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Horizon Scanning Article

"Taxonomic inflation" in the historical context of mammalogy and conservation

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Abstract

Recent criticism by users of mammal taxonomy on current trends in the discipline and in adopted species concept require that an historical perspective should be added to the issue. The low profile accorded to taxonomy by evolutionary biology for most of the XX century, not detailed revisionary work, was the main reason for the artificial stability of taxonomies until the 80's of the last century. Conservation biology has been one of the main causes of upsurge of interest in mammal taxonomy and we argue that lesser inclusive species concepts are largely positive for biodiversity conservation.

In a recent paper, Zachos et al. (2013) criticize the current rise in the number of recognized mammal species, particularly as realized by one of us (C.G.) (Groves, 2013). Even among primatologists, where the Phylogenetic Species Concept now overwhelmingly holds way, a complaint against "oversplitting" (especially as applied to South American primates) has recently been expressed (Rosenberger, 2012). Likewise, concern has been expressed regarding the effects on conservation biology of the practice of raising many former subspecies to specific rank (Isaac et al., 2004; Meiri and Mace, 2007; Frankham et al., 2012).

Mammalian taxonomists are today in very short supply, as are biological taxonomists as a whole (Wägele et al., 2011). In this light, it seems intriguing that those few mammalian taxonomists still working worldwide should be on the receiving end of harsh criticism instead of being acknowledged for their fundamental if largely unfashionable work.

It is our contention that this paradoxical situation can only be understood in a historical perspective.

It would appear that for most current "users" of taxonomy this discipline goes back to the post-WWII years at the most. Earlier literature is often ignored as irrelevant and only of historical interest. Elsewhere one of us discussed, with particular attention to European mammalogy (Gippoliti, in press), the influence of the New Synthesis (Huxley, 1940; Mayr, 1942) in the decline of museum-based taxonomy amid competition from, and the increasing dominance of, other disciplines such as genetics, karyology and ethology. Too many modern biologists seem to give no credence at all to morphology as a taxonomic criterion, or to the significance of primary research in museum-based taxonomy: ten references appealing to "well-known facts" (species *x* is composed of three subspecies…) outweigh the only paper that, after original study, concluded that subspecies *y* is actually a separate species.

There is at last, at the 11th hour, a growing recognition of the relevance of taxonomy for conservation and life sciences in general (Dubois, 2003; Wheeler, 2004). Evidently, however, many researchers dislike current trends in taxonomy because they seem to add complications to scientific communications (Bruner, 2013; Rosenberger, 2012), and especially to conservation assessments (Meiri and Mace, 2007; Zachos et al., 2013). At the root there is the idea that pre-1980 mammalian alpha taxonomy was exhaustive and only new species actually discovered in the field should be added to the list; museum-based revisions,

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however incisive, are nothing more than a nuisance. Actually, the pre-1980 taxonomic "bibles" which are all too often regarded as being set in stone frequently originated from the work of one or two taxonomists based in a single museum and completed in relatively few years, often without the benefit of the regionally exhaustive material to which modern taxonomists customarily have access.

We cite here three recent examples, among several possible, that highlight the positive influence of a renovated taxonomy to mammal conservation and, incidentally, to modern biology.

1. Recently Rueness et al. (2011) published a paper entitled "The cryptic African wolf: *Canis aureus lupaster* is not a golden jackal and is not endemic to Egypt". This paper is to be welcomed as an attempt to shed molecular light onto an old problem raised many times in the past by morphological taxonomists, and we draw attention to it as an example of how, even in this case, the authors were unaware of much of the abundant earlier literature. The presence of the taxon in Libya is actually well-established (de Beaux, 1929; Toschi, 1954), making claims of endemicity clearly unwarranted. A previous study based on morphology (Ferguson, 1981) had concluded that the taxon *lupaster* should be referred to *Canis lupus* and not to *Canis aureus*, but this remained largely unnoticed among mammalogists, and Rueness et al. (2011) were under the impression that they had discovered something new and startling. Incidentally, was also little attempt to establish whether the name *lupaster* is actually correctly applied to this "cryptic African wolf": consultation of checklists such as Ellerman and Morrison-Scott (1951) would have shown that there are at least six names which have been applied to wild *Canis* from the Nile Valley in Egypt and the northernmost Sudan, and only examination of their type specimens would genuinely reveal which of them applies to the "cryptic African wolf", and which to some kind of jackal; a recent morphological study finds that, in fact, the name *lupaster* does not apply to "the cryptic African wolf" (Gonzalez and Groves, *in prep.*)!
2. The primate genus *Callicebus* was reviewed twice by Philip Hershkovitz (1909-1997), a leading neotropical mammalogist based at the Field Museum of Natural History in Chicago. In his first paper, largely on the Amazonian members of the genus (Hershkovitz, 1963), he accepted just three species (two Amazonian plus one in the coastal Atlantic Forest), each with several subspecies. After several years the same author published a second revision of the whole genus, recognizing 13 species, one of them newly described (Hershkovitz, 1988). Whether or not Hershkovitz changed his personal species concept is unclear, but what is clear is this: in his first study he examined 105 specimens, all in the Field Museum; by the time of his second study, he had examined 1188 specimens in 25 museums worldwide. The interesting question is not necessarily why he recognized later so many different species of titi monkeys; rather more interesting is why so few were accepted in his first review. What really is significant is that his 1988 sample was an order of magnitude greater than had been available to him a quarter of a century earlier. The effect of this enormous increase in the number of species is profound; right from the start, Hershkovitz (1988:6) stressed that his revised taxonomic account of *Callicebus* was in total disagreement with the "Amazonian Pleistocene refugia" hypothesis, proposed by Kinzey (1982) using the outdated three species model.
3. *Lepus corsicanus*, a taxon originating not from tropical little-known regions but from Southern Europe, was described and studied by early 20th century taxonomists such as De Winton and Miller, and subsequently placed as a subspecies under *Lepus europaeus* by Ellerman and Morrison-Scott (1951), without explanation except by implication, because these authors' work was explicitly based on the Biological Species Concept. Nothing more was heard of *Lepus corsicanus* until Palacios (1996) restored it to specific status and showed that it was a distinct species endemic of the Italian peninsula and Sicily. It is now a prime target of conservation policy in Italy, where it risked to become extinct unnoticed because of restocking programs with central European hares (Trocchi and Riga, 2001). Actually, game species subjected to restocking programs or other organisms affected by invasive species may be particular prone to "be killed" (cf. Morrison et al. 2009) by neglect due to XX century's taxonomy. It seems us that the ephemeral nature of the subspecies concept as recognized by the Biological Species Concept, and the over-lumping attitude (at species level) of the new systematics, combined to offer an overly simplified view of mammal species diversity and, thus, of conservation requirements (see also Carleton and Schefer Byrne 2006 for a case-study with genus *Otomys* in East Africa).

Our opinion is that Hershkovitz's 1963 paper on *Callicebus* was in total agreement with the low profile accorded to taxonomy during most of the XX century. If we are right, the main aim of mammal taxonomy for most of the century was not to describe diversity but to fix it. Henceforth the current age of so-called taxonomic inflation may be the historic answer to a long period of taxonomic deflation, and of undervaluing of the real extent of biodiversity.

Although we maintain that taxonomy (and taxonomists) must remain independent from other scientific and applied fields, our opinion is that a lesser inclusive species concept is positive to conservation biology (*contra* Meiri and Mace 2007), for several reasons:

- The adoption of the Phylogenetic Species Concept appears a pragmatic way to overcome the "subspecies problem" in conservation (Gippoliti and Amori, 2007); i.e. the disharmonic and non-coherent conservation treatment of below-species diversity by the conservation community for different taxa and in different regions of the planet depending on an animal's charisma and various national legislations. It should be remembered that global assessment of species by IUCN is mandatory, but not of subspecies. Thus the value of global assessment of species conservation status should be greatly enhanced;
- Risks associated with outbreeding depression both in *ex situ* programs and in restocking operations are minimized (cf. Frankham et al. 2012);
- Regional conservation efforts may be encouraged by recognition of endemic species rather than "subspecies".

To counteract conservation problems raised by increased species number (Isaac et al., 2004), we suggest to rely not on taxonomic stability, rather on conservation priority exercises that highlight phylogenetic or taxonomic uniqueness of taxa or areas (Amori and Gippoliti, 2001; Cadotte and Davies, 2010; Collen et al., 2011).

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