

The Pros and Cons of a Consensus List of Asian Primate Subspecies

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Abstract: For effective international implementation, conservation action and legislation should rest on a broadly accepted scientific classification. Such classifications must keep pace with advances in taxonomic research. Provision is necessary for potentially as well as currently recognized taxa. Regional classifications of primate subspecies are scarce. None was published from 1968 to 1997 for Asian primates as a whole. Napier and Napier's (1967) now outmoded (global) classification was only a list. Groves' (2001) classification caused consternation in the number of subspecies promoted to species. In response, a workshop was convened in Florida, USA, in 2000 to address this issue and to compile a consensus classification. The resulting Asian annotated list was published in 2004. Such a compilation usefully collates various taxonomic sources in a single reference citable as that adopted in reporting research results. This need not imply wholesale acceptance. Departures can be specified. The classification can, and should be, the springboard for further research. Its consensual nature tends to reduce individual bias and error and broadens the research input. Conversely, a single-authored classification might surpass it in consistency of taxonomic approach and in evading awkward compromise. By its rarity any classification risks entrenchment, discouraging further taxonomic research and encouraging antipathy toward its successors. Conflicts over the significance of genetic evidence and other questions raised during the compilation of the Asian list confirm that, like its predecessors, this list is not definitive. It should and will be superseded.

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There being no consensus on the best English common name for many species, let alone common names in other languages, effective international collaboration in wildlife conservation requires an agreed scientific nomenclature. The scientific names of well-studied animals with no close living relatives, such as the giant panda and the Indian elephant, are stable and likely to remain so, posing no taxonomic issues for the conservationist other than a possible need to define and conserve subspecies. The scientific names of other species, including high-profile ones like the orang-utan, present the conservationist with inescapable problems.

The taxonomic problem associated with the orang-utan is that genetic evidence questions its long-held treatment as a single species (Brandon-Jones *et al.* 2004). If it is divided into two species, how do we geographically split them and what is the correct scientific name for the second species? The presumption that they are separated by a sea barrier, and that the new species is called *Pongo abelii*, may be wrong. First off, a good case can be made that the name *Simia pygmaeus*

Linnaeus, 1760 belongs to the Sumatran population. Linnaeus based the name on a specimen sent him by an Englishman at a time when there was intense colonial rivalry between the British and Dutch in Southeast Asia. Edwards probably obtained his specimen from the then-British protectorate of north Sumatra rather than from the then-Dutch-dominated Borneo (Röhler-Ertl 1988). Second, the south Bornean population shares characters with the Sumatran population that distinguish it from orang-utans in the rest of Borneo (Brandon-Jones *et al.* 2004). If this south Bornean population is combined with the Sumatran one, its name *Pongo wurmbii* Tiedemann, 1808 predates and therefore has priority over *P. abelii* Lesson, 1827.

Other described species, including some primates, may be so poorly known that no common name exists. This in itself is significant only if it reflects the equivocal taxonomic status of a species. Conservation resources better expended elsewhere may be squandered on an ultimately rejected taxon. Confirming the recognizability of a taxon is more crucial than

debating its status as a species or subspecies. Preoccupation with such discussion risks fatally delaying the implementation of essential conservation measures. In an ideal world all species would be conserved in their entirety. In the real world this is impractical. We should strive to conserve a sample of all the representative subpopulations of every species, be they formally described subspecies, or populations suspected but not confirmed as taxonomically distinct. Groves' (2001) conversion to the phylogenetic species concept is a dramatic but by no means unique example of changing trends in taxonomy. The biological species concept is generally slipping from favor. Today's subspecies may be tomorrow's species, and *vice versa*. We cannot afford to be complacent about the loss of any such population.

Few authors have attempted a classification of all extant non-human primate species. Fewer still have tackled the subspecies. No complete Asian non-human primate subspecies classification was published between the handbook list of Napier and Napier (1967) and the preliminary list that Eudey (1998) compiled in 1997 for her review of the *Action Plan for Asian Primate Conservation: 1987–91*. This was not through lack of innovations. Seven new Asian primate taxa were described during this 30-year interval (Rylands *et al.* 2001); taxonomic revisions continued to appear, notably Jack Fooden's monographs on the macaques; and field evidence steadily accumulated. The absence of an Asian primate subspecies classification merely reflects the absence of an authority with the expertise, resources, stamina, and perhaps sheer bloody-mindedness necessary to accept the challenge. Other than self-satisfaction, the rewards are small and the criticism can be acerbic.

Such criticism is often primarily motivated by an understandable desire to maintain the *status quo*. A long-term field biologist may be reluctant to discard or modify the scientific name of his or her study animal. The public are bemused when the scientific name of their favorite zoo animal suddenly changes. Novel classifications designed merely to promulgate a pet hypothesis or for career development deserve the indignation they tend to provoke. At times, however, even the impartial taxonomist cannot readily propose the ideal nomenclature when two or more phylogenetic interpretations conflict or when crucial historic information is elusive or lost. The primate species that still present such dilemmas belong mainly to groups that are the most taxonomically neglected, such as the Asian Colobinae. (One such case is discussed at the end of this paper.) As research progresses, the resolution of these discrepancies will gradually gain consensus and classifications will stabilize. Along the way, some name changes are inevitable. Taxonomy is a rarefied discipline and, like everyone else, its few practitioners are fallible. Modern taxonomists cannot be blamed for past neglect or incompetence. If you pressurize them to ignore such lapses, future generations will curse you for failing to resolve the issue. The longer the prevarication, the greater the eventual disruption. Animal groups should not be reclassified every week, but once every 30 years is probably too seldom.

The consensus Asian primate subspecies classification discussed in this paper derives from a workshop on primate taxonomy convened by the IUCN/SSC Primate Specialist Group at the Disney Institute, Orlando, Florida, USA, 25–29 February 2000. The workshop was partly motivated by a concern that, for want of any alternative, Groves' (2001) primate taxonomy might become the entrenched standard, despite his aversion to such an outcome, and despite the irony that this apprehension might prevent his taxonomy from achieving supremacy. Reservations about Groves' (2001) taxonomy focus on his conversion to the phylogenetic species concept. This entails promoting to species many taxa previously recognized only as subspecies. Although Groves (2001) has not discarded subspecies as a taxonomic concept and includes them in his classification, he paid little heed to populations potentially recognizable as taxa. The documentation of such populations was a priority for the workshop.

Eudey's (1998) compilation formed the baseline for the Asian primate classification with considerable input from a pre-publication copy of Groves (2001). Progress on our compendium at the workshop itself proved merely to be the tip of the iceberg, and most of the interaction was by subsequent electronic communication. Colin Groves attended the workshop as one of the "African group" (Grubb *et al.* 2003), but his later collaboration on tarsier taxonomy thoroughly earned him co-authorship of the report from the "Asian group" (Brandon-Jones *et al.* 2004). The workshop was originally intended to conclude with regional groups evaluating one another's conclusions. Sadly, time did not allow. A reconvention of the workshop in the future could remedy this omission and bring the regional classifications up to date, preferably with the results this time united in one publication.

The main advantage of the Asian list is that it is a complete contemporary classification available for citation as a single reference. It is also a convenient and valuable bibliographic repository. The IUCN/SSC Primate Specialist Group has adopted it as the basis for the 2001 Asian Primate Red List. It will remain so for forthcoming lists until research yields enough modifications to require a new one. Authors not wholly enamored by the list may still find its use more practical than cherry-picking their classification from various sources. They need only specify where, why, and how they feel inclined to digress from it. The list summarizes the current state of Asian primate taxonomy, emphasizing future research priorities. Its consensual nature tends to reduce individual bias and error and broadens the research input. Admittedly, a single-authored classification might surpass it in consistency of taxonomic approach and in evading awkward compromise.

Classifications are inevitably compendia of the most authoritative available species or species group classifications. The compilers of such compendia may therefore themselves be inputting little into the resulting classifications, except to arbitrate when more than one classification of a particular species conflict. When a compendium is co-authored, the consensual aspect is in reaching agreement or compromise in such arbitration. In practice, the inadequacies of current taxonomy

demand more of such compilers than mere arbitration. Most compilers cite additional information that either elaborates, corroborates, or contradicts the selected classifications. In the case of the dusky leaf monkey *Trachypithecus obscurus*, for example, two major subspecific classifications were published almost simultaneously so neither author had benefited from the other's contribution. The deficiencies in both classifications from one (Pocock 1935) relying on a British and the other (Chasen 1935) on an Asian specimen collection precluded a straight choice between them. The only option was to present a provisional subspecies classification contrasting their approach, with compromise where possible, assisted by additional information from subsequent authors and from my own examination of the important American museum collections. Considerable further research is required on these subspecies and on those of *Presbytis rubicunda*. In his authoritative review of the rhesus macaque *Macaca mulatta*, Jack Fooden (2000) recognized only one subspecies, but genetic evidence suggests there are at least two. We tentatively recognized seven subspecies, six of them to the east of the Bay of Bengal and showing greater genetic affinity with the Japanese and Taiwan macaque species than does the South Asian subspecies. There is some internal separation of the north Indian population even in the *M. mulatta* Y-chromosome clade, which includes *M. fascicularis* populations from north of the Isthmus of Kra.

Of the Asian primates, the taxonomy of the tarsiers is most in need of investigation and revision. Myron Shekelle, Colin Groves, and I accordingly embarked on a considerable amount of original, mainly nomenclatural research. Most problematic are the Sulawesi tarsiers, which, until recently, were thought to comprise a localized, central highland species, *Tarsius pumilus*, surrounded by a generally larger, more widespread, lowland species, *T. tarsier*. In 1991, a new, centrally located species, *T. diana*, was described. Its authors, however, erred in two respects. They neglected an earlier name, *T. dentatus*, which is probably a senior synonym, and they assumed that the type locality of the widespread species was in the north of the island. Although its exact location is debatable, available evidence places the type locality of *T. tarsier* in the south of the island (Fig. 1). This might seem of purely academic interest were it not for the subsequent discovery from playback of the duet call that the range of *T. diana* seems to extend to the west coast, segregating the northern from the southern population, which are both distinct from it in vocalization. If the northern and the central populations are united as one species distinct from *T. tarsier*, then *T. dentatus* predates *T. diana* as their scientific name. Its type locality (Fig. 1), however, unfortunately lies very near the suspected boundary between the northern and the central populations. This poses a problem if they are regarded as separate species. If the type locality lies to the north of this boundary, *T. dentatus* is the available name for the northern population. If it lies to the south, as seems more likely, it is a senior synonym of *T. diana*, and the northern population would require a new name.

In the Asian list we also made an original contribution to the classification of the Indochinese leaf monkeys. This was

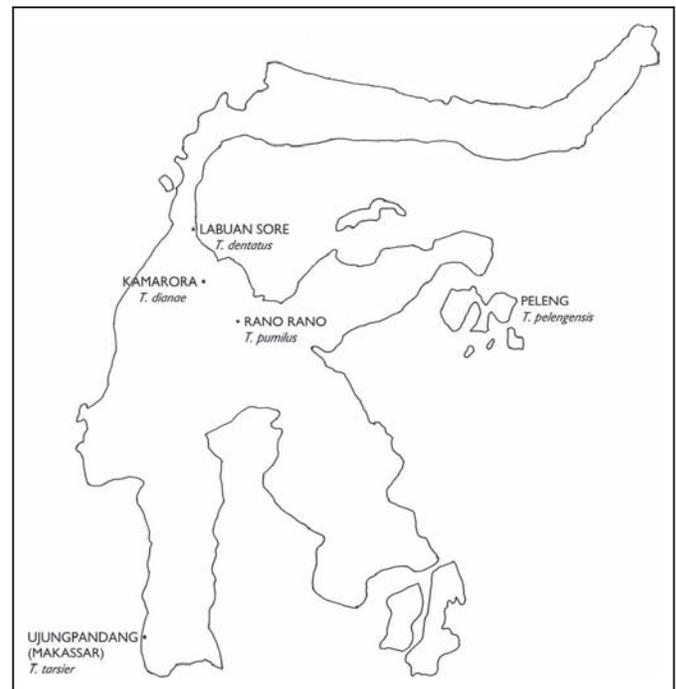


Figure 1. The type localities of tarsier species-group nominal taxa described from Sulawesi, Indonesia.

necessitated partly by the aim to document all subspecies. One subspecies had fallen victim to misidentification and inaccurate locality information. The eccentric American taxonomist Daniel Giraud Elliot (1835–1915) is one of an elite few, bold or daft enough to have embarked on a classification of the primates as a whole. In the process, he described numerous purportedly new primates, most of which have proved to be synonyms. Some of them, however, have survived, at least as subspecies. His helter-skelter approach to describing new primates, notably in Elliot (1909), probably engendered skepticism. Whatever the explanation, another American taxonomist, Wilfred Hudson Osgood (1932) misidentified two Field Museum specimens from Laos as the silver-grey leaf monkey subspecies, *Trachypithecus barbei argenteus*, which is endemic to west continental Thailand (Fig. 2). They are actually good examples of Elliot's (1909) subspecies *T. villosus margarita*. Although Pocock (1928) examined the holotype at the Natural History Museum in London and accepted the subspecies, Osgood's (1932) misinformation evidently sapped his confidence and by 1934, Pocock (1935) had discarded it.

This is particularly unfortunate because, as Pocock (1928) had appreciated, *Trachypithecus villosus margarita* is in reality a key subspecies bridging the morphological gap between *T. v. germaini*, the south Indochinese silver leaf monkey and *T. barbei holotephreus*, the ash-grey leaf monkey distributed from north Vietnam to southeastern Burma (Fig. 2). It is, so to speak, the leaf monkey “missing link.” Without it, Pocock (1935) abandoned his former insightful interpretation of these leaf monkeys as effectively a “ring species.” With it reinstated, we can see that the silver leaf monkey, a close relative and derivative of the ebony leaf monkey, *T. auratus*, has a disjunct distribution in the Malay archipelago and south Indochina (Fig. 2). It is linked by *T. v. margarita* with

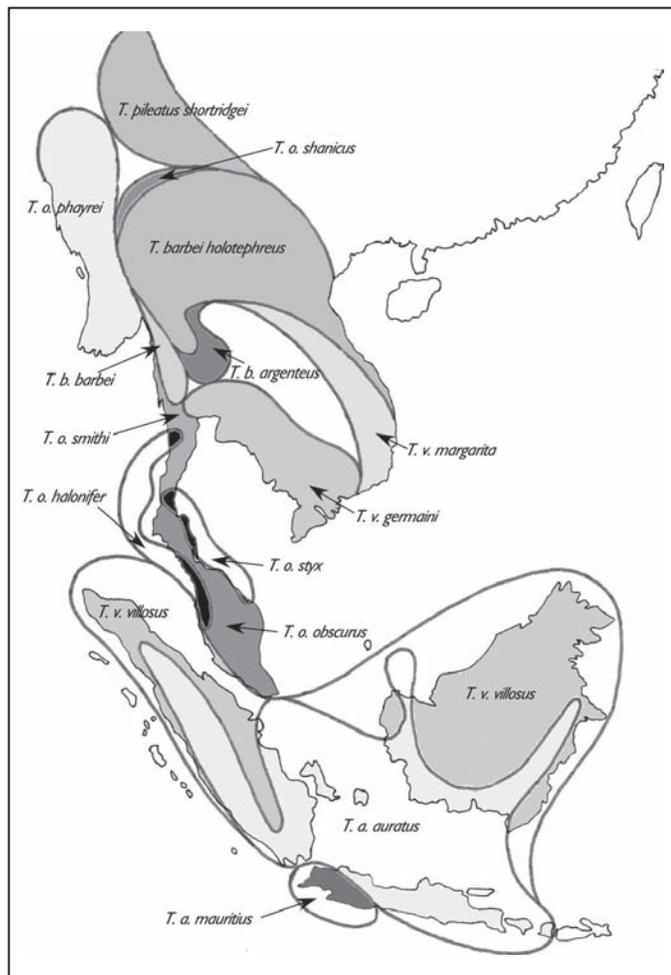


Figure 2. The geographic distribution of Southeast Asian leaf monkeys (excluding the pied leaf monkeys of China, Laos and Vietnam).

T. barbei holotephreus, which in turn, intergrades into *T. obscurus* to the west and probably into *T. pileatus shortridgei* to the northwest. In its dispersal, *T. obscurus* has headed both north into northeastern India and south into the Malay Peninsula where its arrival probably disrupted the distribution of the silver leaf monkey, formerly continuous from Indochina to the Negeri Sembilan coast in West Malaysia.

As to the fate of *T. phayrei* in this rearrangement: typical *T. phayrei* is endemic to southeast Bangladesh, west Burma and northeast India. It is a good subspecies but Pocock's (1935) rationale for treating it as a species is spurious. He rightly acknowledged that it is very similar to typical *T. obscurus* from the southern part of the Malay Peninsula. Situated between them, however, is a group of dark subspecies (Fig. 2). Pocock (1935) reasonably assigned them to *T. obscurus* but unintelligibly contended that as they differ from *T. barbei holotephreus* (which he assigned to *T. phayrei*) in much the same way they differ from typical *T. obscurus*, *T. phayrei* is a separate species. To treat these three subspecies groups as one species, as three species, or as one species sandwiched between a geographically disjunct species, would have been rational. To almost arbitrarily unite the dark subspecies with their southern rather than their northern paler neighbors is illogical. A more judicious solution is to divide

the “ring species” into components reflecting their predominant pelage color: silver (*T. villosus*), grey (*T. barbei*), and brown (*T. obscurus*). We therefore treat typical *T. phayrei* as a subspecies of *T. obscurus*. *Trachypithecus obscurus* is apparently undergoing pelage color saturation in the central part of its range, leaving to the north and south paler subspecies retaining the species' earlier pelage color. Were it not for habitat destruction, the probable outcome of this saturation, which might be expected to spread north and south, would be that *T. obscurus* would eventually regain the *Semnopithecus johnii*-like pelage color of its antecedents. Such metachromic processes probably recur in cycles correlated with the glacials (Brandon-Jones 1999).

I categorize *S. johnii* as an antecedent of *T. obscurus* but incongruously assign them here to different genera. This is emphatically against my better judgment but dictated by the employment in this paper of the consensus Asian primate classification. It illustrates one of the less palatable aspects of co-authoring a consensus document and probably the major, but also the most intriguing, cause of dissension among the co-authors of the Asian list. The geneticists among us—Don Melnick, Juan Carlos Morales, and Caro-Beth Stewart—insisted the genetic evidence assigned *S. johnii* to *Semnopithecus*. I was in a minority of one in protesting that the morphological evidence links it with *Trachypithecus*.

Early in 2002, almost exactly 2 years after the Orlando workshop, I was invited to attend a South Asian primate Conservation Assessment and Management Plan (CAMP) workshop at Coimbatore in southern India (Molur *et al.* 2003). My participation was sought for general taxonomic advice and specifically to provide a sound taxonomic basis for the available data on the conservation status of the Indian langur subspecies. I had recently been doing little research on the subject, so a steep learning curve was required. During the workshop I was largely preoccupied with the Himalayan langurs, whose poorly documented subspecific distributions, particularly in Himachal Pradesh and Uttaranchal, India, were creating problems for the participants. I also heard some intriguing reports of natural hybridization between *S. entellus* and *S. johnii*. Mewa Singh kindly invited me to join a group of participants who had arranged to visit the Annamalai Hills (Tamil Nadu) at the end of the workshop. On one of our days there we managed to see all four free-living diurnal primates at close quarters, including some langurs that may well have been *S. johnii* hybrids. I was also gratified to confirm my suspicion from the under-fur of a London museum specimen that, like other *Trachypithecus* but contrary to reports in the literature, *S. johnii* has an orange (albeit a dull orange) neonatal coat. It turns black after 3 months (Sharma, in Brandon-Jones 2004).

The many questions I had attempted to answer for the workshop report (Molur *et al.* 2003) gradually metamorphosed into a taxonomic revision of the langurs of the Indian subcontinent (Brandon-Jones 2004). Its publication leaves the Asian list already superseded in some respects. The major new development arose from photographs published by Hohmann

and Herzog (1985) of *S. johnii*/*S. priam* hybrids in the Nilgiris. Initially I suspected misidentification of *S. entellus hypoleucos* but other evidence in the article confirmed their hybrid status. Endemic to the Malabar tract in southwest India, *S. e. hypoleucos* was previously split into four subspecies. The two darker ones, both situated just outside the western perimeter of the range of *S. johnii*, so closely resemble the Nilgiri hybrids that they too must be hybrid populations; in their case, of *S. johnii* crossed with *S. entellus*. The paler pelage color of *S. e. hypoleucos* populations further from contact with *S. johnii* misled taxonomists into recognizing the other two subspecies (Brandon-Jones 2004).

The greatest cranial resemblance of *S. entellus* and *S. priam* to *Trachypithecus* occurs in the Sri Lankan subspecies, *S. p. thersites*. From southwest India to the Himalaya, langur skulls generally become more distinct from those of *Trachypithecus* mainly by progressive enlargement (see measurements in Pocock 1928). Pelage color and geographic variation in tail-carriage reinforce this indication that true langurs diverged from the purple-faced leaf monkey, *S. vetulus* in Sri Lanka. *Semnopithecus vetulus* in turn probably previously diverged from its close relative, *S. johnii*. This can explain their hybrid viability when langurs dispersing northwards from Sri Lanka met *S. johnii* in southern India (Brandon-Jones 2004). *Semnopithecus johnii* genes have apparently boosted the *S. vetulus* genetic heritage of langurs flanking, and north of, the range of *S. johnii*. The process is reciprocal, thus possibly exaggerating the apparent genetic divergence of *S. johnii* and *S. vetulus* from other *Trachypithecus* species. It is therefore premature to define *Trachypithecus* purely genetically and to assign *S. johnii* and *S. vetulus* to *Semnopithecus*. The evidence perhaps favours demoting *Trachypithecus* to a subgenus of *Semnopithecus* but, above all, it urges caution in inferring phylogeny from genetic evidence.

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