

Book Reviews

Primate Behavior and Ecology

Review of *Primate Behavioral Ecology* by Karen B. Strier. 3rd edition. Allyn and Bacon, Boston. 452 pp. ISBN: 0-205-44432-6. Paperback: US\$65.00. 2007.

Some textbooks quickly come to be established as the ‘gold standard’ for their particular area of enquiry, and this is often indicated by the speed with which new editions appear, presumably in response to a buoyant market. Such is the case for the behavioral ecology of non-human primates, as this third edition follows hard on the heels of its predecessors in 2000 and 2003. (Contrast this with its equally worthy counterpart in primate ecology, *Primates in Nature*, by Alison F. Richard, which appeared in 1985, but was never revised. What a pairing it would make with this volume!)

Karen Strier is a distinguished primate behavioural ecologist at the University of Wisconsin, best known for her exemplary long-term studies of the marmoset (*Brachyteles*) in Brazil. Equally, she is known for her overall grasp of the field and her ability to take primatology to a wider audience (for example, Strier, 2003). Both of these virtues are repeatedly expressed in this latest edition.

Happily, she has not tinkered with the general structure of the book, which retains its twelve key chapters: Introduction to Primate Studies; Traits, Trends, and Taxonomy; Primates Past and Present; Evolution and Social Behavior; Evolution and Sex; Food, Foraging and Females; Female Strategies; Male Strategies; Developmental Stages through the Life Span; Communication and Cognition; Conservation. Each of these has been bolstered by new material, to varying extents, matching the appearance to new findings in the field. For example, I checked point-by-point the four-page section on ‘tool use’; the newer version has the same four photographs but nine new references cited, and about 10% more text. This is a bit more than the overall increase in the number of pages, which has gone from 422 to 452, as has the number of reference sources cited in the bibliography (both at +7%). A new feature is a more user-friendly, 12-page Appendix of primate names, which now includes geographic regions and numbers of subspecies. Also, to the subject index has now been added a separate author index, making it easier to track the work of particular primatologists.

The strengths of the book remain, in that it is firmly embedded in the real world of primates in the wild, though with some, admittedly selective reference to their captive

counterparts, especially in the section on cognition. The judicious and apt use of topic boxes to develop specific, instructive points is retained, e.g., primates and parasites, hybrid baboons, etc. The mix of evidence and ideas remains sensibly balanced, and examples are used tellingly to illustrate key points. Strier occasionally uses anecdotes and personal experiences to flesh out topics, but never enough for this reader, who would like even more.

Why should a conservationist specialising in non-human primates buy this book? First and foremost, it is the most comprehensive and comprehensible treatment of the topic available. Furthermore, it is timely (although the date of publication is given as 2007, it appeared in 2006, and the literature referenced covers up through 2004). When the final chapter on conservation is reached, it is solidly grounded in basic science, as is the chapter itself: it covers such threats to primates as habitat disturbance and hunting pressure, conservation policies in relation to economic incentives, public awareness, and NGOs. It also examines non-invasive research, from genetics to reproductive biology. There is an earlier section, on rehabilitation, reintroduction, and sanctuaries, and on the ethical treatment of primates. Finally, although the list price of the book is a bit expensive, even new copies can be bought on line for less than \$10.

All in all, whether student or professional, any person in primate conservation should have this book close to hand on their shelves, and, even better, a spare copy to lend to colleagues. A thoroughly admirable and practical aim would be to arrange somehow for mass shipping of this book to Third World conservationists, who day-by-day are working ‘in the trenches’ and would find the book invaluable.

Literature Cited

- Richard, A. F. 1985. *Primates in Nature*. W. H. Freeman and Co., New York.
Strier, K. B. 2003. Primate behavioral ecology: From ethnography to ethology and back. *Am. Anthropol.* 105: 16–27.

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Primate Genetics – Is Taxonomy a Trivial Pursuit?

Review of *Primate Cytogenetics*, edited by Stefan Müller, Ludwig Maximilian University, Munich, Germany. S. Karger, Basel, Switzerland. 268pp. ISSN: 1424–8581. 111 Figures, 56 Tables. Hardcover: 122.00 Swiss francs, Eur87.00, US\$111.00. 2005. A reprint of *Cytogenetic and Genome Research*, Volume 108(1–3).

In appearance this book is like an oversize (285 mm × 215 mm), hardbound *Folia Primatologica*, but with a red spine and lettering. There are 30 articles, divided into three sections: Comparative Genomics and Molecular Evolution (13); Comparative Molecular Cytogenetics and Chromosome Evolution (14); and Primate Meiosis and Nuclear Architecture (3). Sixteen of the articles are dedicated to the human genome and the comparative genetics of humans and apes (and in one case Old World monkeys in general).

The first article (Ryder) is a most interesting essay on “conservation genomics,” the relevance of studies of complete genomes for conservation measures for threatened species. A further three articles examine aspects of primate phylogeny in terms of their place in the evolution of mammals (Froenicke), the phylogenetic relationships of the major primate groups (Hominoidea, Cercopithecoidea, Platyrrhini, Tarsioidea, and Strepsirrhini) (Schmitz *et al.*), and a definition of the ancestral karyotype (chromosome morphology, and banding patterns) for primates (Ruiz-Herrera *et al.* [the copy editor should surely have spotted the adjective being used as an adverb in the title of this paper]). Schmitz *et al.* examine primate origins and their affiliations with such as the Dermoptera and Scandentia, and provide a very interesting discussion of the place of the Tarsioidea—the dichotomy of the haplorhines, including tarsiers, and the strepsirrhine lemurs.

For their investigation of the ancestral primate karyotype, Ruiz-Herrera *et al.* used data on 36 primates, 24 of them platyrrhines, from 20 published sources, besides information from their own work on *Lagothrix* (their Table 1, p.163). What is striking looking at the table is that there is one article from 1982, another from 1992 and all the remainder are from 1996 or later. The studies are quite contemporary, but a number of the scientific names are outdated. This is curious in showing that either the authors are very conservative, or disagree with recent taxonomic arrangements or are just straight inattentive, or have been victims of a copy editor with an ancient taxonomy. This is not a problem in most cases. The species name should always identify the animal involved—whether it changes genus or is placed as a subspecies should not matter. Ruiz-Herrera *et al.*, for example, listed *Ateles paniscus chamek* (of Kellogg and Goldman [1944]), citing a study of G-banding by Medeiros *et al.* (1997). Seuánez *et al.* (2001) also refer to *Ateles paniscus chamek*. Medeiros (1994) and Medeiros *et al.* (1997) in fact regarded the form *chamek* to be a subspecies of *belzebuth* not *paniscus* (as was also argued by Froehlich *et al.* [1991] and more recently by Collins and Dubach [2000]). Four of the six authors of Ruiz-Herrera *et*

al. are also authors of Medeiros *et al.* (1997). Either there has been an unexplained about turn or there was an intrusive copy edit not seen by Ruiz-Herrera *et al.* Although De Boer and Bruijn (1990), Froehlich *et al.* (1991), Medeiros (1994) and Medeiros *et al.* (1997) argued that *A. paniscus* is a distinct form with no subspecies, it is listed in the table of Ruiz Herrera *et al.* as *A. paniscus paniscus*. Most odd. However, this has no particular importance besides misleading and creating confusion, because the form *chamek* has not been redefined. *Ateles paniscus chamek* is perfectly identifiable as what is considered today to be either *A. belzebuth chamek* or *Ateles chamek* (of De Boer and Bruijn [1990], Groves [1989, 1993, 2001, 2005] and Rylands *et al.* ([1983, 2000])).

The major problem arises when the definition of a given name is changed. For example, Table 1 of Ruiz-Herrera *et al.* lists studies of *Callicebus molloc* [sic] and *Saimiri sciureus* by Stanyon *et al.* (2000) as part of their data set. *Callicebus moloch* was one of just three species of titi monkeys recognized by Hershkovitz (1963). It was divided into seven subspecies. In Hershkovitz's 1988 and 1990 re-evaluation, *Callicebus moloch* was divided into eight species and 14 species and subspecies, as part of the “*Callicebus moloch* Group.” Kobayashi (1995) split the “*Callicebus moloch* Group” into two, placing four species (five species and subspecies) into the “*moloch* Group” and the form *Callicebus cupreus* into its own group (three subspecies). Groves (2001) has eight species (13 species and subspecies) in his “*Callicebus moloch* Group,” and Van Roosmalen *et al.* (2002) recognizing also a separate “*Callicebus cupreus* Group” decided on six species. Could the real *Callicebus molloc* stand up? With time passing, probably not—complicating at best and invalidating at worst any future use of the data provided by Ruiz-Herrera *et al.*

Saimiri sciureus, likewise, has, still, a highly disputed taxonomy. Silva *et al.* (1993) recognized just one species throughout the Amazon and Central America, while Costello *et al.* (1993) recognized two species, and Hershkovitz's widely accepted taxonomy (1984, 1987) listed four species and 12 species and subspecies. Thorington (1985) proposed a taxonomy slightly divergent from Hershkovitz (1984). He recognized *S. madeirae*, considered by Hershkovitz (1984) to be a synonym of *S. ustus*. It is necessary to refer to Stanyon *et al.* (2000) to know what exactly is the “*Saimiri sciureus*” listed in Table 1 of Ruiz Herrera *et al.* The identity (current name) of both the *Callicebus* and *Saimiri* according to any of the above authors can be ascertained as long as Stanyon *et al.* give the exact provenance of all the specimens they used for their ZOO-FISH analysis.

For geneticists, having the correct name and definition of the animal whose DNA they are analyzing is paramount, and they surely recognize that. So why does one perceive a certain pocourante attitude to the whole issue of taxonomy? Geneticists are after all responsible now for much reshuffling in primate taxonomy—some lumping, much splitting, discoveries of new populations which are awarded the status of “new species”, and in many cases the discovery that what we thought was X (they look very alike) is in fact something different