

In ontogeny, the first signs of self-preoccupation and self-other compulsive comparison become evident by the middle of the second year, when children start to show not only explicit self-recognition (Lewis & Ramsey 2005), but also unmistakable signs of embarrassment in front of mirrors (see Rochat, 2003, for a developmental account of emerging coawareness). By their third birthday, children express pride, shame, and other secondary or evaluative emotions (Kagan 1981; Lewis 1992). By the time children start to blush, they also begin to lie. They edit and cover up truth to keep face in relation to others in potentially embarrassing circumstances (Lewis et al. 1989; Polak & Harris 1999).

In his seminal work comparing the expression of emotions in man and animals, Darwin (1965) viewed shyness (embarrassment) as a precursor of blushing. He witnessed blushing in his son at around 3 years and shyness months earlier, pointing to the fact that blushing causes the selective crimsoning of the face, precisely the region of the body that is most visible and attended by others. It is the face that is typically and desperately covered in bouts of embarrassment when feelings are exposed. Following Darwin, this is a unique product of human evolution. It is also the expression of a unique psychological process: the never-ending process of intersubjective negotiation by ways of active self-presentation.

Only humans engage, at least to the extent they do, in self-editing and self-advertising via, for example, body adornments and alterations (e.g., plastic surgery, tattoos, piercing, and makeups). These practices are pervasive across ancient cultures – for example, some 4000 years ago in ancient Egypt (Bianchi 1988). The well-preserved 5,000-year-old frozen body of the “Iceman” found a few years ago in the Austrian Alps shows, aside from an arrow wound, deliberate symbolic scarring and tattoos (Fowler 2001). Even older human remains of Pleistocene Australian aborigines (12,000-year-olds and up) suggest deliberate body alteration, in particular forced skull elongation (Brown 1981). All that is part of the basic human need to affiliate. They are signs of deliberate acts of self-presentation and therefore the expression of active, reciprocal negotiation of values and affective experiences with others.

My intuition is that, in evolution, the motivation to negotiate and reciprocate preceded humans’ unique ability to understand and share intentions. In an analogous way, in ontogeny, the need to reciprocate is a necessary condition to the emergence of theories of mind. New, more sophisticated understandings of the self and of others emerge from the primary motivation of finding some agreement on the values of all things: a universal trademark of human cultures.

Distinctive human social motivations in a game-theoretic framework

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Abstract: I discuss implications of Tomasello et al.’s hypothesis that humans exhibit distinctive collective intentionality for game-theoretic approaches to modeling human evolution. Representing the hypothesis game-theoretically forces a question about whether it implies only distinctively human motivations or both distinctive motivations and distinctive cognitive capacities for representation of intentions. I also note that the hypothesis explains uniquely human ideological conflict and invites game-theoretic modeling of this.

The perspective on cultural cognition urged by Tomasello et al. is persuasive in light of the evidence they cite and constitutes a significant advance in our understanding of what is ethologically and developmentally distinctive about *Homo sapiens* in comparison with other apes. *Homo sapiens* is not just, or even mainly, a

uniquely adept mind reader; she is, more fundamentally, an animal that collectively constructs the special cultural entities we call *people* in the course of coordinating around joint projects that have joint-ness itself, and not merely the achievement of environmental changes, as part of their point. In this commentary, I discuss some implications of this perspective for the ways in which we formally model human evolution in game theory. A central purpose of formal modeling is of course to discipline thought. It is thus encouraging that asking questions about the formal representation of Tomasello et al.’s thesis invites some further questions about refinements to that thesis on which they are not fully clear.

In recent work (Ross 2004; 2005; forthcoming), I have argued that, in constructing evolutionary game-theoretic models of human history, it is necessary to mark certain sorts of *ontological phase shifts*, with respect to both types of agents and types of games, in our formalism. First-generation evolutionary psychology obscured this. In particular, approaches such as those collected in Barkow et al. (1992) encouraged conception of modern people as agents with utility functions evolved for a Pleistocene ancestral environment trying to optimize under novel circumstances. I have argued, in contrast, that human organisms are under pressure from birth to narrate distinctive *selves* into existence for the sake of stabilizing behavioral expectations – for others and for themselves – so as to facilitate coordination. Since these selves have different utility functions from *both* ancestral hominids and pre-enculturated infants, they play a range of games drawn from a different selection space. Nevertheless, there must be constraining relations among the games played by early hominids, modern infants, and enculturated people. (The evolutionary perspective tells us there is information flow of a systematic sort among the instances of these kinds of games.) My work has aimed at modeling these relations without reducing one class of games to another. Summarizing maximally broadly, a modern infant’s utility function is an output of a class of evolutionary games G'' played among lineages. Infants are enculturated through play of a class of repeated games G' with adults. G' -level games turn infants into new agents with new utility functions. These agents play classical games of class G with one another, as described by economists and sociologists. Then statistical distributions of adult human behavioral patterns should be simultaneously consistent with short-run equilibrium conditions governing G -level strategies, medium-run equilibrium conditions governing G' -level strategies, and long-run equilibrium conditions governing G'' -level strategies. Binmore (1998) has advocated a similar picture less explicitly. The contrast between it and models of the Barkow et al. type may be glossed thus: according to first-generation views in evolutionary psychology, modern people can be modeled as generic apes with “social wraparounds”; according to Binmore and me,¹ this is seriously misleading.

The hypothesis defended by Tomasello et al. lends itself to formalization in the framework I have urged. Humans’ most recent common ancestors with chimpanzees and bonobos should be modeled as products of G'' -level games that have (at least) two long-run basins of attraction: a basin in which G' -level players receive no utility from cooperation for its own sake (that is, over and above utility from environmental contingencies induced by cooperation) and a basin in which cooperation for its own sake *is* a source of utility. Contemporary chimps, both infants and adults, play games drawn from the first basin; contemporary people play games drawn from the second. Nonhuman apes do not play G -level games.

Expressing their hypothesis in this modeling framework raises a question for Tomasello et al. They suggest that people share cognitive capacities for representation of intentional structures with other apes, but are distinguished from them by a motivation to converge on what could be called “collective” utility functions. They also survey evidence that people, but not other apes, represent intentions “dialogically.” Now, in the game-theoretic framework I have described, differences in cognitive skill sets are relevant to available strategy spaces in games, but, unlike differences

in utility functions, do not necessarily imply re-individuation of games themselves. Thus, addressing the point at the level of the phenomena, the fact that chimps do not use dialogic representations of intentionality may just result from the fact that they are not motivated to do so, rather than from limitations in their cognitive architecture. None of the experiments discussed by Tomasello et al. seem to provide a basis for discriminating between the hypotheses that (1) humans retain the generic ape cognitive architecture with modified utility functions, and (2) motivational adaptations in hominids led to cognitive adaptations in them. For example (citing a case they discuss), does Kanzi show sharply limited linguistic skills because he lacks the relevant Chomskyan module, or because he is interested only in getting objects he wants from people and has no interest in negotiating meaning? Tomasello et al.'s argument might motivate design of experimental protocols that could discriminate between these hypotheses, perhaps by putting language-trained chimps in situations where they can satisfy their first-order desires only by negotiating meanings. This would of course require some ingenuity on the part of the experiment designer.

In closing, I note one aspect of human behavioral distinctiveness that Tomasello et al.'s hypothesis explains, but that they do not mention. As far as we know, only people engage in violence in order to try to eliminate beliefs that diverge from their own. There is a long tradition of explaining ideological conflict by reference to conflict over material resources. This cannot explain why there are no homologues to such behavior in other intelligent social animals. Thanks to Tomasello et al., we can advance a better explanation: only people care nonderivatively about not only who gets what, but about whether others want the same things as they do. Modeling the evolutionary dynamics of these sorts of interacting motivations is a compelling new challenge for game theorists.

NOTE

1. Clark (2002), Dennett (2003), and Sterelny (2004) all provide supporting philosophical arguments for this view.

Why not chimpanzees, lions, and hyenas too?

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Abstract: Examples are cited of group hunting in chimpanzees, lions, and hyenas consistent with evidence for intentionality, organization, and coordination. These challenge the claim for shared intentionality as uniquely human. Even when rarely performed in this way, the significance of such behaviors should not be minimized, especially if this level of "intelligent" action emerges spontaneously in the wild.

The target article sharpens the terms of an ancient debate – What distinguishes the modern human *Homo sapiens sapiens* from all other animals? – by acknowledging that species such as the chimpanzee *Pan troglodytes* possess the cognitive complexity for social strategizing, sensitivity to the intentional actions of others, and glimmers of a theory of mind (Dennett, 1983; Povinelli 1993; Tomasello & Call 1997). The authors also acknowledge the prevalence of animal *intentionality* when behavior is consistent with the ability to choose a plan of action and stay with this plan to achieve a predetermined goal. Instead, they place the animal–human divide in the realm of *shared mental states*, and specifically in the ability of humans to use *shared intentionality* (hereafter SI). This is expressed in "collaborative activities with shared goals and intentions," consistent with "shared psychological states" and unique forms of "cognitive representation." Even if some animal species are capable of understanding the goals, intentions, and perceptions of others, only the human possesses the motivation to share these things in interaction with others.

Since animals are being compared with humans, the argument hinges on documenting observable actions (see Tomasello et al., Fig. 1) and *specifying testable behavioral criteria* for making inferences about underlying SI. This commentary asks whether SI might underlie the apparent convergence between the performances of humans and some animals when *individuals cooperate by coordinating actions for shared outcomes*. Intuitively, *cooperative coordination* (hereafter CC) offers an obvious place to search for SI because individuals develop conjoint actions for shared outcomes based on using each other's behaviors and locations. We have modeled this in rats *Rattus norvegicus* (Schuster 2002; Schuster & Perelberg 2004). But the argument is better made from three examples of CC expressed spontaneously in the wild without the aid of behavioral engineering. All are in the context of group hunting.

In the chimpanzee *Pan troglodytes*, hunts have been described and filmed that are spontaneous and highly organized: a group first gathers and then simultaneously fans out in search for a victim while reducing the chances of detection by avoiding vocalizations and using slow and careful steps to minimize noise (Boesch & Boesch 1989; Mitani & Watts 2001). There is also a division of labor based on roles that was characterized by Boesch and Boesch (1989) as *collaboration*: a "blocker;" a "chaser;" an "ambusher;" and others remaining on the ground to track the hunt and intercept a fleeing target if the opportunity arises. Chimpanzees behave similarly in group territorial "warfare" against members of neighboring groups (Boehm 1992; Watts & Mitani 2001).

Tomasello et al. reject the foregoing as evidence for SI by suggesting that individuals are indeed *acting together* but not collaborating in a way that provides undeniable evidence for joint intentions and coordinated plans. Instead, each participant is said to be performing its own particular role as a response to the locations and behaviors of others and the momentary "state of the chase." Hunts are thereby characterized as helter-skelter running in all directions, with the lucky hunters opportunistically making a kill if an unfortunate victim comes their way. Stanford et al. (1994) suggest that the hunts observed at the Gombe Reserve site are mostly of this type.

Have Boesch and Boesch (1989) exaggerated the levels of intentionality, spontaneity, and organization? Or, as seems more likely, chimpanzee hunts may run the gamut from random and disorganized to deliberate and planned, with the likelihood of each varying both across populations and within populations but across seasons and locations. If so, it would be misleading if the examples of opportunistic and random attacks were used to cancel out the significance of those instances *when chimpanzees do engage in organized, intentional group attacks*. Why would we expect chimpanzees – or any other species, including our own – to rely exclusively on the most complex tactics if success is achievable by lesser means? Moreover, intelligent, creative action is almost by definition limited to *some* members of a population and then only on those limited occasions when *automaticity* in actions and thoughts does not work (Bargh & Chartrand 1999). This variability is reflected in the current criteria for publishing research on themes linked to animal intelligence – political scheming, tool use, deception, or theory of mind – where data may be cited from only those few subjects that exhibit the phenomenon or even from $n = 1$ (e.g., see Premack & Woodruff 1978). The hypothesis of SI in chimpanzees should not be rejected even if organized and intentional hunts are rare.

More interesting for SI in animals is the performance of group hunting in less "cognitively endowed" mammals such as the African lion *Panthera leo* and the spotted hyena *Crocuta crocuta*. The overall picture resembles that in chimpanzees. Schaller (1972) noted that lions are usually opportunistic hunters capable of chasing after prey that suddenly appears, whether acting either alone or in groups that happen to be there. But he added,

[O]n 29 occasions lionesses encircled prey, sometimes by detouring far to one side. . . . The other lions waited during the flanking movement