

Cultural learning: Are there functional consequences?

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Two problems commonly surface when scientists attempt to provide evolutionary accounts of human cognition. First, proximate causation (i.e., mechanism) is often viewed as an alternative to ultimate causation (i.e., adaptive function). It is not. Second, performance on a task is equated with cognitive ability. This is only sometimes true.

In this commentary I raise two objections. I claim that a more complete evolutionary account of human culture will require the integration of a more explicit functional stance, exploring the fitness consequences of the different mechanisms of information transmission in the social arena. This claim is important for the second point, which is that current assessments of the "special" nature of human cognition depend on the reliability, robustness, and interpretation of current work on nonhuman animal cognition. I believe this is a weak foundation for building evolutionary theories of cognition.

Tomasello, Kruger & Ratner believe that cultural learning and the products it generates are the result of cognitive processes whose origins are within the hominid line rather than within the more ancient hominoid or even primate line. This claim is difficult to defend because it relies completely on the strength of experiments with extant nonhuman primates. As the history of research on animal intelligence warns us, we must be careful to distinguish between ability and performance. Because so few controlled experiments have been conducted to assess the cognitive abilities of our closest relatives (and I would include all of the nonhuman primates, not just chimpanzees), I don't believe that current performance is actually indicative of ability. I suppose that in this sense, I am a closet MacPhailian who believes that it takes considerable work to demonstrate the lack of an ability. We are a long way off from claims regarding the representations, beliefs, and intentions of monkeys and apes.

Tomasello et al. want to rule out some of the observations of chimpanzee culture and imitation as bona fide examples because they do not meet some of the stringent criteria that have been set up. I found, however, that their discussion underestimated the potential importance of nonhuman animal work and in particular underemphasized the necessity of considering the function of different mechanisms of transmission and the evolutionary consequences they bring. Consider for the moment the data on putative teaching in wild chimpanzees (Boesch 1991). Tomasello et al. state that, in two cases, mothers appeared to "slow down their tool use when infants were watching." This is completely inaccurate. In one case the mother reoriented the way in which her infant was holding the tool (i.e., a hammer used to open nuts), and in the second case a mother reoriented the position of the nut on the anvil to increase the probability of a successful hit. This direct intervention on the part of the mother suggests both that she recognized how things should be done and that she recognized a deficiency in her infant with regard to the requisite motor task. More important, as Caro and Hauser (1992) have argued in a review paper on teaching in nonhuman animals, *instructive* interactions do not require the ability to attribute mental states to others. This is because from a functional perspective what counts is whether the interaction leads to significant fitness consequences. And in many recent cases of socially mediated learning, there are clear fitness consequences such as increased survivorship (e.g., Aisner & Terkell 1992; Hauser, in press). Although Tomasello et al. cite our paper as providing a "more generous interpretation of animal teaching," it has nothing to do with generosity. Rather, our view reflects an emphasis on the adaptive function of teaching as opposed to the

mechanisms of teaching. Thus, to summarize my first point, if evolutionary account of human cognition is desirable, Tomasello et al. clearly seem to suggest, then it is absolutely essential for the theory to consider the function of different mechanisms of transmission and thus why the hominid line developed its own special breed of cognitive abilities – if it did.

My second point concerns the claim that nonhuman primates lack the appropriate cognitive capacities to engage in cultural learning. We now know from the work of several neuropsychologists that nonhuman primates have the ability to represent, over fairly long periods of time, complex visual and auditory information. Among the key neural players are the prefrontal cortex, amygdala, and hippocampus. The strong claim of the target article by Tomasello et al. is that nonhuman primates, in particular chimpanzees, lack the ability to take the perspective of others and thus lack the ability to make use of intentional instruction or to engage in intentional collaborations. I would like to suggest that our nonhuman primate relatives have the neural machinery to engage in the sorts of cultural learning discussed by Tomasello et al. and that there are in fact cases that are at least as suggestive as those described for preverbal human infants. For example, Stambaugh (1988) has shown that when a subordinate individual is trained to perform a task that yields extremely valuable goods (i.e., food) and no other group member is capable of performing the task, the subordinate's status in the group changes. Specifically, although dominant individuals would normally supplant a subordinate from food, the subordinate individual is now allowed to keep some of the goods as long as he continues to perform the task. In this example, there is clearly collaboration and, although the individuals involved may be basing their actions on the behavior of others rather than their mental states, it implies a level of sophistication that I believe Tomasello et al. fail to consider. And, in the cases involved, there are clearly functional consequences to the collaborative effort.

In conclusion, although I would very much like to see Tomasello et al. place their framework in a more functional based perspective, I applaud them on their effective integration of issues in the cognitive sciences, developmental psychology, and behavioral biology. They have certainly provided those of us working on nonhuman animals with some theoretical targets to shoot at and for.

Imitation without perspective-taking

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Tomasello et al. offer a hypothesis to explain the unique characteristics of human traditions. They have identified features that distinguish human and nonhuman traditions with unusual clarity and their hypothesis is intriguing, but the target article leaves me unconvinced that social-cognitive abilities such as perspective-taking play "a vital role" in cultural learning.

My doubts about the relationship between social-cognitive abilities and cultural learning were aroused principally by Tomasello et al.'s discussion of imitation. They suggested that perspective-taking is logically necessary for imitation and that there is convincing empirical evidence of a link between the two. Both these propositions are, I shall argue, false and their cooccurrence is a sign of confusion. If imitation implies perspective-taking then it would be unnecessary to seek empirical evidence that the capacities for imitation and perspective-taking coincide. Instead of opting to rest their case on one claim or the other, Tomasello et al. have, I suspect, allowed false preconceptions about the psychological processes underlying

imitation to guide their interpretation of the empirical evidence.

Tomasello et al. suggest that perspective-taking is logically necessary for imitation in section 2.2: "Reproducing an adult's novel behavior in both its form and appropriate function (i.e., imitative learning) clearly requires some understanding of what the adult is perceiving and intending because without such understanding the child cannot know which aspects of the adult's behavior are relevant or irrelevant." There are in fact at least two ways in which an individual could achieve form and function or action and outcome reproduction without perspective-taking (Heyes 1992). First, if individuals observe a model executing several different action-outcome sequences (e.g., see a ball being thrown underarm and overarm and landing in different places), then they might learn that certain, observable features of the action are predictive of certain, observable outcomes, and selectively reproduce those features that are predictive of the outcome that is desired. This process may be described as action-outcome contingency learning by observation. Second, an individual could "mimic" a model's behaviour, that is, reproduce the action without expectations about its outcome, let alone any apprehension of the model's goal (if any) in performing the action. Tomasello et al. acknowledge the possibility of mimicry without saying how, in general, it may be distinguished empirically from imitation; and they overlook the possibility of action-outcome contingency learning altogether.

Tomasello et al.'s case for empirical rather than a logical link between imitation and perspective-taking is also unconvincing, for at least two reasons. First, it dismisses without proper justification evidence of imitation in creatures that are thought to be incapable of perspective-taking. For example, the capacity of newborns to imitate facial movements and head turnings (Meltzoff & Moore 1989) is discounted on the grounds that it does not involve "the acquisition of novel behaviors," but it is not clear why this fact might protect Tomasello et al.'s hypothesis from disconfirmation by Meltzoff & Moore's (1989) data. The requirement that a behaviour be novel in order to qualify as evidence of imitation was imposed when researchers could not think of another reliable means of distinguishing imitation from the chance occurrence of matching behaviour (e.g., Thorpe 1956). Now that the possibility of chance or coincidental matching can be eliminated using, for example, Meltzoff and Moore's (1989) "cross-target" procedure, continued adherence to the novelty rule requires explanation. Tomasello et al. might explain their adherence by pointing out that the reproduction of behaviours already in an individual's repertoire is unlikely to effect behaviour transmission. This argument, however, would make Tomasello et al.'s claim that imitation is a form of cultural learning rather vacuous. If imitation were defined, in part, as a learning process that effects behaviour transmission, and if the potential to effect behaviour transmission were judged to be sufficient to make a learning process "cultural," then imitation would inevitably count as a form of "cultural learning." (Because "cultural accumulation" is likely to depend at least as much on processes of information retention as on mechanisms of information acquisition, I would also contest the second premise above, but that's another story - Heyes 1992).

Second, Tomasello et al.'s review of the literature is too narrow to take account of the fact that when imitation is defined as the reproduction of the form and function of a model's novel behaviour there is evidence that both budgerigars and rats can imitate. Budgerigars that have observed a conspecific using either its beak or its feet to lift a flat cover from the top of a food cup tend to use the same appendage as their model to lift the cover (Galef et al. 1986). Rats that have observed a conspecific pushing a joystick in one of two directions for food reward tend subsequently to push the joystick in the same direction (relative to the actor's body) as their model (Heyes & Dawson 1990; Heyes et al. 1992). As far as I know, no one has tried to find out whether budgerigars and rats can attribute intentions; hence

these experiments cannot be said to indicate with certainty that perspective-taking is not necessary for imitation. However, since our reasoning about vermin and budgies may be freer of anthropomorphic bias than our reasoning about children and chimpanzees, they are a reminder that the link between imitation and perspective-taking is a bold, largely unsupported hypothesis rather than a necessary truth.

Turning finally from imitation to "cultural learning" more generally, I found Tomasello et al.'s remarks about "enculturated" and "nonenculturated" chimpanzees inconsistent with their claim that social-cognitive abilities such as perspective-taking are required for imitative, instructive, and collaborative learning. If their suggestion were valid then how could a "non-enculturated" chimpanzee deficient in the relevant social-cognitive abilities become an "enculturated" chimpanzee that had sufficient social-cognitive ability to be capable of cultural learning? Tomasello et al.'s concession that such a transformation may occur implies either that social-cognitive abilities result from, rather than give rise to, imitative, instructive, and collaborative learning, or that there is some other variety of cultural learning that does not require social-cognitive ability. Thus, far from providing convergent evidence that certain social-cognitive abilities are necessary for cultural learning, Tomasello et al.'s interpretation of the chimpanzee data apparently contradicts that conclusion.

On acquiring the concept of "persons"

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In their description of cultural learning, Tomasello, Kruger & Ratner offer a timely and persuasive account of the emergence of certain forms of perspective-taking that are unique to human beings. The developmental story is couched in terms of the child's growth in understanding about the nature of "persons." I believe there is much to be said for adopting such an approach. Tomasello et al. suggest that in successive stages of development, a young child operates with different "concepts" or "constructs" of the person: the intentional agent, the mental agent, and the reflective agent. I shall concentrate on the first of these categories.

The initial stage of cultural learning corresponds with the infant's ability to learn through imitation of another person. Such behavior appears towards the end of the first year of life and requires that "the child must understand the demonstrator in terms of his intentions toward things (i.e., as an intentional agent) in order to distinguish the relevant and irrelevant aspects of the demonstrator's behavior" (sect. 2.4, para. 5). In this regard, Tomasello et al. emphasize the child's understanding of what the adult is perceiving and intending. At several points they also state that cultural learning occurs when the child attempts to understand or perceive a situation in the way that another person understands or perceives it.

Two questions arise. First, what is the *kind* of "understanding" these infants have attained? Second, how is such understanding acquired?

I am doubtful whether it is appropriate to talk about the infant's "concept" of persons at this early period. I say this, even though I strongly agree that this is an important stage in the acquisition of such a concept. Tomasello et al. seem to suggest that in early imitation, infants themselves have the goal of approximating their own actions *and intentions* to those of another person, conceptualized as such. They are supposed to have a concept of persons and a sophisticated degree of self-reflective awareness, so that they can knowingly adopt or try to adopt the psychological stance of someone else. I doubt whether