deformed sasquatch foot) found at Bossburg, Washington; and the widespread conviction among many leading Bigfoot advocates that the late Paul Freeman (the most successful of all sasquatch hunters and one of Meldrum's prime sources) was faking his traces and photos. Daegling recounts the experimental inquiry that he and Dan Schmitt undertook on the Patterson–Gimlin film, which led them to conclude that it could easily have been just a dressed-up human. But they could not rule out the possibility that it might have been something else.

Sasquatch sightings and footprints have been reported from every state in the country except Rhode Island. This distribution is more suggestive of a myth than of a mammal. And yet Meldrum apparently takes it seriously. Describing the dermatoglyphics on some convincing sasquatch footprints, he notes in passing that they were found in Georgia. But he doesn't follow out the implications of that fact, which implies a pan-continental range for this elusive giant primate. Daegling narrates the journalistic, social, and economic history of another East Coast Bigfoot manifestation, the Bardin Booger from his own state of Florida. This apelike mythic creature serves the citizens of Putnam County as a sort of kachina—at once a tourist attraction, a bogeyman, and a ritual clown that performs at local events and offers up political commentary in the newspapers.

For Daegling, the fundamental question is this: why do people report encounters with an animal that doesn't exist? He proposes that Americans see sasquatches where there are none because Bigfoot functions in our culture as an "ecomessiah," a tutelary deity of the vanishing domain of wild nature. But as Daegling knows, hairy, bestial bipeds living in the wilderness are archetypal myths found all over the world. They recur throughout the history of Western art and literature (J.B. Friedman, *The Monstrous Races in Medieval Art and Thought* [1981]). The ancient Greeks knew of them as *gorillai* (a word later applied to the real African ape), medieval Englishmen called them *wodewoses*, and we call them sasquatches. To me, the wide and deep distribution of the archetype of the Wild Man of the Woods suggests that something more fundamental is going on here. Perhaps, like Swift's Yahoos, the Wild Man is a kind of unscientific refraction of the facts of primatology: a signal to us and our children that without our culture and traditions, humans would be little more than a species of big, bipedal monkey.

For Daegling and most other physical anthropologists, the sasquatch is an amusing and intriguing legend, which deserves to be neither dismissed out of hand nor taken very seriously. If somebody brings in a specimen, we will all be eager to look at it; but until then, we have little interest in spending our lives sifting through a mountain of balderdash to see if it contains any nuggets of truth. This attitude drives Meldrum crazy. Throughout his book, he complains indignantly about his lazy colleagues who "are content to remain aloof [and] . . . passively challenge, 'Show me the body'" (p. 44). But what else are we supposed to do? Many of the supposed Bigfoot traces are clearly hoaxes. Others might be genuine, but none of them is beyond the scope of ingenious trickery. The only way to settle the issue is to show us a specimen. Nothing less will do, because footprints, photos, and video can always be faked to whatever degree of precision it takes to gull the experts. As Daegling observes, experts think they are too expert to be fooled, and so they are easy to fool. This is especially true if the pranksters are themselves experts or are clever enough to pick up some expertise from the literature. Remember Piltdown?

What Meldrum chiefly wants, I think, is not that we accept the reality of Bigfoot or compete in the search for the type specimen, but that we honor his own commitment to that search as a legitimate scientific enterprise. I think we owe him that. If the chances that Bigfoot is real are (say) 10,000 to 1 against, having one physical anthropologist devoting half his life to searching for it is roughly an appropriate allocation of our profession's resources. Meldrum deserves to be criticized dispassionately and offered sincere advice, and not to be covered in the sort of scorn and abuse that has been hurled in his direction by some of his colleagues at Idaho State (*Chronicle of Higher Education*, August 4, 2006, A44). For my part, I'm happy to see him out there in the North Woods on the trail of the sasquatch. I hope he finds one. But I feel mortally certain that he won't, and that he is wasting his professional life in the search. We may be obliged to respect his decision to do that, but we are not obliged to follow his example.

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Primates and Philosophers: How Morality Evolved. By Frans de Waal. Edited by Stephen Macedo and Josiah Ober. Princeton, NJ: Princeton University Press. 2006 209 pp. ISBN 0-691-12447-7. $22.95 (hardcover).

One would typically expect that the evolution of morality in humans and nonhuman primates is a line of inquiry beyond the bounds of science and best suited to philosophical debate. And yet Frans de Waal in *Primates and Philosophers: How Morality Evolved* succeeds in placing the topic firmly within the realm of scientific investigation. This book reprints the text of the 2004 Tanner Lecture on the evolution of morality in nonhuman primates and humans delivered by Frans de Waal at Princeton University's Center for Human Values. This is the principal topic of the book and forms the foundation of much of the printed discourse that follows. There are additional sections authored by de Waal on anthropomorphism, the theory of mind in apes, and animal rights. de Waal's contributions are followed by a series of commentaries by Robert Wright, Christine M. Korsgaard, Philip Kitcher, and Peter Singer, all philosophers

of science with different research specializations, who dissect various elements of de Waal's thesis on the evolution of morality. The book ends with a section in which de Waal responds to each commentator's criticisms.

The core of this book, and the reason to read it, is de Waal's short, 58-page essay, "Morality Evolved: Primate Social Instincts, Human Morality, and the Rise and Fall of the Veneer Theory." Here, he presents his thesis on the continuity in the capacity for moral behavior between humans and nonhuman primates, especially between humans and other hominoids. His choice to focus on the evidence for continuity between humans and other anthropoids is exceptional given that the tradition, both scientific and philosophical, is to focus on the discontinuities or those traits that make humans exceptional or unique. For de Waal, the issue appears not to be whether human definitions of morality can be applied to chimpanzees or other primates. Instead, "the relevant question rather is whether [nonhuman primates] possess capacities for reciprocity and revenge, for the enforcement of social rules, for the settlement of disputes, and for sympathy and empathy" (p 16). de Waal then marshals an impressive argument based on history, philosophy, and even current research in cognitive neuroscience to elaborate on the traits underlying moral reasoning in humans and the linkage of these to observations of nonhuman primates. He does not settle many issues with this essay, but he does firmly establish the question as scientifically relevant. Indeed, he will likely instigate many research projects based solely on the number of testable hypotheses he implies. This strictly evolutionary hypothesis of moral capacities is offered in contrast to what de Waal calls the Veneer Theory, the (possibly widely held) belief that moral behavior is a thin, external "veneer" over the selfish, amoral (or immoral) core. Although his response to Veneer Theory is multifaceted, it is when the underlying nonbiological core of this argument is exposed that de Waal is most successful in rejecting it.

It is among the commentaries that this book makes for slow and sometimes painful reading. There is a great deal of quibbling about semantics (read lots of words in quotes, like "building blocks," "naturalistic," etc.). Each commentator picks up a thread of de Waal's argument and raises objections—at times not without merit. All the commentators object in some manner to de Waal's treatment of Veneer Theory. For example, Wright finds de Waal's characterization of Veneer Theory as too simplistic to warrant serious consideration. Singer agrees and instead wonders how much of moral behavior is the result of underlying moral capability and how much is due to "veneer." I sensed that the commentators are, to varying degrees, not yet ready to reject the possibility that human moral behavior is a strategy to satisfy our selfish needs and motives within the constraints of human culture. However, what is most frustrating to me is the seeming unwillingness of these commentators to debate de Waal's interpretation of the data. The problem is, I suspect, that the use of a term like "moral" carries a deeper connotation to philosophers, who have debated the topic for millennia, than the more restricted sense intended by a behavioral biologist. Possibly for this reason, Wright, Korsgaard, Kitcher, and Singer largely ignore de Waal's core arguments and supporting data, as well as his notable anecdotes.

For my purposes, and I believe for the anthropological audience as well, I find de Waal's labors to bring topics like conciliation, reciprocity, and empathy into the realm of the quantifiable to have greater value than the semantic and tangential arguments leveled by his commentators and critics.

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MIXED METHOD DATA COLLECTION STRATEGIES. By William G. Axinn and Lisa D. Pearce. New York: Cambridge University Press. 2006. 230 pp. ISBN 0-521-85568-3. $70.00 (hardcover).

Mixed method approaches are those that combine elements of one method of data collection with elements of one or more other data collection methods. The use of such approaches appears to be increasing in the social sciences, although it is, to varying degrees, the default approach among biocultural, biological, and some cultural anthropology investigators. As such, some of the arguments in Axinn and Pearce's book *Mixed Method Data Collection Strategies* may seem old hat for anthropologists, especially the readers of this journal. What then does an anthropologist who already relies on mixed methods have to gain from this book?

First, Axinn and Pearce lay out a convincing and powerful rationale for why mixed method approaches add value to studies. They rightfully point that mixing methods ensure complementarity by counterbalancing strengths and weakness inherent in all methods. Mixing data collection strategies also generates "a comprehensive empirical record about a topic" (p. 2) and invites greater investigator involvement in data collection. The authors clearly believe that greater involvement yields higher quality data. Nor are those who do secondary data analysis of the hook. Several suggestions for their deeper involvement are made, and I particularly enjoyed the suggestion that researchers give or sit through the interview themselves—something that would shock people who have never collected primary data! In laying out the rationale for using mixed methods the book is also an extended critique of the outdated qualitative–quantitative dichotomy that continues to plague the social sciences.

Second, because of the sociological background of the authors, the book covers a number of methodological issues related to causality and levels of analysis that are covered less frequently by anthropological texts. There is