of the neonatal imitation studies, evidence for an increase in tongue protrusion has been found but not for an increase in mouth opening. This result allows for the possibility that the increase in tongue protrusion was mediated by an increase in general arousal. Furthermore, if the tongue protrusion by the adult model was the cause of the increased arousal, imitation need not be involved. Studies with older children (Meltzoff, 1988b; Heimann & Meltzoff, 1996) used only one target behavior, shaking a plastic egg, and various other controls (e.g., spinning an egg). Again, although it is perhaps less likely in this case, the asymmetry of the methodology (only one target behavior) allows for the possibility that an increase in general arousal could have produced this result. Use of the two-action method would have allowed them to rule out this possibility.

Finally, Anisfeld suggests that when the opportunity to perform an observed response is deferred but imitation still occurs, it suggests that the observer must have developed a representation that has been constructed mentally and retained over time. Perhaps this is true; however, Dorrance and Zentall (2001) included a group of quail that were tested 30 minutes after they observed either stepping or pecking, and the quail tested for deferred imitation showed levels of response copying similar to quail tested in the immediate imitation condition. If imitation, and even deferred imitation, can be demonstrated in Japanese quail, there is no reason why infants below the age of 11 months should not be able to imitate as well. Thus it may be that when imitation deficits are found in young infants, the deficits may reflect sensory and especially motor limitations rather than the inability to imitate.

8.6 What Does Infant Imitation Tell Us about the Underlying Representations?
Birgit Elsner on Anisfeld

In reminding us of Piaget's (1951/1962) original ideas, Anisfeld sheds new light on the discussion about imitation in infants and its underlying representations. Apparently some of Piaget's conclusions have been misinterpreted in the experimental investigation of infant imitation and are thus worth rethinking. Nevertheless, I believe that Anisfeld is too skeptical about the experimental data obtained so far. As to newborns' performance of facial gestures, he is right in stating that thus far it is not clear whether this is imitation or not. Even if it is not imitation, it tells us something about the "inner life" of newborns—what they find exciting and how they react to arousing stimuli. As for deferred imitation, I think the reviewed studies show that 6- to 12-month-old infants represent observed actions on objects and that they remember these actions for some time. These representations may not be symbolic, but still, the studies highlight some aspects that Piaget neglected. Prior to their first birthday, infants are able to acquire sensorimotor knowledge without immediate practice, and they are able to represent that knowledge over some time.

8.6.1 Imitation of Facial Gestures in Newborns

Ever since Meltzoff and Moore's (1977) seminal paper appeared 27 years ago, researchers have discussed whether the fact that newborns stick out their tongues at adults who just did so is an indicator of imitation or not. Some consistently claim that this behavior is indeed imitative (Meltzoff & Moore, 1999b), while others contend that it is not, but rather represents some reflexive arousal (Anisfeld et al., 2001) or exploration response (Jones, 1996).

Following this discussion, one wonders whether it would be possible at all to design experiments with appropriate control conditions that may help to resolve this fundamental issue. Meltzoff's attempts to show differential imitation in several modeling conditions are steps in the right direction. If tongue protrusion occurs more often in tongue-protrusion than in mouth-opening displays, and mouth-opening occurs more often in mouth-opening than in tongue-protrusion displays (Meltzoff & Moore, 1983a), this is strong evidence for neonatal imitation of facial gestures. Also, if young infants are able to shape their tongue protrusion to match a model's tongue protrusion to one corner of the mouth (Meltzoff & Moore, 1994), this speaks against a reflexive mechanism.

It certainly is often possible to question experimental evidence for methodological problems, and Anisfeld is right in pointing out the particular importance of meticulous experimental controls in research on infants. However, his statement that tongue-protrusion effects can be accounted for by arousal is a post hoc interpretation and needs separate experimental clarification. To test the arousal hypothesis, one should define in advance stimuli that arouse the newborn to different degrees and show that tongue protrusion varies as a function of these conditions. Jones (1996) used this more constructive approach and obtained evidence that tongue protrusion occurs in nonimitative contexts, for example, when newborns see blinking lights. However, such evidence does not rule out that in tongue-protrusion modeling situations, tongue protrusion may still be driven by imitative processes.

Leaving aside the question of whether neonatal tongue protrusion is imitation, what can we basically say about the underlying representations? Meltzoff and Moore (1994) speculate that an innate active intermodal mapping (AIM) mechanism drives imitation over the life-span. If this is true, however, why does the ability to imitate apparently disappear after the first weeks of life and then reappear several months later, around 6 to 9 months? This time course resembles that of neonatal reflexes. Some reflexive behaviors are present in newborns, disappear after several weeks, and reappear some months later when the behavior is subject to more voluntary control (e.g., the stepping reflex reappears in walking, the grasping reflex in goal-directed reaching). Thus, the developmental time course suggests that neonatal tongue protrusion is driven by reflexive mechanisms, be it automatic AIM, arousal, or exploration. However, the flexibility of later imitation implies that it is based on different representations than the neonatal behavior.

8.6.2 Deferred Imitation in the First Year

In his review of experimental studies on deferred imitation, Anisfeld concludes that the results do not prove the existence of deferred imitation before 11–12 months. However, in my reading of Piaget, and contrary to Anisfeld, I did not discern that Piaget-assumed deferred imitation to occur earlier than 18 months: "Hitherto [i.e., before stage VI], the child has only been able to imitate immediately movements and sounds" (Piaget, 1951/1962, p. 66). Therefore the evidence for deferred imitation beginning at 11–12 months, which Anisfeld accepts, is one of the reasons to dispute Piaget's timetable of the development of representational imitation in infancy.

Moreover, in my opinion, the reviewed studies provide evidence for deferred imitation in even younger infants. The studies use deferred imitation as a nonverbal test of infant memory, and they show that infants younger than 12 months reproduce part of an observed action sequence and can reproduce an observed familiar action with a novel object after a significant delay. Thus, the studies add to Piaget's work the supposition that babies prior to their first birthday acquire sensorimotor knowledge, not only by performing actions on their own, but also by observing others. Because Piaget concentrated on acquiring cognition by self-performing actions, he may have underestimated infants' ability to learn by observation. More important, the studies show that 6- and 9-month-olds represent the observed actions for some time without immediate practice. Anisfeld rejects this evidence because he considers the infants' reproductions to be recognition responses; the sight of the object activates the target action. Remembering a specific observed object-action association, however, requires some form of representation. It may not be symbolic, but at least it exists.

Taken together, the reviewed studies provide evidence to dispute Piaget's (1951/1962) theorizing about a qualitative, stagelike development of imitation. Evidence points to a quantitative, continuous development, which may be dependent on the emerging memory capacity. Hence, imitation starts before the first birthday with the application of known actions in new situations, and moves forward to combining actions sequentially without immediate practice. Therefore we should accept the reviewed studies on deferred imitation as what they were designed to be: nonverbal tests of infant memory.

8.7 Joining the Intentional Dance
Guy Claxton on Tomasello and Carpenter

I remember when I was about 5 years old standing watching my father doing something or other at his workbench in the garage, and after a while, saying to him innocently, "Dad, what are you trying to do?" Unfortunately he took this amiss; he thought I was questioning his competence. "I'm not trying to, I'm doing," he said, crossly. But I didn't mean that. I wanted to know what the goal was, so I could make better sense of his physical actions. Without some knowledge of where he was heading, I was having a hard time learning anything. I take it that the main aim of Tomasello and Carpenter's chapter is to validate this youthful concern of mine, and to show how early it develops.

I am a lapsed experimental psychologist turned educator. So although I am fascinated by Tomasello and Carpenter's delicate research designs, my main interest is in the implications of their ideas about imitation for real-life teaching and learning beyond early childhood. In this context, their findings about the ontogenesis of "intention reading" are powerful indeed. It makes good sense to me that as we grow up, our social learning draws on all the different aspects of imitation that Tomasello and Carpenter are at pains to distinguish. Of course it is useful to be able to mimic someone else's skilled action—to see exactly how a more accomplished piano player or arguer or teaser produces their effects. Of course it is useful to be able to

watch other people getting objects, situations, and third parties to reveal their dispositions and affordances. Watching an elder sibling teasing the dog, I learn more about the art of teasing, *and* about the dog. And, most important, I get to infer what teasing is *for*, by observing what outcomes seem to please my big sister (what Tomasello and Carpenter signify by "There!"), and what misses the mark ("Whoops!"). It is indeed useful to have these conceptually distinguished so carefully, and interesting to have some clues about their developmental sequence. In practice, though, I wonder, for people of school age and beyond, how much these distinctions matter. Would a teacher want to break down his or her modeling into separate demonstrations of action, goal, and result? Probably not. If she wants her students to be able to perform a mathematical operation, she is more likely to be effective, I think, if she treats the operations she is using, where she is heading, and what the equation looks like when it has been solved as a single unit.

Likewise, in practice, it doesn't seem too important to worry about whether the learner is imitating directly or reciprocally. As Tomasello and Carpenter rightly point out (along with several other contributors), the young child's underlying goal is not to be an accurate imitator, but to join the social dance—what Bruner called, a long time ago, the "web of social reciprocity" (Bruner, 1960). Whether I do just what you do, or whether I do something complementary (as in playing peek-a-boo, or seesawing), is probably less important most of the time than getting that delicious feeling of entrainment; of being part of the action and having a part to play in making it happen. With different kinds of partners, you learn different kinds of dances, and sociocultural theorists such as Jay Lemke (2001) conceive of growing up as the introjection of an increasing variety of dances, dancing partners, and things to do when someone treads on your toes. It seems that infants will happily join a whole range of different kinds of dances. For example, mothers and babies, one of whom is deaf, dance in a very different way from normally hearing mothers and babies, but development is jeopardized only when the dance—any dance—breaks down, as it does with mothers suffering postpartum depression.

This leads me to a major question that Tomasello and Carpenter's chapter leaves hanging: why and how intention reading itself develops. They are content to sketch out a developmental timetable, and leave the mechanisms unexplored for the moment. Yet it seems to me that intentionality—acting as if you have a purpose in mind—may itself be learned through imitative processes. Western caregivers, through both their comments and their reactions, love to engage children in the particular dance called "you meant to," obsessively attributing intention to acts that may have had no goal at all. Yet in order to join this dance fully, especially at a linguistic level, children have to learn to impute intentional states to themselves. They come to seem to want, and to talk as if they wanted, what their partner responded to in their earlier behavior *as if* they wanted. The "intentional stance" (Dennett, 1987) (or perhaps better, the "intentional trance") does not just automatically mature during the second year of life; it is systematically and diligently modeled, coached, and elicited by adults who have, in their turn, been enculturated into an intentional folk psychology. I would love to know more about how the intentional stance arises out of the intentional dance, as I am sure it must.

I have two final caveats. The first concerns mentalistic language. It is very hard to talk about intention reading without inadvertently implying that children are in some way conscious of the intentions they are reading (and transmitting). When we say a child "understands what the actor intends," or "has a theory of other minds," it is hard not to begin to slide into the Cartesian trap of assuming that there must be "something going on" in their consciousness. But there need be nothing. All kinds of systems, many of them nonbiological, display intentionality, and Rodney Brooks' robot Kismet, at Massachusetts Institute of Technology, even seems to read it, up to a point (Brooks, 2003), but there is no reason—other than ingrained Cartesian habit—to assume that intentionality entails conscious reason. I suspect children remain small zombies long after they have learned to talk and act as if they were dealing with genuine mental entities called hopes and plans. It would be interesting to know Tomasello and Carpenter's thoughts about that.

Finally, the experimental caveat. One of the reasons I am no longer a card-carrying experimental psychologist is because the "myth of presumed universality," or the "assumption of content (and context) irrelevance," seem quite untenable, even for small children and primates. To assume that you can extrapolate to basic mechanisms from a subject's performance in one task is neat, convenient, productive, always unjustified, and frequently wrong. I would like to see a wider, more ingenious array of tasks being used in research programs like those of Tomasello and Carpenter, and greater use made of sociocultural and activity theory paradigms. Indeed, imitation, broadly conceived, is one of the core processes presumed by the growing army of neo-Vygotskean researchers, and I think their methods could illuminate the work of the cognitive scientists, just as much as approaches such as those of Tomasello and Carpenter could bring some much-needed rigor to the sociocultural camp.

8.8 Two Elegant Experiments
George Comstock on Harris and Want

The two elegant experiments by Paul Harris and Stephen Want reported in volume 2, chapter 6 (1) provide convincing evidence that the age of three is a critical threshold for the appearance of selective imitation that makes use of errors in acquiring tool-using skills by observing models, (2) are given interesting and insightful interpretations that rest on the rejection of plausible but inadequate alternatives, (3) lead to the formation of hypotheses and suggested designs for future research, and (4) encourage speculation on the emergence of the ratchet effect in the Upper Paleolithic, when the development and adoption of tools changed progressively and comparatively rapidly. In effect, they constitute a very satisfying paradigm for experimental research.

The key finding is that it is only by the age of three that children become able to learn from errors made by a model in imitating the model's use of tools. At an earlier age, the observation of errors fails to enhance performance. Thus, selective imitation in which responses that achieve a goal are favored appears only after some particular stage of cognitive development is reached. It is not solely a function of the ability to imitate. The authors advance an interpretation that rests on the entire sequence of a verbally signaled complete or partial error in tool use followed by a correct example. They reject the notion that stimulus enhancement had a major role in the selective imitation because the children did not improve by merely seeing use of the objects that achieved the goal of freeing a toy man from the Visalberghi tube trap or the authors' own Y-tube. They also reject reinforcement—the observation or experience of success—because performance did not improve as a function of the number of trials. Instead, they argue that what was required for learning what not to do was the combination of an observable partial or full error, a verbal signal ("Oops!") calling attention to the error, and observation of the correct response. The first two factors apparently make the correct response more identifiable as the means to achieve the goal. This interpretation assigns importance to two elements in achieving improved tool use—one cognitive and dependent on reaching a developmental threshold, and the other social and dependent on observing error and success.

The children evidently were quite motivated to free the toy man. The authors rightly recognize this as a common characteristic of tool use because the use of a tool ordinarily achieves or makes easier the achievement of a goal, and therefore this characteristic is properly part of a paradigm for studying acquisition of tool-using skills. Nevertheless, the presence of strong motive raises the question of when in a child's development tool use may be learned simply because a tool is at hand. Such serendipitous learning, attributable in part to stimulus enhancement as well as expectations about future utility, is quite useful to everyday life. A good example is knowing how to use a fire extinguisher before it is needed in one's kitchen. The question raised is at what age do curiosity and the demand features of the tool begin to have a role. The authors' paradigm similarly invites consideration of transfer effects. Whether learning one skill improves performance of another skill has been the subject of debate since the beginnings of the discipline of psychology. The tasks employed here seem well suited to investigating this issue, and the sequential pairing of the Visalberghi trap tube and the authors' Y-tube would be an excellent vehicle for such inquiry.

The experiments do not directly address the question of what happens when children observe only the error. The implication of the authors' reasoning is that children would simply imitate the error, with no improvement in the success rate. This raises a much larger question about the social transmission of behavior. Under what conditions (other than the circumstances represented by these two experiments), if any, is it effective for a model to display errors, in short, what not to do. Although it is a big leap from the authors' data to public information campaigns in the mass media, such a "what not to do" approach was the basis of the now notoriously ineffective antidrug "Just say no" campaign.

Four obvious hypotheses are that modeling what not to do would increase in effectiveness (1) with the direness of the consequences of the error, (2) with the clarity of the portrayal of the error (a hypothesis supported in the present instance by the modest disparity between the partial and full incorrect conditions), (3) when there are few rewards or none associated with the error, and (4) when the credibility or likelihood of the undesired outcome is high. Even so, with threat appeals there remains the possibility of denial to avoid the anxiety associated with thoughts of the risk. In the case of "Just say no," all four hypothesized factors may have been weak, while denial may have been courted. Theory and experience thus suggest that the positive, rewarded outcome is particularly effective in stimulating imitation, and examples of what not to do should be accompanied by correct responses.

At first reading, the context of the broad historical sweep of human tool use seemed a literary device to stir interest rather than a topic to which the behavior of young children at the present time would be pertinent. Upon

reflection, however, the data seem insistently informative about the emergence of the ratchet effect. Reward or success seems an inadequate explanation for this effect, although perhaps the level of achievement became sufficiently enhanced to increase significantly the motive to develop tools. Another possibility is that population growth and increased contact among people made social transmission more likely. Although it may be wild speculation, the present data hint at an evolutionary factor: the development of cognitive skills analogous to those displayed at age three but not at age two, which would represent an improvement in the capacity of humans to learn from each other about the use of tools.

8.9 Against Copying: Learning When (and Whom) Not to Ape
Guy Claxton on Kinsbourne

Academic cognitive psychologists, neuroscientists, and so on are folk psychologists too. They may subscribe just as fully as lay people do to the "commonsense" psychological assumptions of their culture. They may do so just as unwittingly. And in doing so, invisible constraints are placed on the kinds of psychological approaches and ideas they are willing to countenance professionally. It takes a great deal of effort to escape from the unconscious gravitational field of these enculturated presuppositions.

This may be one reason why the "motocentric theory of perception," as Patricia Churchland calls it, keeps being forgotten and rediscovered (Churchland et al., 1994). It is countercultural in at least two senses. It refuses to place "perception" at the front of a sequence of cognitive operations, and in doing so it refuses to divide the brain into boxes with textbook labels, with "vision" here, "motor programming" over there, and "motivation" somewhere else. The motocentric view is harder to think about than the commonsense view because it is messier and more "holistic," as well as being culturally "strange." However, it is undoubtedly a more accurate representation of human cognition (and animal cognition, come to that).

Perception is not the extraction of a (more or less) accurate representation of the external world, upon which cognition proper then works. It is imbued with the selective influences of desire and capability right from the start. Perception, emotion, motivation, and action are functionally so tightly intertwined that it is misleading to think of them as separable systems at all. Jakob von Uexküll's delightful essay "A stroll through the worlds of animals and men," demonstrated this vividly in the 1950s (von Uexküll, 1957). James J. Gibson got part of the way there in the 1960s with his notion of "affordances" (Gibson, 1966). In the past few years such scholars as Andrew Clark, Patricia Churchland, Susan Hurley, Francisco Varela, and George Lakoff and Mark Johnson in their *Philosophy in the Flesh* have been having another go at formulating the motocentric view (Clark, 1997; Churchland, 2002; Hurley, 1998; Varela et al., 1991; Lakoff & Johnson, 1999). And now Marcel Kinsbourne's elegant ruminations in chapter 7 are designed to convince us that the renewed study of imitation, mirror neurons and all, does more than merely show that perceptual and motor "systems" are more tightly coupled than commonsense Cartesianism would have it. He reminds us that sensing, doing, feeling, and wanting are simultaneous facets of one rapidly unfolding, complex, neurodynamic process. The perceptual ends of the neural net are continually being affected by evanescent patterns of priming that reflect the momentary senses of capability, feeling, arousal, and need. Max Clowes used to say "There's no seeing, only seeing *as*" (personal communication); but it's worse than that: there's only seeing *for*.

Some of these sources of bias in perception are built in. I am inclined to agree with Kinsbourne, Trevarthen (1999), and others that the desire for "interactional synchrony" is innate; that babies will unearth and do whatever it takes to achieve entrainment (even if it risks damaging other aspects of their development); and that the "joint attention" that ensues forms the basis for the learning of language and much else. However, the part of the story that particularly intrigues me is the development of the inhibitory mechanisms that make this tendency to entrainment both more covert and more selective. Instead of entraining through simple forms of copying or reciprocating, we learn to keep it inside, and turn public imitation into private imagination. [There is neuroimaging evidence that "mental rehearsal" of, for example, piano scales, produces very nearly as much neuronal growth as actual practice (Pascual-Leone, 2001).]

We also learn to seek entrainment with a restricted circle of family, friends, and acquaintances—to inhibit the imitative impulse in the presence of some (kinds of) people, and to release it (albeit increasingly covertly) in the presence of others. Kinsbourne explains that privacy and selectivity are useful options if for no other reason than that imitation entails learning, and you cannot learn "everything." It seems as if we have two kinds or levels of inhibition at work here: one that curtails the external concomitants of mirror neuron activation but leaves cortical activation patterns relatively unaffected (and unattentuated), and another that prevents even these cortical patterns from forming (and thus blocks learning).

It seems likely that these developments of inhibitory sophistication continue well beyond infancy and thus may be of consequence in educational as well as familial settings. Though it is an enormous inferential leap from the laboratory to the classroom, one cannot help but be struck by the intense imitative selectivity of the average adolescent (as well as by the prevalence of those forms of covert identification that constitute the majority of daydreams). If it is true that mental rehearsal influences the development of the brain, then time spent "being Madonna" in the private theatre of your imagination will have a direct effect on the development of Madonna-like traits and habits. At the same time, the circuitry corresponding to the mathematical operations that Mr. Chips is trying to get you to learn will have lost the neural competition and been dampened down—as will the residual inclination to pick up Mr. Chips-like traits and habits, also.

In the context of sociocultural approaches to education, the latter is an important issue, for education is not only about the development of bodies of knowledge, skill, and understanding; it is (if Vygotsky, 1978, is right) about the progressive enculturation of a set of attitudes and values. These beliefs are communicated by a whole variety of means: the curriculum (mathematics is a different subject from physics), the timetable (learning can be turned on and off at will), assessment (what you know is what you can recall and write down under pressure), and so on. However, the most powerful, perhaps the determining force, is the mien of the teacher. Or it is unless the "interactional synchrony" of the student-teacher relationship has been blocked. Kinsbourne's approach invites us to explore when and how these acquired barriers to interpersonal transfer develop and are raised, but disappointingly, he fails to pursue the question himself.

Being face-to-face is neither a necessary nor a sufficient condition for imitational transfer to take place. It is not necessary because internalized models of other people can be used as the basis for imitation, as in daydreams, and it is not sufficient because Mr. Chips so often fails in his efforts to communicate his love of math. (Indeed, there may be some students in his class who resolve that he is exactly how they do *not* want to turn out.) There has to be a certain quality of relationship, we must suppose; call it respect, admiration, possibly just affection (which creates positive affect, and allows the pores in the "osmotic membrane" between brain and brain to open and each to be affected by the other.)

In sum, I found Kinsbourne's chapter philosophically congenial and practically stimulating. My main regret is that he sprays out fruitful ideas in a way that varies from the terse to the cryptic. I do hope that he finds the time to spell out all the interesting ramifications of his approach at greater length, before too long.

8.10 Imitating Violence
Susan Brison on Kinsbourn

According to Marcel Kinsbourne (in vol. 2, ch. 7), imitation is a form of entrainment, or "adopting shared rhythms of behavior" (p. 167), which is "more innately compelling than reasoned argument in inducing two, or many [persons], to adopt the same point of view" (p. 172). As a philosopher interested in theories of freedom of expression (Brison, 1998a,b) and in the effects of violence on the self (Brison, 2002), I find this view both refreshing and disturbing. It is refreshing in contrast with the overly rationalist, indeed Cartesian, view of free-speech theorists who assume that we are all rational, autonomous, and conscious information processors and decision makers. It is disturbing because if it is true, it indicates that we are naturally more prone to imitate media violence than free-speech theorists and public policy makers have so far been willing to acknowledge.

On April 4, 2002, I drove to the Grafton County courthouse in North Haverhill, New Hampshire, to attend the sentencing hearings of Robert Tulloch and James Parker, the two teenage boys who had pleaded guilty to the murders of my friends and colleagues, Half and Susanne Zantop. We heard, from the assistant attorney general, about the gruesome stabbings and about the state's case against the defendants. One of the things we learned was that the boys possessed—and had enjoyed playing for hours on end—a particularly violent and realistic interactive video game in which the player stabs his victims and watches them as they bleed to death.

That afternoon, I picked up my 7-year-old son from school and noticed that his school librarian had sent home a recent article from a local Vermont paper, the *Times-Argus*, entitled "Video game violence: harmful to society or just harmless fun?" It began with a quote from *Electronic Gaming Monthly*: "If you've ever wanted to run through a crowded mall while mowing down innocent shoppers with an M-16, or take a grenade launcher to storefronts and parked cars, [State of Emergency] is your game. [It] offers violent, vicarious thrills that are socially unacceptable, brazenly immoral and a helluva lot of fun."

What are the effects on children of violent video games and other forms of media violence as entertainment? What are their effects on adults? No one supposes that every child or adult who plays with or watches violent entertainment goes on to commit criminal acts, or even becomes more likely to do so. And many violent criminals (most of them, presumably, at least until very recently) have had no exposure to such violent entertainment. But this does not mean that there is *no* probabilistic causal connection between exposure to such media and the commission of violent crimes (just as the fact that not all smokers get lung cancer and some people who get lung cancer never smoked does not indicate the absence of a causal connection between smoking and lung cancer).

Not only are violent interactive video games cause for concern, given the desensitizing and disinhibiting effects they may have on those who play them, but there is evidence that even passive viewing of representations of violence can, in some contexts, have disinhibiting effects on some viewers' tendencies to imitate what they see. Kinsbourne's chapter indicates that the phenomenon of imitation is more pervasive and complex—and more central to human behavior—than we previously realized. His research suggests that the human drive to imitate others' behavior can undermine our autonomous decision-making processes—a finding that has important implications for a defense of free speech based on the view that citizens, as autonomous agents, have a right to unfettered freedom of expression and to unrestricted access to others' speech.

Even if media violence can be shown to have harmful societal effects, that finding by itself is not enough to warrant the governmental restriction of such speech, in the United States, anyway, since the free speech principle embedded in the First Amendment of the U.S. Constitution indicates that even *harmful* speech is worthy of special protection against government interference. As I have argued (Brison, 1998a), if speech is harmless, then there is no need to give it special protection, since a background assumption of our constitutional democracy is a general principle of liberty stating that the government may justifiably interfere with individual liberties only to prevent people from harming others.

What can be the reason for protecting even harmful speech? Numerous defenses of a special free-speech principle have been given, including the argument from truth, the argument from democracy, and the argument from autonomy. All of them presuppose that speech (which, under First Amendment doctrine, includes such things as graphically realistic violent films and video games) has no (or merely negligible) effects that are not under the conscious control of the audience. So, even if it can be shown that watching violent films and video games leads to an increased tendency to violence in the viewers, it is argued that the *viewers*, not the media, are entirely responsible for the violence because they consciously and autonomously choose to be influenced by what they see (and what they do, in the case of interactive video games). The violence is considered to be entirely due to the mental intermediation of the viewer—a conscious intervention that is assumed to break the chain of causality from the viewing of violent scenes to the committing of violent acts.

As Susan Hurley has argued, however, the research by Kinsbourne and others suggests that the imitation of others' behavior, including others' violent acts, is not always a consciously mediated process that is under the autonomous control of the viewers or imitators.[4] It might be argued that if we consider violent media to be even partially responsible for the violent behavior perpetrated by its consumers, then we must consider the perpetrators *not* responsible. In conversation, the assistant attorney general in the Zantop killings case told me that had the case gone to trial, the killers' frequent playing of this particular violent video game would have been used as evidence, not by the prosecution, but by the *defense*, as part of an insanity plea, in an attempt to show that the killers were not responsible for their actions. However, it does not follow from the claim that violent media cause people to be violent that the perpetrators are not 100% responsible for their violent acts. Two or more people can each be 100% responsible for the same crime, as in the case of multiple snipers who simultaneously fire many shots, fatally wounding their victim. If people are entrained, to use Kinsbourne's term, in violent behavior by their ever-greater exposure to increasingly violent media in our society, then we, as citizens, have to start taking responsibility for the violence that results.

4. Susan L. Hurley makes this argument in her excellent article "Imitation, media violence, and freedom of speech" (2004).