alliance? and what was the selective rationale for increased paternal and maternal investment in offspring (in terms of both intensity and duration)?

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By scouting the boundaries between social anthropology, primatology, and human sociobiology, this attempt to "encourage the kind of intergroup affinity that advances scientific understanding" does not consider the boundaries *physical anthropology* shares with these disciplines. Perhaps this is for the best. Just slightly more than 25 years ago it was a physical anthropologist who suggested that a very productive way to understand humans might be to take an "ape's-eye view of human evolution" (Washburn 1965). Now we have come a full lap around the track, as this independent scouting trip trots out the same proposal.

Washburn's attempt was very insightful and its influence far-reaching. His idea was to examine the great apes to outline where humans became different, with the intent of using the paleoanthropological record to hypothesize about why these differences appeared. Rodseth et al. use the comparison with the higher primates in a subtly different way, taking the primate context as an explanatory basis for human variation.

Perhaps an analogy with locomotor analysis can clarify the difference. All primates use a complex and varied locomotor repertoire, with the variability depending on ecological, social, and, yes, no doubt inclusive-fitness-related variables. The Washburn approach would be to use comparative studies to discover what is truly unique about human bipedalism. Here it proves to be not the behavior itself, as many primates are regularly bipedal, but the fact that human bipedalism is *obligate*, with consequences influencing the understanding of its origins. Rodseth et al.'s comparative approach would seem to use the pattern of primate variation to understand human bipedalism, assuming that the *causes* of the human pattern are to be found in the commonalties underlying bipedalism in other primates.

While this sounds reasonable, it ignores a fundamental difference. Humans are necessarily always bipedal, not bipedal by choice under certain circumstances, and this undermines the comparative approach for explaining its function because the universality of this locomotor pattern makes it the *context* rather than one possible *consequence.* Any locomotor pattern, including obligate bipedalism, could of course be described in terms of its constituent elements, along "species-neutral" dimensions, but this would be reductionism at its worst (Gould and Lewontin 1979) and is probably the only sure way to misunderstand human locomotion.

Turning to the behavioral comparision, while Rodseth et al. recognize that language (and, I would add, more generally culture, which in humans is language-dependent) is unique to humans, this has greater importance than the absence of language from the behavioral repertoire of other primates might suggest. Human language provides the context for the human behaviors these authors endeavor to compare and is not simply a unique aspect of "another unique species" that allows one to explain how humans maintain social relationships in the absence of spatial proximity. The human community is based on stratified roles whose definitions, expectations, and relationships are language-dependent, with the result that gender rather than sex is a primary variable, and kinship is a social institution rather than a categorization of relationships due to common ancestry.

Why not "Primate Society as a Human Community"? Shouldn't what's good for the goose be good for the gander? I believe that it is clear why not and prefer the conclusion that primates may be validly regarded as a fascinating group of species, deserving of study for perfectly good reasons, without the necessity of using those studies better to understand ourselves.

[Rodseth et al.'s reply will appear in the August-October issue. —EDITOR.]

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