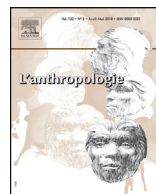




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Original article

New pigeons in old holes: Anthropology, algorithms and alpha taxonomy

*De nouveaux types dans d'anciennes catégories :
anthropologie, algorithmes et taxonomie*

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ABSTRACT

Almost three-quarters of a century ago, Ernst Mayr (1950) shocked paleoanthropologists into an exceptionalist taxonomic minimalism that lingers today. As a result, a constant stream of new fossil discoveries has been shoehorned wherever possible into existing taxa, almost irrespective of morphology. This compression has been especially true at the level of the genus, where virtually all except very early hominins have over the last couple of decades been classified and named according to the unconscious algorithm, “if it isn’t *Australopithecus*, it is *Homo*” (and vice versa). This has led to a vastly overstuffed genus *Homo*, which has lost any phylogenetic or morphological coherence. Genera are monophyletic clusters of species; and although their boundaries in terms of species content are arbitrary, they do contain phylogenetic structure that ultimately needs to be expressed. Homininae is a large and diverse taxon; and this essay is a plea to paleoanthropologists not only to reflect the phylogenetic structure within it by employing an adequate number of generic and species names, but at the same time to acknowledge the internationally agreed rules (especially of priority) that govern the naming of zoological taxa.

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R É S U M É

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Il y a près de trois quarts de siècle, Ernst Mayr (1950) a choqué les paléanthropologues avec un minimalisme taxonomique exceptionnel et non justifié qui persiste aujourd'hui. En conséquence, un flux constant de nouvelles découvertes de fossiles a été inséré dans la mesure du possible dans les taxons existants, presque indépendamment de leur morphologie. Cette compression a été particulièrement notable au niveau du genre, où pratiquement tous, sauf entre les tous premiers hominines, ont été classés et nommés au cours des deux dernières décennies selon l'algorithme inconscient, « si ce n'est pas *Australopithecus*, c'est *Homo* » (et vice versa). Cela a conduit à un genre *Homo* largement surchargé, et manquant de toute cohérence phylogénétique ou morphologique. Les genres sont en principe des groupes d'espèces parentés qui sont monophylétiques. Bien que leurs frontières en termes de contenu spécifique soient arbitraires, elles doivent contenir une structure phylogénétique qui doit finalement être exprimée dans la taxonomie. Homininae est un taxon vaste et diversifié, et cet article est un appel aux paléanthropologues non seulement à refléter cette structure phylogénétique en employant un nombre adéquat de noms génériques et d'espèces, mais aussi de reconnaître les règles internationalement reconnues (en particulier de priorité) qui régissent la dénomination des espèces zoologiques.

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1. Introduction

It is one of the most unfortunate ironies of evolutionary biology that the first major commentary on the human fossil record by a world-renowned systematist should have caused a deep and long-lasting aversion to taxonomy and systematics among paleoanthropologists. Most biologists take it as read that knowing exactly how the natural world is structured is prerequisite to understanding everything else about it; and even paleoanthropologists are fond of quoting Theodore Dobzhansky's epithet that "nothing in biology makes sense except in the light of evolution" (Dobzhansky, 1972). Yet, while in no other area of paleontology does one ever (let alone regularly) expect to hear it said that "taxonomy is just quibbling about names," even today such mindless declarations are often taken by paleoanthropologists to be emblematic of worldly sophistication.

This regrettable situation can be laid firmly at the feet of the distinguished ornithologist Ernst Mayr, one of Dobzhansky's co-architects of the New Evolutionary Synthesis and the author of the hugely influential *Systematics and the Origin of Species* (Mayr, 1942). At a prestigious meeting held at the Cold Spring Harbor Biological Laboratory in 1950, Mayr (who likely had never seen an original human fossil) told the assembled good and great of anglophone paleoanthropology that the multiplicity of species and genera they recognized in the human fossil record was entirely illusory (Mayr, 1950). Instead, Mayr declared, there were only three species represented in the entirety of that record, all belonging to the single genus *Homo*. These were *Homo transvaalensis* (what we would today call the australopiths); *Homo erectus*; and *Homo sapiens* (including the Neanderthals). What is more, because according to Mayr (and Dobzhansky) the human ecological niche was so broad as to eliminate the possibility that more than one human species might exist at any one point in time, the three hominid species necessarily represented a single, time-transgressive, transformational series.

Mayr's audience was dumbfounded, and not just because it was hearing this gradualist dogma from the world's foremost authority on speciation and the splitting of lineages. It was also because the year 1950 marked not only the midpoint of the twentieth century but a moment of generational change in

paleoanthropology. Among those listening to Mayr there were distinguished representatives of the aging generation of prewar anthropologists/anatomists whose interests were still obsessively centered on the single species *H. sapiens*, but who were already uneasily if dimly aware that the system of expert pronouncement on which they had operated was effectively without theoretical justification (see Tattersall, 2009, for wider context). But there were, on the other hand, also members of the cohort of younger postwar biological anthropologists who were poised to launch the “New Physical Anthropology,” with its emphasis on behavior and function at the expense of typology.

Each of these two factions was ready, for its own reasons, to ditch the old way of doing things; and remarkably, both responded to Mayr's broadside in the same way, namely, to shy away from systematics altogether. The old guard dimly realized that it had been skating on thin intellectual ice, but was distressingly bereft of alternatives; the New Physical Anthropologists, in contrast, downplayed questions of taxonomy (and by extension of phylogeny) chiefly as a matter of principle. Either way, for more than a decade after Mayr's attack anglophone paleoanthropologists of all stripes steered clear of taxonomic names entirely, preferring to think of the human fossil record in terms of individual specimens that they referred to mostly by their provenance: “Swanscombe,” “Steinheim,” “Solo IV,” and so forth. When they were represented visually, phylogenies lost their traditional linear characteristics in favor of diagrams consisting of formless blobs within which those individual fossils floated (Tattersall, 2018).

As the twentieth century progressed, and the human fossil record steadily expanded, taxonomic names inevitably began to creep back into the paleoanthropological picture. The effective turning point came in 1964, when Louis Leakey (temporarily) crowned his search for “the earliest *Homo*” with his naming, on slender grounds, of *Homo habilis* from Olduvai Gorge (Leakey et al., 1964). But even then, Leakey (who was always ready to name a new species of anything at the drop of a hat) remained something of an exception to the rule. Indeed, Louis' own son Richard for many years more conventionally took taxonomic reluctance to the ludicrous opposite extreme by imposing a moratorium on awarding a species name to any of his Turkana Basin fossils that were believed to be classifiable in the genus *Homo*. Predictably, this bizarre policy led to a confusing plethora of “*Homo* spp.” in the literature; and Richard ultimately relented, though for reasons he never articulated. In the end, then, the sheer pressure of discovery began to assure that a new species name would occasionally appear in the human fossil record, if only for newly discovered materials that had to identified more satisfactorily.

Still, even if the slow accumulation of new hominin species and of new names acknowledging them was in the long run inevitable, Mayr's influence lingered powerfully in paleoanthropology. Most perniciously and pervasively, it expressed itself in the form of a tenacious and ongoing minimalist tradition that served not only to discourage naming new species when there was any alternative whatever, but that even more destructively acted to impede the creation of new genera into which those species might be organized. This tendency has been hugely detrimental to paleoanthropology, since even a small monophyletic group of species inevitably embraces a complex branching phylogeny. And the resulting phylogenetic structure is inevitably obscured if too many of those species are arbitrarily jammed into the same genus (Tattersall, 2017). In other words, while genera and other higher taxa may not be “real” biological units in the sense that species are, they are nonetheless the consequences of phylogenetic processes; and if they are to be meaningful at all, they need to reflect phylogeny in some coherent way. For there is no escaping the reality that, while there is no theoretical limit to the number of species a genus can contain, the information content of any undivided genus diminishes as its species content enlarges.

As a passive consequence of a rapidly expanding human fossil record, there are now close to thirty species generally recognized within the hominin subfamily (or, if you prefer a ground-up rather than a top-down classification, the hominid family). And to those of even a modest splitting inclination, there are perhaps half a dozen more. In contrast, there are rather few genera currently available in which to arrange those species if we want our classifications to mean anything, i.e., to reflect in some way the details of the complex phylogenies that underly them. Overstuffing a limited number of genera will, of course, conveniently eliminate the necessity to make some difficult decisions; but it will provide this tactical advantage only at the strategic expense of a significant loss of phylogenetic information.

There is little mystery about why paleoanthropologists have shied away from the vagaries of systematics, even as workers in other areas of paleontology readily acknowledge the necessity to confront them full on. The problem is, of course, one of scale; for paleoanthropologists scrutinize the human fossil record at a level of detail far greater than anything that is brought to bear on equivalent taxa of ruminants, arthropods, or lizards. And while it is an understandable consequence of self-study by a hugely egocentric species, this close focus brings its own problems. Specifically, this intensity of scrutiny makes systematics uniquely difficult, obliging researchers to recognize lineages (which must necessarily be historically discrete) at very fine levels within what is a very closely-knit and recently evolved group. Morphological differentiation is often minimal among the most closely related components of that group, especially in the hard tissue characters that preserve in the fossil record. What is more, aDNA studies have shown that introgression involving very recently differentiated hominin lineages has been a common theme in hominin history (see review by Reich, 2019), even where the lineages concerned are or were clearly on separate evolutionary trajectories—as was the case, for example, with Neanderthals and modern humans. Although it may marginally or even significantly affect their functional qualities, a modest degree of intermixing does not in the end diminish the biological individuation of such lineages; but it does mean that the phylogenetic signals encoded in their morphologies will be more difficult to extract. Still, it is nonetheless critical to make the effort to extract them, because it remains just as true at this fine level of resolution as it is at coarser ones, that if you haven't first properly identified the actors, you'll never understand the evolutionary play. Which is, of course, what we should all be committed to doing, irrespective of the theoretical stance we take.

2. The genus *Homo*

The unfortunate effects of Ernst Mayr's 1950 onslaught on paleoanthropological practice continue to reverberate, and nowhere more unproductively than in our notions of our own genus *Homo*. In recent years the number of species recognized within *Homo* has multiplied considerably, as new fossils have been shoehorned into it. The addition of each new form has broadened the morphological boundaries of the genus, in turn making it easier to rationalize squeezing in the next thing, almost regardless of morphology. When Linnaeus named the genus *Homo* in 1758, its type and only species (apart from *Homo troglodytes*, an indeterminate ape) was the large-brained *H. sapiens*, the standard by which the membership of other hominins in the genus now has to be judged. By the time the twentieth century approached its midpoint, *H. sapiens* had been joined in Europe and western Asia by the cranially distinctive but equally large-brained species *Homo neanderthalensis*, and in Eastern Asia by various forms of the rather earlier and smaller-brained *H. erectus*, which nonetheless had a cranial capacity considerably in excess of that of any australopith. Together, all three forms of *Homo* appeared to make a reasonably cohesive monophyletic group that Mayr declared to form part of a single transforming lineage (with australopith roots). Within that lineage, the inevitable morphological discontinuities were ascribed to the famous incompleteness of the fossil record.

The serpent then slithered into Eden when Louis Leakey and colleagues described *Homo habilis* in 1964. Many felt that this motley assortment of fragmentary hominin bones from several sites at Tanzania's Olduvai Gorge did not differ very much from the gracile South African australopiths; but his new *Homo* species satisfied Leakey's twin obsessions: his hunger both to find the ancient ancestor of the genus *Homo* on his own African turf, and to discover the maker of the simple stone tools that for years had been turning up at Olduvai. Based on morphology alone (for example, the holotype lower jaw had dental proportions broadly reminiscent of gracile australopiths, and at about 680 mL a fragmentary braincase was closer in volume to the australopiths than to *H. erectus*), it was difficult to argue that these materials deserved recognition as *Homo* (one influential authority had, indeed, established a 750 mL "cerebral Rubicon" for our genus); but to the firm believer in the theory of "Man the Toolmaker" that Leakey was, those stone tools were the clincher – even though the first application of the potassium-argon dating method in paleoanthropology had put the oldest of the fossils and tools at a then-staggering 1.8 Ma (Leakey et al., 1961); Leakey himself had informally

guessed a third of that age. This all took quite a bit of metabolizing on the part of Leakey's colleagues, but eventually most of them went along with the new and incredibly ancient "handy man."

After that capitulation, all bets were off. Discoveries by Richard Leakey's team in the Turkana Basin of northern Kenya included a fragmentary cranium (KNM-ER 1470) that was some 1.9 million years old and had a cranial capacity of around 800 mL, just above the cerebral Rubicon. For many observers this find settled the reality of *Homo habilis*, although the specimen is nowadays usually referred to *H. rudolfensis* (see Wood, 1999). Various other large-brained hominins were also eventually recovered in the same general region, some of which (notably the crania KNM-ER 3733 and 3883) were eventually classified as "early African *H. erectus*" or as *H. ergaster* (Groves and Mazak, 1975). One of them, the famous 1.6-million-year-old "Nariokotome Boy" (KNM-WT 15000; F. Brown et al., 1985), consisted of most of the skeleton of an adolescent who was not only substantially taller than any australopith, but who showed relatively modern body proportions and a brain of some 880 mL, almost as large as the much younger (> 700,000-year-old) original *H. erectus* skullcap from Trinil in Java. "*H. erectus*" fossils of varying morphologies have subsequently been found widely in Africa, at sites mainly in the ~1-million-year range but reaching back to as much as 2 million years: see Tattersall (2015) for a review.

More recently, the island of Flores, some way to the east of Java, has produced skeletal evidence of a form that was baptized *Homo floresiensis*: a tiny and very oddly proportioned hominin only around 100,000 years old, with a brain of 426 mL in volume, far down in the australopith range (P. Brown et al., 2004). Very recently, some tiny bone fragments over 50 thousand years old, from the island of Luzon in the Philippines, have been called *H. luzonensis* (Détroit et al., 2019), and might represent a similar form. Back in Africa, in the Rising Star cave system that lies adjacent to some of the most famous and much more ancient "Cradle of Humankind" australopith sites, abundant remains have been discovered since 2013 of another very diminutive hominins that has been called *H. naledi* (Berger et al., 2015). Originally guessed on their physical characteristics to have lived about 1.5 million years ago or more, with some very primitive features of the postcranial skeleton and brains in the 465–610 mL range, the Rising Star hominins amazed the paleoanthropological community yet more by turning out to be only about 300 thousand years old (Dirks et al., 2017).

Also quite diminutive in stature, and with notably small brains in the 546–775 mL range, is a series of five crania, some with associated partial postcrania, from the 1.8-million-year-old site of Dmanisi, in the Republic of Georgia (reviewed by Rightmire et al., 2006). They make a pretty heterogeneous group; but all have been allocated at one time or another to *H. habilis* or *H. erectus*, and the oddest of them currently rejoices in the magnificent (if taxonomically illegal) quadrinomen of *H. erectus ergaster georgicus* (Lordkipanidze et al., 2013).

Very occasionally, a new genus name has crept into the hominin vocabulary, but this has happened almost exclusively among the very ancient putative precursors (*Sahelanthropus*, *Ardipithecus*, *Orrorin*) to the small-bodied, small-brained, archaically proportioned, and later australopiths. In 2001, Meave Leakey et al. (2001) had the courage to place a distinctive but shattered and still inscrutable 3.5-million-year-old cranium from northern Kenya into the new hominin genus *Kenyanthropus*. But otherwise, even though the field of paleoanthropology has been regularly astonished over the last few decades by mind-boggling and totally unexpected discoveries such as those at Dmanisi and Flores and Rising Star, almost every new fossil found has been placed either in *Australopithecus* (as in the case, for instance, of the amazing Stw 573 "Little Foot" skeleton from Sterkfontein, and the new gracile species *A. sediba* from Malapa), or into our own genus *Homo*. The upshot is that both genera, but most egregiously *Homo*, are by now bursting at the seams. Any genus that can contain creatures as differently structured as *H. sapiens*, *H. floresiensis* and Dmanisi Skull 5, is a genus that is in a league entirely of its own among the mammals, vastly more variable than any other I know of. The genus is a particularly interesting taxon because it is the true basic *gestalt* grouping among mammals, depending on morphological similarity for its recognition in a way that species do not. Genera must, of course, be monophyletic; but beyond that, how inclusive they are is a matter of subjective taxonomic judgment (with that unspoken *gestalt* requirement); and over the span of mammals in general, it is remarkable how few instances there are of disagreement over generic boundaries. This suggests that mammalian taxonomists are genuinely perceiving something here about how nature is organized, however difficult that something may be to specify or quantify.

As currently conceived by most students of the human fossil record, the genus *Homo* clearly stands away from the norm among other mammal genera. Nonetheless, liberated from the necessity of considering broader patterns in nature by their intense focus on the origin of *H. sapiens*, postwar paleoanthropologists have continued to go about their idiosyncratic business unperturbed. Mayr shocked them into a minimalist mindset from which they have found it difficult to emerge, and as a result acknowledging the obvious complexities of hominin phylogeny has taken a back seat to the desire to recognize the fewest possible higher taxa within Homininae. This desire has continued to dominate even as the paleoanthropological generations have turned over and the hominin morphological spectrum has continued to swell as the subfamily's fossil record has expanded to an astonishing degree. For the past few decades, as its practitioners have bent their energies toward battering new fossils into pre-existing pigeonholes, the field of paleoanthropology has effectively been operating on the simplest possible systematic algorithm, at least as applied to Pleistocene hominins: "If it isn't *Australopithecus*, it must be *Homo*," or vice versa. Either way, the generic diagnosis is one of exclusion; and the dominance of this dichotomous mindset has meant that there has been effectively only one choice of genus for virtually all hominids that lived subsequent to about two million years ago. From a systematist's point of view, this is nothing short of ridiculous: the fact that *Homo naledi* is patently not *Australopithecus* does not change the reality that it has little significantly in common with the genus that is perforce defined by *H. sapiens*. What is more, there is self-evidently structure in the by now distinctly speciose hominin fossil record from the Pleistocene (however defined, long timescale or short); and we can be pretty sure that we are currently aware of only a small fraction of the hominin lineages that existed over that period. As a result, it is patently obvious that we are going to need to recognize more hominin genera if we are ever to properly characterize the evidently complex geometry of events in hominin evolution over the Pleistocene (Tattersall, 2017).

The notion that the genus *Homo* needed to be rethought was mooted as early as 1992, when Bernard Wood suggested that *Homo* as then constituted was probably neither monophyletic nor adaptively coherent (Wood, 1992). Several years later, Wood revisited the issue with his student Mark Collard (Wood and Collard, 1999), beginning their analysis by lamenting the absence of generally agreed criteria for membership in *Homo*. They filled this lacuna by suggesting that such membership should only be extended to hominins that were demonstrably more closely related to *H. sapiens* than to australopiths, and that showed similarities to our species in body mass, limb, and jaw proportions (that is to say, in relatively large body size, long legs, and small jaws in concert with reasonably large braincases). Successful candidates for *Homo* status should also show evidence of obligate bipedality, and a prolonged growth schedule. Wood and Collard regarded all of these tendencies as sequelae of a massive ecological shift toward living in open environments, thereby placing adaptation and "adaptive zone" at the fore as diagnostic benchmarks for our genus – a risky practical proposition, because of the high frequency of homoplasy among closely related forms. After applying their criteria to the fossils available at the time, Wood and Collard concluded that such forms as *Homo habilis* and *H. rudolfensis* did not qualify for membership in *Homo*, leaving *H. ergaster*, a form with a modestly large brain and reasonably modern body proportions, as the earliest bona fide member of our genus. This move certainly tidied up the genus *Homo* considerably, but the identity of *habilis* and its fellow *refusés* was left open. Wood and Collard did not explicitly assign the rejected hominins to *Australopithecus*, but the implication was there; and the authors' failure to broach the issue of what the fossils in question actually were served in practice to reinforce the dichotomous algorithm that still bedevils paleoanthropology. Sadly, while it certainly made the evidence for any *Homo* over two million years old look weak at best, Wood and Collard's intervention did nothing in the longer run to discourage the subsequent cramming into *Homo* of numerous younger (and older) fossils that would strain mightily to meet their morphological criteria.

A case in point is the energetic resumption of Louis Leakey's now time-hallowed search for "the earliest *Homo*" that has been a major theme of the past two decades in paleoanthropology (see review by Tattersall, 2014). A number of bits and pieces in the 2.0 to 2.5 million-year range from various African sites have been touted as "early *Homo*" (see Antón et al., 2014, and Tattersall, 2015, for reviews from different perspectives); and the latest entrant in the "earliest *Homo*" stakes is a partial mandible from Ledi-Geraru in the Afar region of Ethiopia that is dated to 2.8 million years ago (Villmoare et al., 2015). This specimen falls intermediate in time between older and younger fossils from nearby Hadar

that have been assigned, respectively, to *Australopithecus afarensis* and *Homo* sp. The Ledi-Geraru jaw matches small *A. afarensis* in size, and it is described as exhibiting a variety of other features in common with that species. It also shows certain indeterminate differences from that australopith, and it is said to have some mandibular features that “align it with early *Homo*.” The “early *Homo*” with which it is aligned, of course, already represents the genus at its most unattractively bloated; and the picture looks rather different if you compare this specimen with its counterpart elements in the type species *H. sapiens*. For, by general mammalian standards, there is precious little to justify the inclusion of both in the same genus. On the other hand, there is still enough to justify the describers’ conclusion that the Ledi-Geraru fossil represents a lineage separate from *A. afarensis*. It is even plausible that it might represent a lineage that was in some way ancestral to later forms generally classified as *Homo*, even if only in its bloated sense. But once again, the notion that “if it isn’t *Australopithecus*, it must be *Homo*” is obviating the possibility that this specimen might be named or classified in a way that might help to clarify its actual phylogenetic relationships, or that might even simply reflect the complexity of the hominin family tree at that early period (see [Tattersall, 2017](#)). The wider clade that contains *H. sapiens* may have deep roots; but if the genus to which our species belongs is to retain any morphological (or even adaptive) coherence, we must resist the desire to keep pushing it back in time regardless of any lack of conformity with the Wood/Collard criteria

3. Paleoanthropology and taxonomy

Almost as insidiously destructive as the “quibbling about names” claim, is the common practice of giving a formal nod to the importance of zoological nomenclature while completely ignoring the rules that govern it. This is an ingrained defect of paleoanthropological procedure that recedes back in time far beyond Ernst Mayr – indeed, it was one of the deficiencies of the field that Mayr specifically complained about in his 1950 address. Unlike other areas of paleontology, which emerged in the early nineteenth century out of geology and comparative anatomy, paleoanthropology developed in the later nineteenth century as antiquarians, archaeologists, and others sent hominin fossils to human anatomists and physicians for study. This practice was not without its own obvious logic, but it had one very major drawback. For, as well versed as they might have been in human morphology, those clinically oriented savants were firmly focused on the species *H. sapiens* and were at best only marginally concerned with the larger zoological context within which the hominin subfamily existed. They lacked much if any interest in evolutionary process, or in the formalities of systematic procedure; and they relied largely on expert pronouncement in justifying and communicating their conclusions. In the taxonomic arena, they threw around zoological names as if they were naming their pets. According to Ernst Mayr himself, Franz Weidenreich, the distinguished describer of the Peking man fossils, had “stated that in anthropology ‘it always was and still is the custom to give generic and specific names to each new type without much concern for the kind of relationship to other types formerly known’” ([Mayr, 1950](#): 109). In other words, the paleoanthropologists had remained deaf to Darwin’s insistences both on the importance of variation within named species, and on the tendency of nature to diversify; and this obliviousness expressed itself in everything they did, leading inevitably to a general insularity and to a spirit of paleoanthropological exceptionalism. In other fields of paleontology, hierarchies of zoological names reflected the structures of the relevant branches of the tree of life; in paleoanthropology, they existed simply for the experts’ convenience. As a result, at the time when Mayr’s useful housecleaning reduced Hominidae to the single genus *Homo*, there were some fifteen generic names in regular use just for the relative handful of fossils then known that we classify today as *Homo*.

The good news here is that fourteen of those fifteen genera have, mostly thankfully, disappeared into synonymy; the bad news is that the exceptionalist paleoanthropological attitude toward naming hominin fossils not only survived Mayr’s onslaught but remains alive and well seventy years later. A good example in point is supplied by [Roksandic et al.’s \(2021\)](#) recent creation of the new species *Homo bodoensis* to accommodate the Bodo 1 partial cranium from Ethiopia’s Middle Awash region, as well as much else from the middle Pleistocene. Now, there may well be good reasons for believing that the Bodo fossil deserves full species status; but Roksandic et al.’s argument for doing so is riddled with

misunderstandings of what zoological nomenclature is all about. They start by faulting Otto Schoetensack, the industrialist-turned-anthropologist who in 1908 named the species *Homo heidelbergensis* to which the Bodo cranium is often assigned, for being ignorant of the evolutionary synthesis and cladistics, notions that would come along only decades after his death. The fact that Schoetensack was a man of his time is, of course, entirely irrelevant to the legitimacy of the name he gave his fossil: the species name *H. heidelbergensis* stands or falls on whether anything closely comparable enough to be assigned to the same species was previously named, and the answer to that is clearly no. Furthermore, questioning the grounds for the “revival” of *H. heidelbergensis* in the 1980s is equally irrelevant to the key question of whether, today, the morphology of the holotype mandible from Mauer justifies species distinction. Roksandic and colleagues may or may not correctly lament that “revival of taxonomic names rarely produces desirable clarity;” but even to raise that dubious proposition in this context betrays a willful ignorance of the principle of taxonomic priority on which the stability of zoological nomenclature on this planet absolutely depends. If we cannot rely on priority, how are we to choose between multiple available names for the same organism? With their understanding of taxonomy garbled on this scale, one can almost see why Roksandic et al. can claim with a straight face that “the theoretical underpinnings of taxonomy, and hominin taxonomy in particular, is [sic] a potentially more serious impediment [than the sparseness of the fossil record] to understanding human evolution and the place of individual fossils in it.” Yet reason should prevail; and even the human anatomists who dominated paleoanthropology a hundred years ago would have been aghast at this suggestion.

Having expressed their general unhappiness with established taxonomic procedures, Roksandic et al. then get procedural. They do this while displaying virtuoso-level ignorance of the distinction between nomenclature and taxonomy, by recommending in a single breath that the name *H. heidelbergensis* be suppressed, and its contents reassigned to *H. neanderthalensis*. The first of these advocated acts is an objective and nomenclatural one, while the second is subjective and taxonomic: they are both procedurally and intellectually unrelated. The suppression of *heidelbergensis* can be rejected out of hand, since the only possible grounds for suppression of the name would be that the holotype is inadequate or not inherently distinctive – and if there is one thing on which everyone can agree, it is that the Mauer jaw is highly distinctive, the question being whether anything else is sufficiently similar to it to be considered conspecific. Even if Mauer is one-of-a-kind, then, *H. heidelbergensis* is a reality to be reckoned with. Whether all the other fossils recently assigned to *H. heidelbergensis* do indeed belong to the same species as Mauer and/or each other (see [Schwartz and Tattersall, 2005](#) for an overview) is something that can and will be endlessly debated, because its resolution depends on individual systematic judgment. But most observers would likely disagree with Roksandic et al. that some or all of their *heidelbergensis* material (including the Atapuerca Sima de los Huesos fossils that their finders no longer attribute to *H. heidelbergensis* [[Arsuaga et al., 2014](#)]) can be subsumed into *H. neanderthalensis*.

For reasons that never clearly emerge because their preferred phylogeny never does, Roksandic et al. also find it necessary to formally propose suppressing the species name *Homo rhodesiensis* that Arthur Smith Woodward gave to the Kabwe cranium in 1921. They had objected to use of the Mauer jaw as a holotype partly on the irrelevant grounds that it was a mandible, a “bone [that] is normally considered to be extremely plastic and may or may not reflect associated morphological changes in the crania” (mandibles probably constitute the largest single category of holotypes in the entire vertebrate fossil record). But there can be no such objection in the case of the Kabwe cranium, which is largely complete and magnificently preserved. No, apart from the tortuous systematic reasoning, the main problem here appears to be the “pernicious political baggage” deriving from the association of the fossil’s name with a country founded by Cecil Rhodes. Whatever one may think of Rhodes’ colonial methods, it is likely that not everyone will consider Roksandic et al.’s argument totally compelling that “Decolonizing anthropology should take precedence over rigid taxonomic rules.” Those rules exist for good reason. Taxonomy, systematics, and nomenclature are the very bedrock of our studies of human evolution; and phylogenies have to be properly sorted out, and preferably expressed in classifications, before we can reliably proceed to anything else. Stability in the nomenclature on which those classifications are founded is assured by the principle of priority; and nobody has yet come up with anything like a satisfactory alternative. Until and unless someone does, paleoanthropologists should

abandon their traditional exceptionalism, join the world of real paleontology, and read the *International Code of Zoological Nomenclature* (ICZN, 1999).

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