

# Anthropological taxonomy as subject and object

## The consequences of descent from Darwin and Durkheim

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Fig. 1. If humans are both similar to, and different from, other primates – as this turn-of-the-century advertisement highlights – then which set of relationships should be the focus of a scientific classification?



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1. A paraphyletic taxon is a group that contains some, but not all, of the descendants of a recent common ancestor. Thus 'reptiles (minus birds)' is paraphyletic, as birds fall phylogenetically within reptiles, and yet are contrasted with them. In order to be acceptable as a cladistic taxon, it must include all the descendants of the common ancestor; thus birds would have to be incorporated within reptiles, rather than contrasted with them. In the primates, prosimians (including tarsiers, but not the monkeys and apes, to whom tarsiers are nevertheless allied) are also paraphyletic.

2. Diamond (1992: 2) suggests that 'a zoologist from outer space' would miss these things, or ignore them. Clearly, postgraduate zoological education in outer space is not what it used to be.

3. Moreover, the group formerly known as australopithecines would now be nameless, for the subfamily Australopithecinae would no longer exist, and it designated a paraphyletic group in any case (*Australopithecus* plus *Paranthropus* minus *Homo*). Nevertheless it is easy enough to find examples of concurrent use of 'hominins' and 'australopithecines' in the literature – defeating the purpose of revising the classification in the first place!

4. Cladistic analysis maps the distribution of newly emergent traits onto taxa, and assumes that taxa sharing

Durkheim and Mauss argued long ago that classifications of nature were projections of social classifications. The very idea of kinship is a social device created to impose order on the grey zone where law and heredity overlap, and its logics and variations have been a bedrock of anthropological relativism since the field's professionalization. By the 1960s, ethnoscience and symbolic anthropology were focusing on the cultural processes of division and aggregation and the meanings they produced (Atran 1998, Bowker and Star 1999).

Biological anthropology, however, whose structure is dictated by the classificatory practices of science, has generally resisted intellectual engagement with the cultural context of that practice. The principal exceptions are Landau's (1991) study of palaeoanthropological explanation from the standpoint of folklore, and the common textbook discussions of 'splitting' and 'lumping' as scientific fads or idiosyncrasies. For example, the conference papers collected in Sherwood Washburn's edited volume *Classification and human evolution* strikingly contain no mention at all of the general, cognitive significance of classification – aside from a self-servingly relativistic comment from the palaeontologist Louis Leakey (1963: 34):

Since the names which we apply, *at any and every level in the taxonomic sequence* are inevitably arbitrary and artificial, it does not, I believe, matter what we decide to do, provided only that the majority of those who are concerned in the classification, at any given time, are agreed as to how they will use the classification system that is set up and provided they are clear as to what they mean by the different names that are applied (*italics in original*).

There are, however, clear tensions within the volume: chemist Emile Zuckerkandl coins the term 'molecular anthropology' to refer to the introduction of biochemistry into the study of evolutionary relationships in anthropology, and suggests that since only two amino acid differences

are detectable in the structure of gorilla and human haemoglobin, out of 287 possibilities, 'from the point of view of hemoglobin structure, it appears that gorilla is just an abnormal human, or man an abnormal gorilla, and the two species form actually one continuous population' (1963: 247). Similarly, Morris Goodman (1963) argues that the genetic similarity of humans to African apes (chimpanzees and gorillas), and their separation from orangutans, should take precedence in our classification over the divergence of humans from the apes. The palaeontologist George Gaylord Simpson rejects Goodman's suggestion on the grounds that '*Homo* is both anatomically and adaptively the most distinctive of all hominoids' and '[c]lassification cannot be based on recency of common ancestry *alone*' (1963: 28-29, emphasis in original). Shortly afterwards Simpson (1964) dismissed Zuckerkandl's judgment as 'nonsense', on the eminently sensible grounds that anyone who cannot tell a human from a gorilla on the basis of their haemoglobin need only look at something else.

Clearly there were a number of epistemic assumptions about the basis of scientific classification, and the purposes and methods of establishing biological relationships, which were coming into conflict. Not the least of these involves the philosophical practice of reductionism, to which anthropology bears a foundational antipathy. It is, after all, arguable whether or not molecular genetics provides the proper lens through which to see all of nature. But to confuse a human with a gorilla was beyond even the reductive scope of geneticists; for that you needed chemists!

### Problem 1: The origin of species

Not very long ago, a textbook of biological anthropology, if it ventured to produce this datum, would inform a student that there are approximately 170 living species of primates. This followed major primatology texts (Richard 1985, Smuts et al. 1987) and may provide some scientific context for understanding the adaptive and ecological

such a trait acquired it from a recent common ancestor. If traits can be acquired by gene flow, that is to say, horizontally rather than vertically, then the method breaks down, for such an evolutionary pattern would be reticulating rather than bifurcating. The human fossil record arguably looks this way (Holliday 2003).

5. But one should always footnote the fifth subspecies, *Homo sapiens moenstrosus*, a grab-bag category constructed by Linnaeus to accommodate particularly strange peoples from anywhere on earth.

6. E.O. Wilson was very possibly naive and genuinely surprised by the furious controversy ignited by the publication of *Sociobiology: The new synthesis* in 1975. Its political implications, however, were clearly not lost on others.

Ackermann R.R., Rogers, J. and Cheverud, J.M. 2006. Identifying the morphological signatures of hybridization in primate and human evolution. *Journal of Human Evolution* 51: 632-645.

Aiello, L.C. 1993. The fossil evidence for modern human origins in Africa: A revised view. *American Anthropologist* 95: 73-96.

Arcadi, A. 2006. Species resilience in Pleistocene hominids that traveled far and ate widely: An analogy to the wolf-like canids. *Journal of Human Evolution* 51: 383-394.

Atran, S. 1998. Folk biology and the anthropology of science: Cognitive universals and cultural particulars. *Behavioral and Brain Sciences* 21: 547-609.

Bendyshe, T. 1865. The history of anthropology. *Memoirs of the Anthropological Society of London* 1: 335-458.

Boas, F. 1911. *The mind of primitive man*. New York: Macmillan.

Bowker, G.C. and Star, S.L. 1999. *Sorting things out: Classification and its consequences*. Cambridge, MA: MIT Press.

Campbell, C. et al. (eds) 2006. *Primates in perspective*. New York: Oxford University Press.

Clark, G.A. and Lindly, J.M. 1989. Modern human origins in the Levant and western Asia: The fossil and archeological evidence. *American Anthropologist* 91: 962-985.

Conroy, G.C. 2002. Speciosity in the early Homo lineage: Too many, too few, or just about right? *Journal of Human Evolution* 43: 759-766.

Dawkins, R. 1993. Gaps in the mind. In: Cavalieri, P. and Singer, P. (eds) *The great ape project: Equality beyond humanity*, pp. 80-87. New York: St. Martin's Press.

Dennett, D.A. 1995. *Darwin's dangerous idea*. New York: Simon and Schuster.

diversity among our close relatives. Today, however, primatology texts (Strier 2006, Campbell et al. 2006) tell us that there are perhaps 340 living species of primates, about double the earlier figure. As a number invested with the 'truthiness' of scientific authority, the roster of scientifically accepted living species of primates still contextualizes our place in the natural order. Whatever scientific message it offers now, however, is presumably rather different.

Taken at face value, the proliferation of primate taxa might suggest that primate diversity is increasing – that is to say, that speciation is occurring at a rapid rate. But this inference is substantially at odds with the widely known fact that most primate species are threatened in the wild, and many indeed may face imminent extinction. Alternatively, it might mean that new, previously unknown, primate species are being regularly discovered. Actually, however, very few such discoveries are on record, notable exceptions being the golden bamboo lemur (*Hapalemur aureus*) and the highland mangabey (*Lophocebus kipunji*).

So what accounts for this explosive increase in the number of scientifically recognized primate species? The answer is conservation. Primatologists, and especially field primatologists, work under a threat that other scientists are spared: their subject matter is disappearing as a result of anthropogenic activities, ranging from direct predation to deforestation associated with economic development. While graduate students in astronomy may go to sleep secure in the knowledge that Jupiter will still be there when they retire, primatology students have no such assurance about their subjects.

Public interest in conservation and biodiversity has introduced new sources of funding into field primatology, which creates a professional incentive to highlight these issues as well. Primatology has thus evolved into advocacy for primates themselves: the leading journal in the field features a logo with an aye-aye imploring readers in Latin, 'Let us live!' Conservation legislation, however, has tended to be written at the species level, since a species is generally acknowledged to be the most natural unit of ecology. But such legislation may easily be circumvented, as long as another population of the same species is still extant somewhere.

The obvious response to this loophole would be to elevate the taxonomic status of local populations: make subspecies into species, and create the taxonomic space to do so by elevating species to genera. In this way more primate populations are protected, and the spirit of the legislation is retained. The only sacrifice would be any pretext to be representing 'nature' accurately or objectively.

This sacrifice is one that nearly all people interested in primates (myself included) are willing to make, for the preservation of primates in the wild is quite simply more important than an accurate tabulation of the number of 'real' species of them (see Strier 2006). Indeed, this 'taxonomic inflation' is not limited to the primates, but has been identified elsewhere in ecological studies (Isaac et al. 2004). The upshot, however, is that biological anthropologists do not really *know*, nor really *care*, how many species of living primates there actually are, that they present to students a number that bears little relation to whatever the actual number is, and that they quietly reduce the active and (anthropologically interesting) negotiation between nature and culture in this matter to an unproblematic fact of nature.

## Problem 2: You big ape!

Simpson (1971) dismissed the suggestion that the family Hominidae be expanded to incorporate chimpanzees and gorillas, on the basis that the scientific classification is not based on phylogenetic relations alone. The adaptive difference of humans from the 'great apes' was an obviously significant aspect of their evolutionary history.

Goodman's suggestion, in other words, was based on a poor grasp of the principles of zoological classification, which are intended to condense the holistic relationships of animals into a retrievable linguistic format. To express only genetic relationships, at the expense of all others, was at best reductive and at worst perverse.

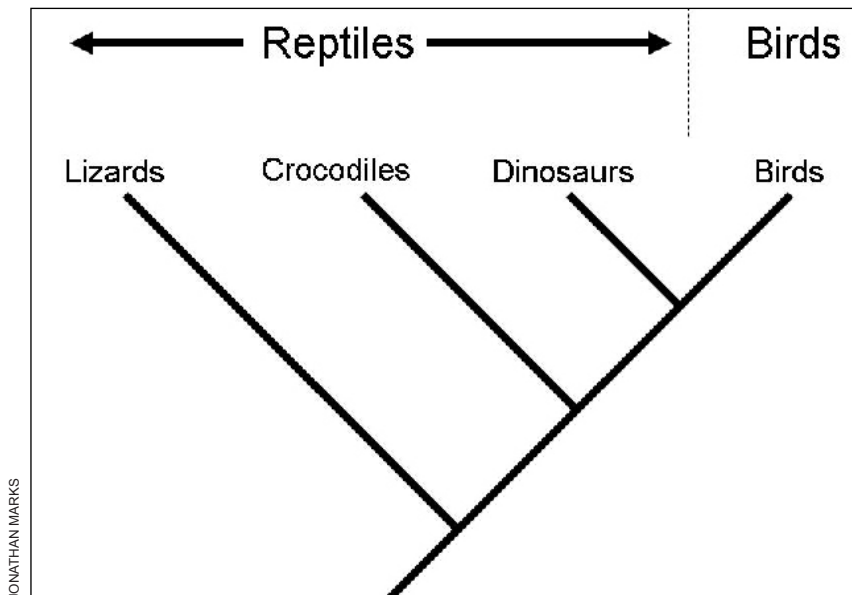
The 1980s, however, brought a philosophical change to the principles of zoological classification (Eldredge and Cracraft 1982). Cladism, or phylogenetic systematics, began to predicate classification solely on phylogenetic relationships, and not on divergence. In other words, what had been a mistake on Goodman's part in the 1960s was now no longer a mistake. Following the tenets of cladism, primate systematists began treading the path Goodman had advocated years earlier, associating humans and great apes together in a newly reorganized and expanded family Hominidae. The orangutan would be alone in the shrunken subfamily Ponginae, contrasted with humans, chimps and gorillas, now in an enlarged subfamily Homininae. The divergence of humans from the apes would be encoded at an even lower level. The merit of such a system is that it applies a single criterion consistently, that of recent common ancestry.

The system has additional merits: associating us so closely with highly endangered apes might be useful in building public awareness of primate conservation, and might also help confute the creationists. We are not so different from apes; we fall among apes; we *are* apes (Dawkins, 1994). Jared Diamond (1992) used this classification epigrammatically to call us 'the third chimpanzee'. Indeed, to challenge the classification is often to invite the charge of abetting the creationists, by focusing on the uniqueness of the human species (Dennett 1995).

And yet there is no reason why science should establish its classification in reaction to the creationists. Moreover, there are three good reasons for considering this new classification to be scientifically and intellectually inferior. First, adaptive divergence is literally a fact of life. To ignore it is to obliterate any number of familiar higher taxa: for example, reptiles (from which birds diverged), whales (from which dolphins diverged), fish (from which tetrapods diverged), and prokaryotes (from which eukaryotes diverged). While excising such paraphyletic taxa<sup>1</sup> is indeed a goal of some zoological systematists, many would simply acknowledge adaptive divergence as an important part of evolution in the century and a half since Darwin first wrote *The origin of species* about it. A cladistic classification cannot tolerate contrasting birds (Aves) with dinosaurs, crocodiles, and lizards (Reptilia), for the latter assemblage does not comprise closest relatives. Rather, it names lizards (Squamata) and contrasts them with birds, dinosaurs and crocodiles (Archosauria), the closest relatives of the lot, in the process burying the significance of a feathered flying reptile for the history of life on earth. 'For this reason,' writes Wilkinson (2002: 4), 'many biologists, including some who use cladistic analyses, are not enthusiastic about cladistic classification.'

Humans are in fact no more apes than birds are reptiles. We have ape ancestry, and, while retaining its vestiges, we have evolved away from it. In other words, there is nothing necessarily anthropocentric in acknowledging the adaptive divergence of humans, or of anything else, in a zoological classification. On the contrary, it could well be considered a very important part of the evolutionary history of life. (And who but a creationist would want to ignore the adaptive divergence wrought by natural selection?)

Second, humans have indeed diverged from the apes. There is, obviously, a lot of apeness recoverable in the human body and behaviour (Huxley 1863, Goodall 1971), but surely even the most incompetent observer could hardly miss the fact that in a group of apes in their rep-



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representative habitats, our species is the only one walking, talking, sweating, shaving, weeping, cooking, praying, building, marrying, bartering, bathing, covering themselves, reading, driving, seasoning their food, drinking things other than water and inhaling things other than air, using material culture (stripped twigs notwithstanding), and inheriting.<sup>2</sup>

Thirdly, the cladistic classification has the effect of separating orangutans – on the basis of their early phylogenetic divergence – from a group composed of humans, chimpanzees and gorillas. But this hardly reflects the biological (especially ecological) relationships, in which humans are the ones interacting with their environments in ways that are signally different (and again, hard to miss) from what the ‘other’ apes are doing. Why should we privilege the relationships of phylogeny over those of ecology, and separate the orangutan from the others, rather than separating the human? I like orangutans as much as the next fellow, but humans are at least as zoologically unique and as interesting as orangutans are; one could therefore argue for prioritizing the expression of the ecological position of humans in an evolutionary classification, rather than the phylogenetic position of the orangutan.

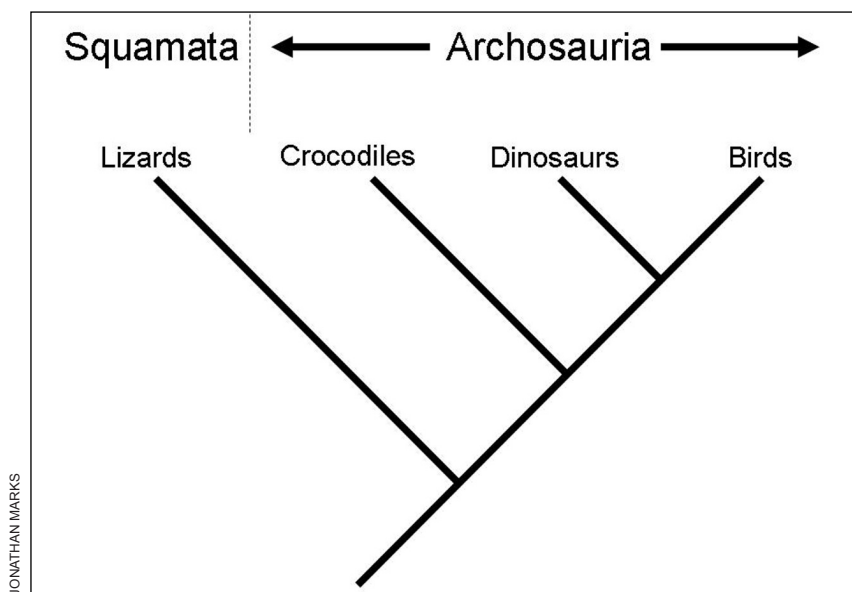
The issue is how to articulate the complex multidimensional relationships of species in the two-dimensional format established by Linnaeus a century before *The origin of species*. The biologist Julian Huxley (1955) once proposed contrasting humans with all other multicellular life, as the subkingdom Psychozoa – conscious animals. The point is that it is no more scientific to classify humans within the great apes than in contrast to them. Rather, it is the consequence of accepting and rigidly applying an arbitrary set of rules – in this case, rules that specifically privilege genealogical relationship over all other forms of biological relationship. And once again, the interesting negotiation of nature and culture becomes reduced to an unproblematic natural fact: we are apes.

**Problem 3: Meet the hominins**

Classically, the fossil bipedal apes and their descendants have been called hominids, derived from the family Hominidae, which was the group distinct from the family Pongidae, the great apes. However, if the family Hominidae now encompasses all the great apes (on the basis of their genetic similarity), then the bipedal group will have to be segregated at a lower taxonomic level.

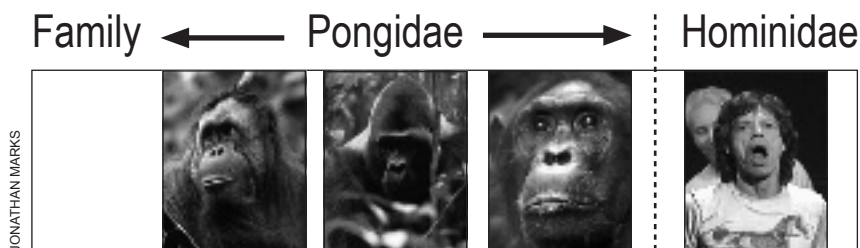
In the cladistic system, we split the orangutan off at the subfamily level and retain humans, chimps and gorillas as Homininae. There is no known fossil record for this group, but if there were, the species they represent would be called hominines, from the subfamily Homininae. Leaving the position of the gorilla somewhat vexed, we now split the human lineage off at a rarely invoked, even lower level – as the tribe Hominini. Its members are the genera and species of our bipedal relatives that we used to call hominids: now they are hominins.

In the older system, we theorized human ancestry by dividing hominids into those with large jaws and small brains (subfamily Australopithecinae, or australopithecines) and those with smaller jaws and larger brains (subfamily Homininae or hominines). We can't do that under the cladistic system, because those taxonomic levels are too high, and are now occupied by chimps and gorillas. In fact, lowering the taxonomic level at which humans are separated from chimps and gorillas has the consequence of dramatically condensing the taxonomic space available to theorize the hominins. As a consequence, hominin taxonomy becomes simply a species listing.<sup>3</sup> And of course, the list keeps growing, because of the taxonomic practice of ‘splitting’ in palaeoanthropology – that is to say, overestimating the amount of taxonomic diversity in fossil sam-

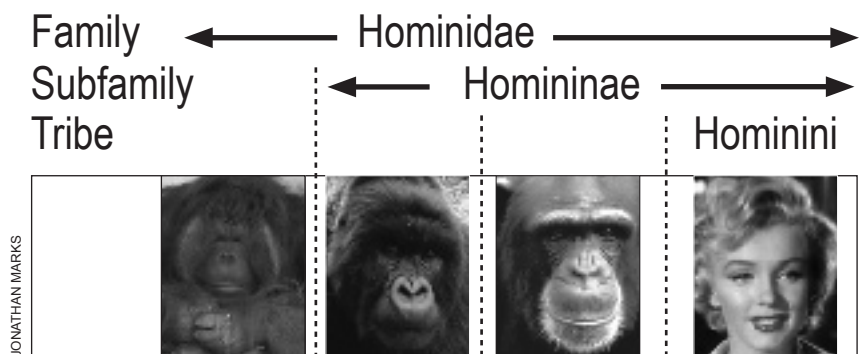


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Figs 2 and 3. Above: a classic ‘evolutionary’ classification, emphasizing the adaptive divergence of birds from a phylogenetically diverse assemblage of reptiles. Below: a ‘cladistic’ classification, using solely the phylogenetic relationships.



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Figs 4 and 5. Above: A classic evolutionary classification, emphasizing the adaptive divergence of humans from a phylogenetically diverse assemblage of great apes. Below: A ‘cladistic’ classification, using solely the phylogenetic relationships.

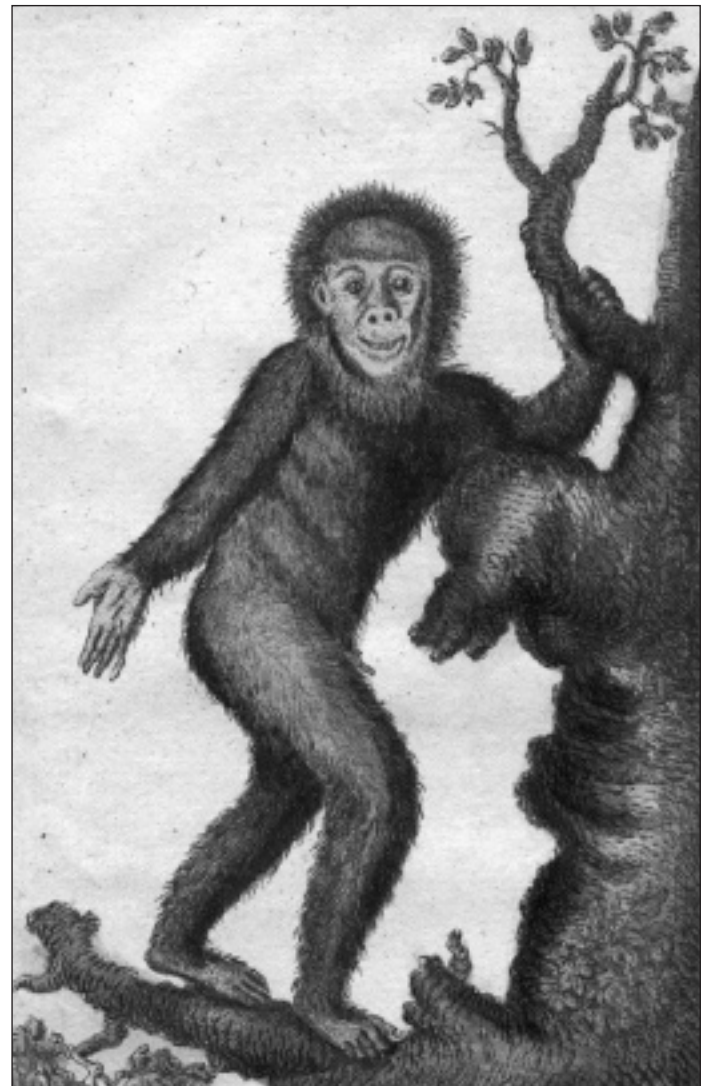
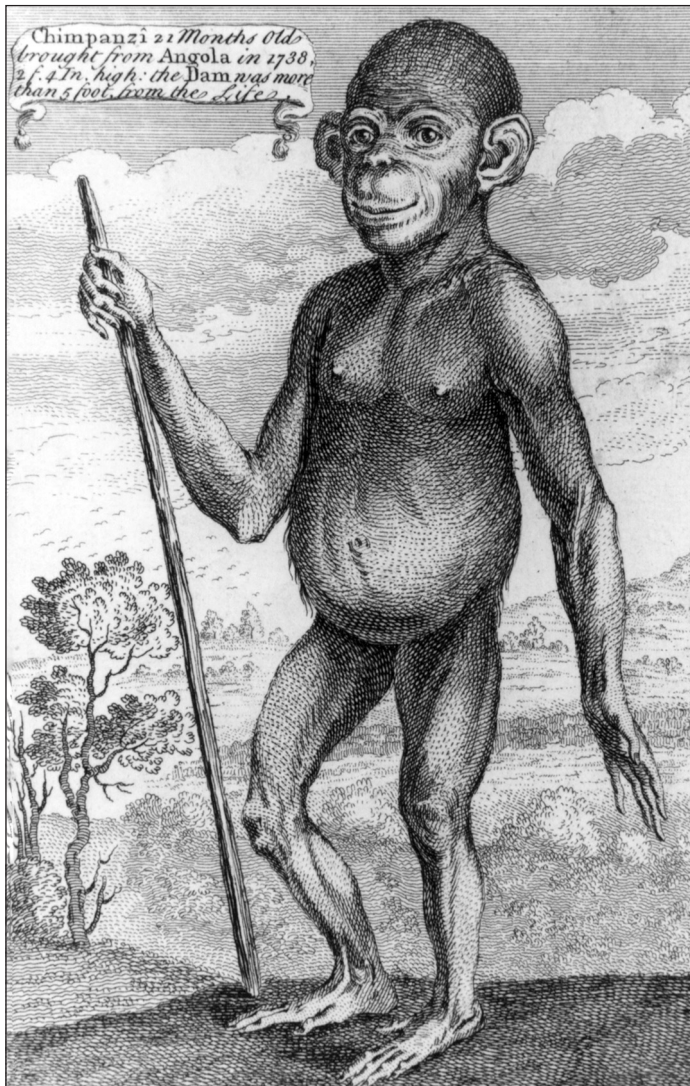


Fig. 6 (left). The London chimpanzee of 1738/9, the first ape widely displayed in Europe.

Fig. 7 (right). *Le Jocko*. From a reprint edition of Buffon's *Histoire naturelle*, c. 1800.

ples, and minimizing the amount of physical difference attributable to other sources, such as sex, geography, age and anatomical distortion.

Splitting, however, is not a mere fad or idiosyncrasy, as it is commonly trivialized. It is a strategic practice, sensitive to a moral economy of science. Splitting permits more scholars to possess and control key specimens; it is consequently a democratizing process. Possession and control of key specimens is a crucial commodity in a scientific culture where access to the original material is necessary for advancement in the hierarchy. Reifying a colleague's species may indeed help gain or maintain access. Moreover, discovering new taxa gives the appearance of progress in the field, whereas discovering more specimens of already well-known taxa does not. Further, the methods of cladistic analysis fail in principle below the species level,<sup>4</sup> so any application of cladistics favours splitting – that is what 'cladistics' literally means. And finally, interpreting the human family tree as 'bushy' rather than linear has ideological value (Gould 1984) – and you can't get a bushy macroevolutionary tree without species.

The upshot is that new textbooks of physical anthropology, and even of general anthropology, can readily present the first-year student with the theory that nine species of the genus *Homo* have arisen, and eight become extinct, in the last 2.6 million years: *Homo sapiens* (still here), *H. neanderthalensis*, *H. heidelbergensis*, *H. antecessor*, *H. erectus*, *H. ergaster*, *H. rudolfensis*, *H. habilis* and *H. floresiensis*. Leaving aside the problematic *H. floresiensis* and the trivial *H. sapiens*, it is of course possible that the human genus was indeed so speciose and so tragically beset. There is certainly plenty of support for

this interpretation in the scholarly literature (Wood and Collard 1999, Tattersall and Schwartz 2000). The point I wish to make, however, is that this is again by no means an unproblematic fact of nature. It is, at best, a contested fact (Conroy 2001), and at worst a metaphysical proposition, and thus neither a fact nor a non-fact at all.

The reason species are widely taken to be natural units is that there is, at least in principle, a non-arbitrary criterion for their establishment. Two organisms are parts of the same species if they participate in a common gene pool, and see one another as potential mates or competitors for mates. Unfortunately, that criterion is not directly available to the palaeoanthropologist (Godfrey and Marks 1991). One can adopt proxy measures, and make analogies to levels of skeletal differentiation found in living species (assuming living species are themselves unproblematic), but ultimately the answer to the question of whether *Homo erectus* and *Homo ergaster* could interbreed is not only unknown but unknowable. We can continue to learn how different they were from one another, but what is at issue is not the fact of difference but, once again, the interpretation of that difference.

There are some reasons to suspect that biological anthropology is currently grossly overestimating the speciosity of the genus *Homo*. First, the differences among the presumptive species are subtle to begin with; this is adequately attested every semester, as first-year anthropology students invariably need to have the differentiating features pointed out to them. That such small differences can still be made the subject of such intense professional scrutiny attests mainly to the long-standing power of the skull as a fetish object in physical anthropology. The skull is, if any-

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Leroi, A.F. 2005. A family tree in every gene. *The New York Times*: 14 March.

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— 2002. *What it means to be 98% chimpanzee*. Berkeley: University of California Press.

Mesoudi, A., Whiten, A. and Laland, K.N. 2006. Towards a unified science of cultural evolution. *Behavioral and Brain Sciences* 29: 329-383.

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Relethford, J.H. 1998. Genetics of modern human origins and diversity. *Annual Review of Anthropology* 27: 1-23.

Richard, A.F. 1985. *Primates in nature*. San Francisco: W.H. Freeman.

Scheper-Hughes, N. 2006. Alistair Cooke's bones: A morality tale. *Anthropology Today* 22(6): 10-15.

thing, over-analysed and under-understood. Indeed, even our long-standing appreciation for the skull's sensitivity to the conditions of growth and development (Gravlee et al. 2003) is still perceived as threatening in some corners of physical anthropology (Sparks and Jantz 2002).

Second, there is considerable anatomical and technological continuity among these taxa (Clark and Lindly 1989, Wolpoff and Caspari 2000). Many reject the split between *Homo ergaster* and *Homo erectus*. *Homo rudolfensis* is there principally to accommodate ER-1470, which remains an oddity nearly four decades after its initial, controversial discovery. Rather few physical anthropologists actually do differentiate '*Homo heidelbergensis*' and *Homo erectus* from '*Homo antecessor*' in practice.

Third, we are only dealing with about two and a half million years here. In addition to our obsession with the meanings of skull bits, our time frame is quite narrow, compared to the macroevolutionary timescale of vertebrate and invertebrate paleontology generally. To infer such high rates of speciation and extinction and such short species durations for the human lineage would seem to necessitate some qualification. Indeed, it seems counter-intuitive on the face of it, given human tendencies toward gene flow (Wolf 1982) and the cultural elaboration of difference (Boas 1911). On the other hand, we are not necessarily really talking about 'humans' here, but about a set of sister taxa to humans, whose dispersal patterns, subsistence strategies, symbolic faculties and gene pools are largely unknown. If their ecologies and demographics were like those of African baboons, they may well have formed geographically localized, taxonomically differentiated units, but if they were more like African wolves, then they probably did not (Arcadi 2006). To assign these extinct taxa the status of species is to make a positive statement emphasizing their difference and otherness. Yet Foley (2002: 33) acknowledges, '[it] is unlikely that these are all biological species [...] These are probably a mixture of real biological species and evolving lineages of subspecies.' Thus he removes the classificatory enterprise once again from even trying to represent biological 'reality' – the very reality to which biological anthropology claims privileged access. And if it is not self-consciously trying to summarize real patterns of biological diversity, then what is the classification really for?

#### Problem 4: No dogs or Neanderthals allowed

Splitting up the hominins, making them speciose rather than cohesive, also revives a hoary old issue: were the Neanderthals a part of our species, or not? This is an important question only in historical context: Europe is where palaeoanthropology began, and Neanderthals are the ones around whose remains the first palaeoanthropological questions were formulated. It stands to reason that they would be highly mythologized central players in the origin narrative of Europeans.

Our understanding of Neanderthals centres once again on the interpretation of difference. A synthesis of sorts



Fig. 8. (left). Profile portrait of a young man, by Masaccio, c. 1425 (image reversed). Right: Profile portrait of a young Neanderthal, after Masaccio, 2007.

was achieved in the 1940s as remains from Mount Carmel were interpreted as evidence for reproductive continuity between Neanderthal and human populations (Dobzhansky 1944), an interpretation of those particular remains that is no longer tenable. It is, however, far from clear just how genetic contact between Neanderthals and humans might be expected to appear in the skeletal remains that are accessible to palaeoanthropologists (Ackermann et al. 2006). There is considerable cultural continuity as well. While we tend to privilege the rapid cultural change that seems to characterize modern humans, that itself is a cultural feature, not a biological one. The development of art is often considered revolutionary (indeed so revolutionary as to have been triggered by otherwise undetectable genetic difference, in some accounts), but it is useful to observe that much more time elapsed between the cave art of Chauvet and the cave art of Lascaux (ca. 23,000 years), than between the cave art of Lascaux and the pop art of Warhol (ca. 15,000 years). Art has actually been remarkably conservative for most of its existence, and the cultural differences between Neanderthals and moderns may not signify absolute discontinuity.

The original 'mitochondrial Eve' work incorporated an argument for the reproductive incompetence of humans with Neanderthals, but this is now taken to be an overstatement at best (Templeton 1993, Relethford 1998). Neanderthal mtDNA suggests the Neanderthal-human difference to be comparable to that between subspecies of chimpanzees. Making allowance for the fact that some would elevate chimp subspecies to species, it still seems clear that the decision about species status for Neanderthals is under-determined by both the palaeontological and the genetic data. Consequently, the Neanderthal genome is unlikely to resolve anything.

So why classify Neanderthals and humans as different species? Clearly, to emphasize their difference from us in a formal way. But that difference could also be expressed at a subspecific level. As Wolpoff (2003: 665) notes,

naming a Neanderthal species confuses the questions of whether Neanderthals are different from living people (they are) [...] and whether Neanderthal features can be found in subsequent Europeans (they can).

In other words, there is some kind and some amount of continuity between them and us that can be problematized and understood (Aiello 1993, Trinkaus 2005). From the other side, Tattersall (2003: 665-666) concedes:

No doubt, some Pleistocene hanky-panky occurred as the Cro-Magnons moved into Europe, if humans then were anything like they are today. But even enthusiasts concede that the species we know today as *Homo sapiens* is unlikely to owe much if anything to genetic influences acquired from extinct hominid species such as *H. neanderthalensis* in the process of driving them to extinction.

The problem is that his example holds equally for the English vis-à-vis the Tasmanians, without the deduction that they ought to be regarded as different species. At issue is a single fundamental question, and not so much a scientific as an aesthetic one: how different is different? Once again, science possesses no authoritative voice on the interpretation of difference, except to note its ubiquity and to describe some of its patterns. From the standpoint of anthropological knowledge and pedagogy, then, presenting the Neanderthals unproblematically as a sister species has the effect of representing as science what is actually hermeneutics – that is to say, a culturally situated interpretative practice derived from esoteric premises.

#### Problem 5: Clearing the way for *Homo sapiens Europaeus* (Linnaeus 1758)

The act of overestimating the divergence between Neanderthals and humans, and elevating them to species

Fig. 9. Crossing the line: the 'apeness' of humans (or 'humanness' of apes) evoked in an advertisement.



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status, has a significant consequence for the taxonomy of living humans. As long as Neanderthals are a contrasting subspecies to *Homo sapiens sapiens*, the subspecies of all living peoples, there is no formal taxonomic category remaining in which to encode the differences among extant human populations. The subspecies is as low as it gets. If the Neanderthals are elevated to a separate species, however, a vacuum is created at the subspecies level, within which the differences among human races can be encoded. In physical anthropology, Linnaeus' 'subspecies' and Buffon's 'race' have been largely coextensive since the late 18th century (Marks 1995). Linnaeus himself identified four human subspecies, upon which he projected differences of colour, law and dress, in addition to geographical landmasses and the Aristotelian humours (Bendyshe 1865, Marks 2002).<sup>5</sup>

Subsequent students of human form used different criteria and divided the species up in different ways, into different constituent elements. For many decades physical anthropologists worked under the illusion that this process could be free of cultural value, and yet (in America, at least) its principal exponents saw their work explicitly as justifying slavery (Josiah Nott and George Gliddon), eugenics (Hooton) and segregation (Carleton Coon; see Jackson 2005). The reason, it now seems clear in retrospect, is that one generally studies racial science in the first place with an eye towards explaining social difference by recourse to nature, rather than to human agency. The argument that social inequalities are attributable to innate differences, rather than to remediable, if pervasive, injustices carries obvious political import. Both sides of the political spectrum acknowledge this issue, as it arises with each generation. The only ones who don't readily recognize it tend to be the purveyors of the scientific research themselves, who adopt a (usually disingenuous) posture of objectivity and distance from the social and political implications, which they hope (incorrectly) will absolve them of responsibility.<sup>6</sup>

Nevertheless, this process of 'naturalizing difference' is at the heart of subspecific taxonomy of *Homo sapiens*. It is again a cultural act, assigning people to qualitative categories on the basis of diverse criteria – notably geography, facial form and allele frequencies – while judging people within each category to be only trivially different from one another. The study of human biological diversity has progressed to the point that we now realize its primary aspects to be polymorphic (from person to person within the same population), clinal (forming gradients), and local (as opposed to global), with only a small component being macrogeographical. Culture, however, abhors a vacuum, and a taxonomic void at the subspecies level has significant social consequences. After all, there is biomedical capital poised to exploit the ambiguity of genetics in articulating patterns of human difference (Kahn 2004, Duster 2005), and there are indeed biologists willing to meet the demand for ostensibly authoritative voices to reinscribe race as a natural and explanatory category (Leroi 2005, Wade 2006). Issues of biological classification are thus never very far from the nexus of cultural value and social action – as much in the 'modern' as in the 'primitive' forms of classification examined by Durkheim and Mauss – and didn't we already know that?

## Conclusion

My purpose here has been to identify and explore an intellectual space inbetween physical-biological anthropology and social-cultural anthropology – namely, the representation of our place in the natural order through practices of organization and naming. The articulation of our place in that order is not an unproblematic fact of nature; it is the result of a set of complex, situated cultural practices. While many socio-cultural anthropologists have left the dichotomy of 'nature' and 'culture' far behind them, it is still commonplace to find that dichotomy in the background of biological discussions, for example, of 'human nature' (Pinker 2002, Konner 2004), 'memetics' (Mesoudi et al. 2006), and, of course, 'race'. This background serves immediately to render any such biological or naturalistic discourses inherently facile.

What would be more satisfactory is a franker recognition of the interpenetration of 'natural' and 'cultural' facts – itself common knowledge in anthropology. This appreciation is indeed arising in some areas of contemporary biological anthropology, where science and human rights are increasingly coming into conflict. If modern ethical standards repudiate the use of biomedical data collected without appropriate consent, it becomes increasingly difficult to sanction the use of bio-anthropological data collected under analogous conditions.

Genetic specimens exist in a network of meanings about kinship and personhood, including classical issues of blood magic, and modern issues of the commodification of the body (Scheper-Hughes 2006), which biological anthropologists can continue to ignore or downplay only at their peril. Osteological specimens are similarly central players in narratives of ancestry and history, and those aspects again can no longer be ignored or downplayed, as the high-profile cases of Sarah Baartman (Tobias 2002) and Kennewick Man recently demonstrated.

Where the classic and deceptive division between natural and cultural facts still holds sway in biological circles, it may be incumbent on anthropologists to complete the education of biologists and, to the extent that they may identify intellectually with biologists, of biological anthropologists as well. On the bright side, this opens up the possibility of compelling biological anthropology students to think critically about the cultural nature of science, including, most especially, their own. ●