American Journal of Primatology 74:692–695 (2012)

COMMENTARY

New World Monkey Nightmares: Science, Art, Use, and Abuse (?) in Platyrrhine Taxonomic Nomenclature

ALFRED L. ROSENBERGER*
Department of Anthropology and Archaeology, Brooklyn College, Brooklyn, New York

The taxonomy and nomenclature of New World monkeys is becoming precariously unstable and impractical, plagued by revisions aimed at conforming to approaches that reject the Biological Species Concept for narrowly construed reasons and resulting in a hyperinflated taxonomy at species (often) and genus (sometimes) levels. This undermines a major goal of classification at the most basic taxonomic levels to ease communication and facilitate research. Since it is difficult to justify extensive changes in terminology without a deeply justified theoretical purpose or without showing what scientific benefits these alterations can bring, working primatologists need not accept this doctrinaire trend. Knowing as little as we do about what a species actually is, does not justify contorting the value of a species nomenclature so that it reflects nothing more than coat color, a node, or endpoint of a dendrogram. Am. J. Primatol. 74:692–695, 2012.

Key words: platyrrhines; New World monkeys; taxonomy; species

INTRODUCTION

You and I are permanent captives of taxonomic nomenclature. Without it, there would be no way to use the literature. No way to identify animals in the field or in the museum. No way to organize data collection and analysis. No way to compose hypotheses and communicate them. No primatology. The taxonomic and nomenclatural system employed for New World monkeys (NWM) is breaking down, but it need not be. Users have the ultimate say in how language is employed in our culture, so the polity must decide how far this process will go.

Like you, I am a user of the system. I have only limited experience researching the species-level taxonomy of living platyrrhines, but little is needed to know that we are being inundated with taxonomic changes that are neither well-justified nor obvious requirements of evolutionary theory or scientific progress. The impetus for name changes became viral following the publication of Colin Groves' book, "*Primate Taxonomy*," in 2001. But the trend was evident some years before, driven by the conservation community. (And here I feel compelled to proclaim my allegiance to the conservation mission but not to their method of exploiting *science* in the form of taxonomic hyperinflation to advance the cause, either to emphasize the holocaustic scale of the matter or to satisfy unfortunate legislative language that only approves saving species, literally, rather than biodiversity.) I was one who paid attention to Groves' NWM classification, and it was perfectly obvious that in spite of his attempt to justify taxonomic decisions with a global explanation that appealed to science, there was something eerily ironic about the process of shoving all those subspecies, one after another, one rung up in the Linnaean hierarchy to make them species. I stopped counting how many out of sheer frustration. I also took to heart Groves' decision to elevate the yellow-tailed woolly monkey, *Lagothrix flavicauda*, to genus status (*Oreonax*), a possibility I came close to entertaining at some point myself [Hartwig et al., 1996]. Because this move would have a more direct impact on my own work, I repeated and extended Groves' analysis and found it to be wanting [Matthews and Rosenberger, 2008; Rosenberger and Matthews, 2008]. But the point is that primatology has a responsibility to critically examine its own institutions—language is an institution accessible to all; no prior expertise required—and to reject ill-conceived notions when necessary.

Recent papers on *Cebus* provides an example of this new taxonomic landscape [Lynch Alfaro et al., 2012a,b]. I do not take this issue with the science,

*Correspondence to: Alfred L. Rosenberger, Department of Anthropology and Archaeology, Brooklyn College, Brooklyn, NY 11210. E-mail: alfredr@brooklyn.cuny.edu

Received 1 February 2012; revised 2 April 2012; revision accepted 3 April 2012

DOI 10.1002/ajp.22037
Published online 17 May 2012 in Wiley Online Library (wileyonlinelibrary.com).

although one hopes it will be rigorously tested going forward. What worries me more is the way the research was played out in the taxonomic and nomenclatural domains. Most of us have been willing to commit to working memory the five or so species names used for *Cebus* monkeys during the last half-century in order to navigate the literature. It worked. If we needed to go more deeply into the taxonomy by following names, there were always the subspecies tags and a small pile of papers and books for reference. That worked, too. But how many of us will be willing and able to retool and memorize the names of 12 *Cebus* species? Now add the ten species of *Alouatta* recognized by Cortés-Ortiz et al. [2003]. And, the 29 species of *Callicebus* upheld by Rylands and Mittermeier [2009]. I doubt many of us will try, even the most pious taxonomic zealots, though such interminable lists might be good fodder for a perversely amusing form of graduate student penance.

My question is, what is to be gained by renaming so many entities that already carry perfectly useful, individualized labels? How will it improve the science? It has long been recognized that nomenclature is an imperfect vehicle for maintaining order and communicating, and alternatives exist for those who may need to partition data into units of any size for specialized studies—numbers work well. At the alpha taxonomy level, to argue that it has become necessary to elevate named subspecies to the rank of species because we now have a better appreciation of what a species actually is in the real word, and the old system was plain wrong, does not persuade. The reality is that all species concepts and diagnoses are working hypotheses. And, no matter what one believes is closer to nature's truth, as biological entities those populations are perpetually undergoing evolutionary flux. This means the essence of our human naming systems, compunction to have a consistent static definition of memorable terms, can only be a marginal fit with the nature of the phenomena to which the names are applied anyway.

No more objectivity or scholarly efficiency is gained by renaming such ephemera. Indeed, holding fast to philosophically driven paper definitions of species, such as the Phylogenetic Species Concept, as a justification for these shifts is highly questionable because they are essentially untestable propositions. Yes, the links of the tree that drive taxonomic redefinition of the network endpoints can be tested for consistency. But that still does not reflect or explain much about what those terminal entities actually are in the real world–what other biological attributes they might have; what their evolutionary role might be; why they exist in the first place, etc.—other than being groups discerned by cladistic relationships, which is itself a much reduced version of actual phylogenetic history. Thus, method and style would trump substance. Some will refer to the endpoints by names; others may like numbers; some would prefer calling them species as homage to Linnaeus; others might dub them Thing 1/Unit A, Thing 2/Unit A, and so on. True, the more conventional Biological Species Concept is also very difficult to test empirically, but falsification criteria have been developed to enhance its scientific foundation and utility in broad biological terms, and few could argue convincingly that the system has not worked. The species-level nomenclatural free-for-all we see today (which affects other primates as well as NWM and, apparently, primates more than other mammals) is like a mirror-image throwback to the nomenclatural Tower of Babel Hershkovitz [e.g. 1977 et seq.] and Napier [1976] helped clear up, much of that a holdover of lousy old-fashioned science, even pre-evolutionary. One might think of it, uncharitably, as a swapping of coat color for clade as the species standard.

The *Cebus* monkey case also serves as a prime example of pragmatic counter-reaction to the sweeping, punctuated disequilibrium inflicted on an established working system, which threatens the hard-earned commonality of language that users of nomenclature have adopted from the purveyors of taxonomy. For example, Fragazy et al. [2004] wrote an indispensable book synthesizing decades of biological research on this, perhaps the most important, model genus of NWM. In setting the stage for all that would follow, the authors, with the acknowledged collaboration of Anthony Rylands, took great care to present an extensive modern taxonomy of eight species. (Later Rylands and Mittermeier [2009] would advocate 11 species.) But in the same very first chapter they recant, saying, "... almost all of the published literature on capuchins refers to the four traditional species (*C. apella*, *C. capucinus*, *C. albifrons* and *C. olivaceous*) and thus, in order to be consistent with the authors of the original research, the subsequent chapters of our book will, with a few exceptions, refer only to these four species." In doing so, the book lumps into one as many as seven distinct species of Tufted capuchin (*C. apella*) according to the classification of Rylands and Mittermeier [2009]. So, one wonders: Given what's vogue, has this semantic revolution hastened "The Complete Capuchin" toward a premature and unjust obsolescence?

A comparable note of discord echoes with *Ateles*. The authoritative Rylands and Mittermeier's paper [2009], one of several appearing in the various publications promoting comparable taxonomies, recognized seven species (*A. geoffroyi*, *A. paniscus*, *A. belzebuth*, *A. hybridus*, *A. fusciceps*, *A. chamek*, and *A. marginatus*). In contrast, Collins [2008], who produced much of the original genetic research on the topic, upheld only three, and at most four, in his critical assessment. These were the same four species endorsed by Di Fiore et al. [2010] in a major synthesis of atelines focusing on behavioral ecology. Like Fragazy

et al. [2004], Collins, Di Fiore, and colleagues chose simplicity over chaos, not only because primary evidence and working theory guided them, but also because it was practical.

In their *Cebus* project, Lynch Alfaro et al. [2012a,b] also promote changes in genus-level taxonomy. Here, it may seem that there is yet more room to maneuver, for systematists have been even less successful in developing criteria as to what a genus is. We accept only that a genus must be conceived or measured artfully against some quality, for that standard remains elusive. Customary, default guidelines point to a phylogenetic rationale and an assessment of gaps in morphology and adaptation between species or among clusters of species. That formula does not spell out a consistent procedure, but it has proven productive. Invoking an arbitrary time dimension, albeit one measured by a molecular clock, as a criterion for the genus, a reversion to the classification methods of Hennig [1966] if not the Victorian-era Louis Agassiz, and the criterion that Lynch Alfaro et al. (2012a,b) endorse, distorts the accepted practice of using the array of supra-specific categories as functional equivalents, to express (when possible) the ecophylogenetic hierarchy of a radiation. Nor does it seem necessary to double the number of genus names for *Cebus* monkeys because our long-standing taxonomy is seen to underestimate woefully the numbers of species that can now be finally arranged into presumptive monophyletic groups. Systematists have long dealt with this organizational problem, and they have found solutions to keep the genus taxon stabile. Using informal names for supra-specific groups is one way to do it; using the subgenus category is another. In working with NWM, Hershkovitz [1977] and Napier [1976] adopted both strategies to good effect. Szalay and Delson [1979] deployed the subgenus extensively in classifying the entire order along cladistic lines.

Building on others, Hershkovitz [1977] and Napier [1976] produced foundational taxonomic works in this field and concluded that 16 genera of living NWM were a proper reckoning of platyrrhine diversity. This is not an inviolate proposition. But that multidimensional genus concept, a hypervolume of sorts since it was meant to reflect everything known about the taxa and not just phylogeny, has proven to be brilliantly workable. Though not strictly a scientific hypothesis, one might even say it has been corroborated. Think of the enormous quantity of new research that the framework yielded over the many decades since its inception, how much knowledge fell into place because of that structure, and all the new fossil platyrrhines whose taxonomy has been calibrated against it. Of course, there have been occasional challenges and revisions to the Sweet Sixteen scheme, as proposed additions (*Callibella*), deletions (*Cebuella*), or insertions (*Mico*) based on new discoveries or interpretations. How could there not be, giving the times and our scientific mandate to test and retest? But now Lynch Alfaro et al. [2012a,b] ask us to split *Cebus* monkeys into the genera *Cebus* and *Sapajus* to recognize the split we have known about and worked with for more than a generation. Shades of *Australopithecus* and *Paranthropus*. Platyrrhinology: Meet Paleoanthropology. Let's for a moment drop the pretence, scientific affectations, go native, raise the binoculars and simply ask: Aren't these animals the same thing, more or less? And, isn't the *species* what we mean by "more or less"?

As one who has advocated strenuously for changes in the higher classification of platyrrhines, I am not averse to freshness when it comes to systematics. But I caution against undermining our capacity to work and communicate through changes in the nomenclatural system that are not justified by profound need. At least one of my colleagues who has examined thousands of fossils firsthand, many just out of the ground, and is one of our most accomplished and experienced primate systematists, has made it a proud point of his career *not* to name anything a new species. Few of us hold so true to the principle of stability, or are as sensitive to the inevitable vagaries of taxonomic practice and the scientific underpinning of species to be so perspicacious. But there lies a pragmatic lesson. For the data-rich living forms in particular, perhaps we should be as wary of changing or creating taxonomic names at the species and genus level as we are mindful when conferring on our own children the names they will carry for a lifetime, for fear of having to live with the unintended consequences.

ACKNOWLEDGMENTS

This manuscript adheres to the American Society of Primatologists principles for the ethical treatment of primates. I wish to thank an anonymous reviewer for very thoughtful and helpful comments on the manuscript.

REFERENCES

Collins A. 2008. The taxonomic status of spider monkeys in the twenty-first century. In: Campbell CJ, editor. Spider Monkeys. The biology, behavior and ecology of the genus *Ateles*. Cambridge: Cambridge University Press. p 50–72.

Cortés-Ortiz L, Bermingham E, Rico C, Rodríguez-Luna E, Sampaio I, Ruiz-García M. 2003. Molecular systematics and biogeography of the neotropical monkey genus, *Alouatta*. Mol Phylogenet Evol 26:64–81.

Di Fiore A, Link A, Campbell CJ. 2010. The atelines: behavioral and socioecological diversity in a New World radiation. In: Campbell CJ, Fuentes A, MacKinnon KC, Panger M, Beader SK, editors. Primates in perspective. 2nd edition. Oxford: Oxford University Press. p 155–188.

Fragazy DM, Visalberghi E, Fedigan LM. 2004. The complete capuchin: the biology of the genus *Cebus*. Cambridge: Cambridge University Press. 356 p.

Groves, C. 2001. Primate taxonomy. Washington DC: Smithsonian Institution Press. 350 p.

Hartwig, WC, Rosenberger AL, Garber PA, Norconk MA. 1996. On atelines. In: Norconk MA, Rosenberger AL, Garber PA, editors. Adaptive radiations of neotropical primates. New York: Plenum Press, p 427–431.

Hennig, W. 1966. Phylogenetic systematics. Urbana: University of Illinois Press. 280 p.

Hershkovitz P. 1977. Living New World monkeys, volume 1: (Platyrrhini) with an introduction to primates. Chicago: University of Chicago Press. 1132 p.

Lynch Alfaro JW, Boubli JP, Olson LE, Di Fiore A, Wilson B, Gutiérrez-Espeleta GA, Schulte M, Neitze S, Ross V, Schwocho D, Farias I, Janson C, Alfaro ME. 2012a. Explosive pleistocene range expansion leads to widespread Amazonian sympatry between robust and gracile capuchin monkeys. J Biogeogr 39:272–288.

Lynch Alfaro JW, de Sousa E, Silva J, Jr., Rylands AB. 2012b. How different are robust and gracile capuchin monkeys? An argument for the use of *Sapajus* and *Cebus*. Am J Primatol 74:273–286.

Matthews LJ, Rosenberger AL. 2008. Taxonomic status of the yellow-tailed woolly monkey, *Lagothrix flavicauda* – an object lesson in the implementation of PAUP*. Am J Phys Anthropol 137: 245–255.

Napier PH. 1976. Catalogue of Primates in the British Museum (Natural History). Part I: Families Callitrichidae and Cebidae. London: British Museum. p i–xii, 1–121.

Rosenberger AL, Matthews LJ. 2008. *Oreonax* – not a genus. Neotrop primates 15:8–12.

Rylands AB, Mittermeier RA. The diversity of the New World primates. (Platyrrhini): an annotated taxonomy. 2009. In: Garber PA, Estrada A, Bicca-Marques JC, Heymann EW, Strier KB, editors. South American primates. Comparative perspectives in the study of behavior, ecolgy and conservation. New York: Springer. p 23–54.

Szalay FS, Delson E. 1979. Evolutionary history of the primates. New York: Academic Press. 580 p.