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Chauvet Cave rock art by 'Neanderthals'

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Abstract. The attribution of the rock art in Chauvet Cave to the Aurignacian and the challenges to its dating are reviewed. Similarly, the dominant view that the palaeoart of the Early Upper Palaeolithic technological traditions is generally the work of invading modern humans is tested against the lack of evidence of the presence of such hominins. Even the need to account for the physical changes of hominins towards the end of the Late Pleistocene by a replacement hypothesis is refuted. It is contended that what were replaced were not populations, but merely genes, through an unintended domestication selecting neotenous features; and that this process took place simultaneously in four continents.

KEYWORDS: Chauvet Cave, Aurignacian, Neanderthal, Domestication hypothesis

The antiquity of the Chauvet Cave rock art

The most painstakingly studied and perhaps also the most pristine Palaeolithic cave art site known is Chauvet Cave in the French Ardèche (Chauvet et al. 1995; Clottes 2001). The standard of the fieldwork being carried out there is peerless (Bednarik 2005). The site's rock art is also the best-dated of the Palaeolithic sites so far subjected to any form of scientific dating (Clottes et al. 1995; Valladas et al. 2004). Interestingly, the Chauvet Cave dating endeavours have attracted more sustained criticism than any of the other attempts to date European Pleistocene cave art (e.g. Zuechner 1996; Pettitt and Bahn 2003). The reason for this is that the Chauvet results have severely challenged the traditional stylistic chronology of Upper Palaeolithic rock art (together with numerous other factors; Bednarik 1995a, 2016). There is considerable disagreement on this point, with some authors defining Chauvet as blending in well with aspects of style and content of secure Aurignacian art, such as the series of portable objects from southwestern Germany, while others reject the Aurignacian antiquity of Chauvet on the basis of their individual stylistic constructs and favour its placement in the Magdalenian.

It is very healthy to subject scientific propositions to falsification attempts and all current dating claims for rock art, anywhere in the world, are tentative and based on experimental methods. They are presentations of testable data,

Figure 1. *Cave bear images in Chauvet Cave, being examined by the author, photograph courtesy of J. Clottes.*

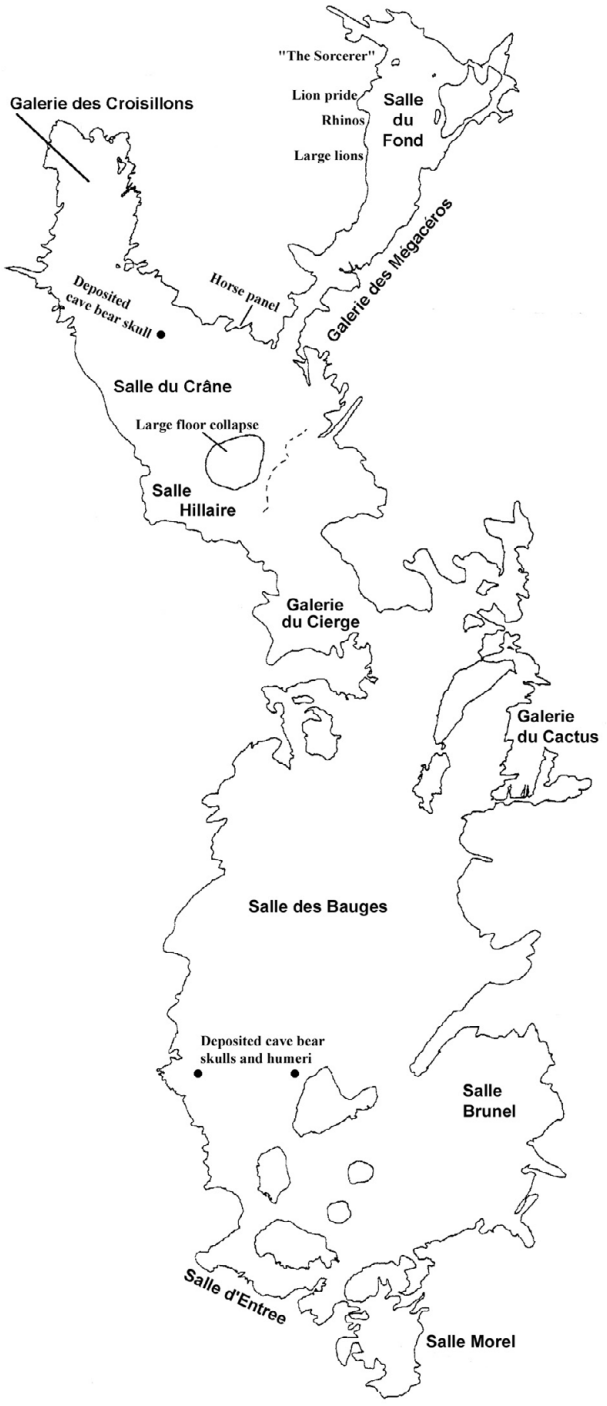

Figure 2. *Map of Chauvet Cave, France.*

and need to be interpreted in the context of the considerable qualifications that apply to them all (Bednarik 2002). However, the use of stylistic argument (i.e. rhetoric based on untestable cognitive processes involving autosuggestion that have been shown to fail time and again) needs to be questioned. The issue is not whether stylistic constructs are valid, but that they are intuitive. To see how such revisionist efforts fare in the case of Chauvet Cave, the following is offered for consideration.

Among the 3703 identified faunal remains found on the floor surface of the extensive cave, those of the cave bear account for 91.8% (Philippe and Fosse 2003); and there are about 315 identifiable cave bear hibernation pits preserved in the cave. Clearly it was a bear hibernation site, like thousands of others across Europe (Bednarik 1993), and probably so for tens of millennia. The most recent cave bear finds in the main cave are about 24 000 years (24 ka) old, while the Salle Morel appears to have remained open to that species until 19 ka ago. The timing of the collapse of the cave entrances is confirmed by the recent dating to 18 ka BP of a stalagmite grown on one of the uppermost collapse boulders inside the blocked original entrance. The collapse must have occurred significantly earlier, and since about 24 ka ago, the cave was only entered by small animals, such as snakes, martens and bats. Unless the disappearance of the cave bear from the Chauvet record also marks its extinction in the region, it also precludes access to humans in the Magdalenian technological period. In addition, a Magdalenian age of the rock art is precluded by the simple fact that clear depictions of cave bears occur in Chauvet, and that this species is thought to have been extinct in the region by the beginning of the Magdalenian (Rabeder et al. 2000: 107). In the three bear images in a western side chamber of the Salle des Bauges, the characteristics distinguishing *Ursus spelaeus* from *Ursus arctos* (e.g. steep forehead) are deliberately overemphasised (Fig. 1).

So far, three instances of anthropic deposition of cave bear remains have been observed on the cave floor, two in the Salle des Bauges and one in the Salle du Crâne (Clottes 2001; Bednarik 2005, 2007). They are also of importance to the relative dating of the human activity in the cave (Fig. 2). Evidence for cultural placement of cave bear skulls and long-bones has been reported from many caves, especially in central Europe, but it is temporally restricted to the final Mousterian and Aurignacoid traditions, most notably the Olschewian (Abel 1931; Andrist et al. 1964; Bächler 1940; Bayer 1924, 1928, 1929a, b, 1930; Bednarik 1993; Bégouën and Breuil 1958; Brodar 1957; Cramer 1941; Ehrenberg 1951, 1953a, b, 1954, 1956, 1957, 1958, 1959, 1962, 1970; Kyrle 1931; Malez 1956, 1958, 1965; Mottl 1950; Rabeder et al. 2000; Rakovec 1967; Stehlin and Dubois 1916; Trimmel 1950; Trombe and Dubuc 1946; Tschumi 1949; Vértes 1951, 1955, 1959, 1965; Zotz 1939, 1944, 1951). This cave bear 'cult', as it was unfortunately called in the mid-20th century, remains unrefuted, despite the endeavours of Koby (1951, 1953; Koby and Schaefer 1960) and others (Jéquier 1975). Generally, this evidence is in excess of 30 ka old at the known sites, and if the finds in Chauvet are of the same tradition, which seems very likely, the first phase of the cave's human use must also predate that time. That does not necessarily prove that the cave's early rock art phase has to be of the same period, but the onus to demonstrate that it is not is on those rejecting the Aurignacian attribution of this art. No such refuting evidence has been offered, and the doubters seem to be inspired by traditional stylistic reasoning alone.

Some of their arguments are mistaken or simply false:

> Nevertheless, the rock and cave art which is definitely known to be Aurignacian looks pretty crude and simple, a long way from Chauvet — which of course is why the Chauvet dates caused such a shock. […] [W]hat are the chances that a single Aurignacian cave would contain so many different features, themes, styles and techniques which, over a hundred years of study, have become so

> strongly and indubitably associated with later periods? (Pettitt and Bahn 2003: 139)

Very little rock art can be attributed to the Aurignacian (or for that matter to any other period, anywhere in the world) with adequate confidence to make such sweeping claims. The conceptually or cognitively perhaps most complex portable art of the Upper Palaeolithic is of the Aurignacian, including the two therianthropes from Swabia (Hohlenstein-Stadel, Schmid 1989; and Hohle Fels, Conard et al. 2003), the Hohle Fels female figurine (Conard 2009), and the anthropomorph from Galgenberg (Bednarik 1989), so why should we be 'shocked' to observe a similar level of sophistication in Aurignacian rock art? 'Aurignacians' seem to have been somewhat interested in 'dangerous animals' and vulvae (Delluc and Delluc 1978), and these do feature prominently enough in Chauvet. Moreover, it is obvious that Chauvet comprises at least two art traditions, so the variety of content and techniques is also no surprise. Finally, Chauvet is certainly not alone. The author has long considered the complex early phase of the cave art in Baume Latrone to be of the Aurignacian (Bégouën 1941; Drouot 1953; Bednarik 1986). Moreover, the small corpus of l'Aldène, reflecting the principal faunal elements in the Chauvet art, was created before the decorated passage became closed 30 260 ± 220 BP (Ambert et al. 2005: 276–7; Ambert and Guendon 2005). Other sites will no doubt be found to belong to those early traditions.

It is more appropriate to ask, what are the chances that Zuechner's idea, that all of the 250 charcoal samples so far analysed from Chauvet are derived from fossil wood, is correct? Far more likely than the involvement of fossil wood would be the use of much earlier charcoal, but that argument is not even made in respect of Chauvet, perhaps because a few of the dates come from torch marks. The possibility of a systematic error in all of these internally or stratigraphically consistent dates, implied by Pettitt and Bahn, is also specious: why should this affect all the dates from one site, but none of those from other sites, which these writers are in agreement with? Their argument could be made if they presented some evidence that points to a systematic distortion at just the one site, but without such data their case remains unsubstantiated.

The real problems with Chauvet are not even considered by the critics of the dating attempts, who seem only concerned with salvaging a superseded stylistic chronology. All carbon isotope determinations of the *European late Pleistocene shift* in southern Europe need to be considered sceptically, because of the effects of the Campanian Ignimbrite event and the cosmogenic radionuclide peak about a millennium earlier (Fedele et al. 2002). The best available ^{14}C determinations for the CI eruption place it between 35 600 ± 150 and 33 200 ± 600 carbon-years BP (Deino et al. 1994), but the true age of the event has been suggested to be 39 280 ± 110 BP, derived from a large series (36 determinations from 18 samples) of high-precision single-crystal $^{40}Ar/^{39}Ar$ measurements (De Vivo et al. 2001). Alternatively, Fedele and Giaccio (2007) have proposed that a significant volcanogenic sulphate signal in the GISP2 Greenland ice core, occurring precisely 40 012 sidereal years BP, represents the Campanian eruption. Therefore, in southern France, carbon isotope dates only marginally lower than the carbon age of the CI event may well be several millennia too low, and the true age of the early Chauvet phase could theoretically be as high as 36 to 40 ka BP.

Who created the Chauvet cave art?

The second important issue to be considered is, who were the people that made the Chauvet art? We may reasonably assume that they possessed an Aurignacian technology, but what about their physical characteristics? Now that the only securely dated anatomically 'relatively modern' human remains in Europe are 27 700 years or younger (Henry-Gambier 2002), earlier dated finds should be considered to be of 'Neanderthaloid' people. The entire issue of dating nearly all Würmian human remains from Europe has undergone incredible changes in recent years. For instance, the sensational exposure of all datings by Professor R. Protsch as fraudulent (Terberger and Street 2003; Schulz 2004; Street et al. 2006) means that there are now no post-Neanderthal human remains known in Germany that are more than 18 600 years old. The recently dated Mladeč fossils, between 26 330 and 31 500 carbon years old (Wild et al. 2005), lack credible stratigraphic provenience (Bednarik 2006) and are not modern, but are intermediate between robust and gracile *Homo sapiens*. The same applies to some degree to the Crô-Magnon specimens, which in any case now appear to be of the Gravettian rather than the Aurignacian (Henry-Gambier 2002). The similarly ambiguous Peştera cu Oase mandible (Trinkaus et al. 2003) and the subsequently found facial bones from a different part of the same large cave, thought to be 35 ka old, are both without archaeological context and also neither modern nor typically Neanderthal. Much the same applies to the six human bones dated from another Romanian cave, Peştera Muierii, which are intermediate between robust and gracile Europeans (Soficaru et al. 2006). The four specimens from Vogelherd (Fig. 3), however, are clearly modern, but their claimed age of 32 ka has now been rejected convincingly: they are Neolithic and are all between 3980 and 4995 carbon-years old (Conard et al. 2004). The 'Neanderthaloid' Hahnöfersand skull, formerly 36 300 years old, is now a Neanderthal of the Mesolithic, at only about 7500 years (Terberger and Street 2003), and the Paderborn-Sande skull, also dated by Protsch, is not 27 400 years old, but only 238 years. Another specimen often cited by the African

Figure 3. *Stetten 1, from Vogelherd, Germany, is not Aurignacian, as long claimed, but is Neolithic.*

Eve advocates as an early modern, though still fairly robust individual is from Velika Pećina in Croatia, now safely dated to about 5045 carbon years.

French contenders for EUP age present a mosaic of unreliable provenience or uncertain age, and direct dating is mostly not available. Like the Vogelherd and other specimens, those from Roche-Courbon (Geay 1957) and Combe-Capelle (originally attributed to the Châtelperronian levels; Klaatsch and Hauser 1910) are thought to be of Holocene burials (Perpère 1971; Asmus 1964), and the former is now apparently lost. Similar considerations apply to the partial skeleton from Les Cottés, whose stratigraphical position could not be ascertained (Perpère 1973). Finds from La Quina, La Chaise de Vouthon and Les Roches are too fragmentary to provide diagnostic details. The *os frontale* and fragmentary right maxilla with four teeth from La Crouzade, the mandible fragment from Isturitz and the two juvenile mandibles from Les Rois range from robust to very robust. Just as the Crô-Magnon human remains now appear to be of the Gravettian rather than the Aurignacian, so do those from La Rochette. The Fontéchevade parietal bone does lack prominent tori (as do many other intermediate specimens) but the site's juvenile mandibular fragment is robust. The loss of the only relevant Spanish remains, from El Castillo and apparently of the very early Aurignacian, renders it impossible to determine their anatomy.

There are now virtually no 'anatomically fully modern' specimens from Europe prior to the Gravettian and contemporary traditions, and even those of the Gravettian are still relatively robust. However, there are numerous Neanderthaloid finds up to the beginning of the Gravettian, around 28 ka BP. In six cases, Neanderthal remains have now been reported in occupation layers containing the tools of early Upper Palaeolithic traditions: from the Châtelperronian of Saint Césaire and Arcy-sur-Cure, from the Aurignacian at Trou de l'Abîme, the Olschewian in Vindija Cave, the Streletsian of Sungir' and from the Jankovichian found in Máriaremete Upper Cave (cf. Bednarik 2008a).

We have therefore Neanderthals and post-Neanderthals from the period 45 to 28 ka ago, and we have less robust remains from the subsequent millennia. This suggests, firstly, that all early Upper Palaeolithic traditions were by Neanderthaloids; and secondly, that full anatomical modernity did not begin at any specific time, but appeared gradually. The trend towards gracility first becomes evident about 50 ka ago, and it continues still in the Holocene, right up to the present time. Humans 10 ka ago were generally 10% more robust than they are today; 20 ka ago they were 20% more robust, and so forth. The fundamental question to be asked is: why did humans of the second half of the Late Pleistocene develop into inferior forms not only in Europe, but in all four continents occupied at the time? Why was the trend towards increasing robusticity, such as we see in the australopithecines and much later again in robust *Homo sapiens*, suddenly reversed and led to rapidly increasing gracility? Why did our brain size, skeletal robustness and muscle power all decrease so rapidly and so uniformly around the globe, resulting in selection for so many neonate features that they are not even attempted to be listed here (but see Bednarik 2008b)? And why did natural selection tolerate the establishment of around 8000 deleterious genetic conditions in 'modern humans', which range from Mendelian disorders to mental illnesses and neurodegenerative conditions? These are the key questions to answer in human evolution, and yet they have not attracted any attention whatsoever, except by this author (e.g. Bednarik 2008a, 2008b, 2011, 2014 *et passim*).

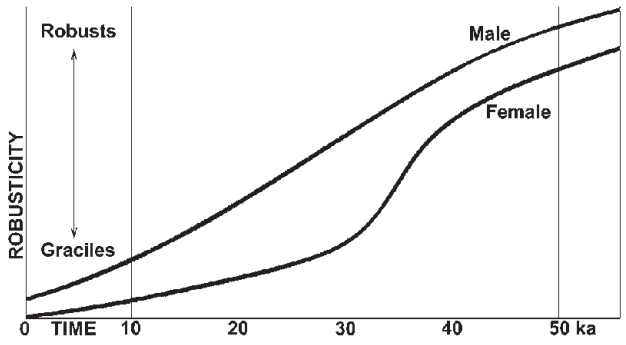

Figure 4. *Schematic depiction of male and female relative cranial gracility in Europe through time, showing that the decline in robusticity is gradual in males, but accelerated in females between 40 and 30 ka, who thus led in the gracilisation of humans.*

The hypothesis replacing the 'replacement hypothesis', the 'domestication hypothesis' (Bednarik 2008a, 2008b), introduces a new, most intriguing possibility. Based on the observation that rapid gracilisation is a universal fea-ture of the last part of the Late Pleistocene (Fig. 4), and that it marks effectively a foetilisation of hominins, this hypothesis attributes the neoteny of 'anatomically modern humans' to culturally moderated breeding patterns. The resulting loss of robusticity involved several reductions in evolutionary fitness: the size of the brain decreased at a time when demands made of it increased; skull thickness and cranial robustness were greatly reduced, as was general skeletal strength and physical power. These deleterious effects occurred more or less concurrently in all regions occupied by humans at the time, including Australia, and cannot be explained in Darwinian terms. They are, however, explainable by Mendel's (1866) theory of inheritance.

Mating preferences and their genetic results in respect of personality and anatomical traits (Laland 1994), which could become cultural selection variables, can be modelled by methods of the gene-culture coevolutionary model (Cavalli-Sforza and Feldman 1973; Feldman and Cavalli-Sforza 1989; Aoki and Feldman 1991; Durham 1991). There is no evidence that mating choice in non-human animals is governed by such factors as body ratios, facial features, skin tone, hair, symmetry or youth, yet in present humans they are so deeply entrenched they may be hard-wired. Facial symmetry, seen to imply high immunocompetence (Grammer and Thornhill 1994; Shackelford and Larsen 1997), is a preferred variable, and in females neotenous facial and other features are so also (Jones 1995, 1996). Clearly, these cultural preferences of 'attractiveness' had to be introduced at some stage, and if the palaeoanthropological record is any indication, this may have begun during the early phase of the Upper Palaeolithic in much of the world (Bednarik 2008b).

Neoteny may seem to be deleterious to a species, but it also involves certain benefits. Most importantly, it effects the retention of plasticity or 'morphological evolvability' (de Beer 1930: 93). Adaptively useful novelties become available as maturation genes are freed by pedomorphosis. In a species whose behaviour is increasingly determined by cultural factors, corresponding plasticity of cultural behaviour may foster the curiosity, inventiveness and inquisitiveness of youth. It seems entirely possible that these traits, so important to the most recent cognitive developments of humans, may account for the rise of iconographic (i.e. 'juvenile') graphic art forms, documented at Chauvet and elsewhere (Bednarik and Sreenathan 2012). There is no evidence that most Upper Palaeolithic cave art of western Europe is the work of adults, but there is ample evidence that it is the work of juveniles (Bednarik 2008c). An increasing 'preference' for iconic art towards the end of the Pleistocene had significant effects on the proliferation of new symbol systems; it made possible the revision of immutable constructs of reality expressed in the more regimented graphic semiotics of earlier societies. Hence it is possible to perceive the change to the more permutable figurative system as reflecting the trend towards other neotenous attributes that marks the final Pleistocene (Sreenathan et al. 2008).

Conclusions

Archaeology's concepts of Pleistocene palaeoart are marred by a series of misconceptions. For instance it is widely believed that such 'art' consists largely of semi-naturalistic megafaunal images in caves. In fact there are only a few thousand such motifs known, whilst over 99% of Pleistocene art consists of aniconic or non-figurative patterns. Indeed, there are almost no figurative graphic images available from the Pleistocene outside of western Europe, and this massive remaining corpus has received almost no sustained attention by comparison. Many scholars assume that most surviving rock art of the Ice Ages occurs in the Franco-Cantabrian region of Europe; yet this phenomenon is in fact far more common in Australia — possibly up to a hundred times more common (Bednarik 2010). It is also widely unknown that there is much more surviving 'Middle Palaeolithic' rock art in the world than 'Upper Palaeolithic'; and most commentators believe such traditions began with the latter period, commencing with the Aurignacian. The mental construct of most commentators, of 'art' beginning with animal figures, has not only prompted the historical neglect of most of the world's Pleistocene art; it has even led to the pronouncement of many sites of such zoomorphs as Palaeolithic in the absence of any evidence — and even when these bodies of rock art were only a few centuries old (Bednarik 2016). When in addition to these issues we also consider that the notion of a 'Palaeolithic style' must be mistaken, because included in it is the rock art of many dozens of sites that are clearly not Palaeolithic — or at least unlikely to be so — we begin to appreciate that this entire topic is in dire need of fundamental review. We also realise that these fantasies account for the severe distortions defining what is believed about Pleistocene palaeoart.

Precisely the same gradual change seen in human skeletal characteristics is also found in the complex mosaic of the European tool traditions from 45 ka BP to the end of the Pleistocene. As the house of cards built by the African Eve advocates is collapsing, they have to prepare themselves for the possibility that not only the Aurignacian proper, but also the Bohunician, Szeletian, Jankovician, Olschewian (which this author considers relevant to Chauvet), the Bachokirian, Spitzinian, Streletskian, Gorodtsovian, Krems-Dufour variant, Uluzzian, Uluzzo-Aurignacian, Proto-Aurignacian, Jerzmanovician, Lincombian and the Altmühlian traditions might all relate to humans other than what they perceive to be 'moderns'. After pointing out many years ago (Bednarik 1995b: 627) that we have no evidence whatsoever that the Early Aurignacian is the work of 'moderns', the author can now add that the ethnicity of the makers of *any stone tool tradition of the entire first half of the so-called Upper Palaeolithic* — including the entire Aurignacian — appears to be that of robust, Neanderthal-like humans, or of their direct descendants.

Chauvet Cave contains not only the world's most stunning cave art; it also features thousands of human and animal tracks on its floor. Some of these are exceedingly well preserved, and upon examining them closely they appear to be of 'Neanderthals' rather than 'anatomically modern humans' (Bednarik 2007; cf. Clottes 2001: Fig. 28). Naturally the presence of 'Neanderthal' footprints does not prove that the rock art was also made by these people, but surely the possibility needs to be considered. The traditional response, that the Neanderthals could have never been sufficiently advanced to produce such masterworks, is simply no lon-ger adequate now that the Aurignacian appears to be a Neanderthal tradition. It is merely a repeat of the archaeological mantra that the cave art of Altamira in Spain cannot possibly be attributed to Stone Age people.

Moreover, there is a growing corpus of evidence that 'Neanderthals' or other robust *Homo sapiens* produced rock art. For instance, there have been unconfirmed reports of 'Neanderthal' petroglyphs in Zarzamora Cave (Segovia, Spain) and there are those found recently in Gorham's Cave, Gibraltar. At the latter site, a design of eight deeply engraved

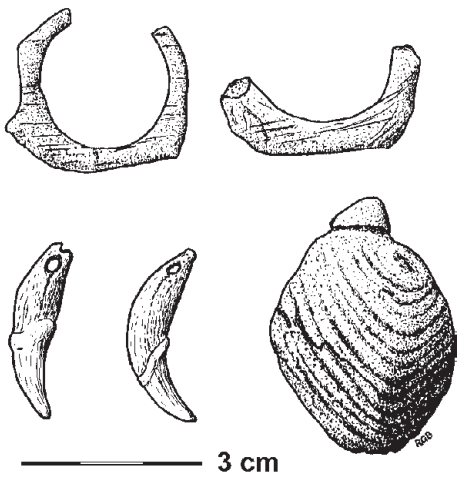

Figure 5. *Some of the jewellery from the Châtelperronian of Grotte du Renne, Arcy-sur-Cure, Yonne, France, made by 'Neanderthals'.*

lines on the lime-dolostone bedrock floor predates sediment layer IV, dated to c. 39 cal. ka BP, when the cave was occupied by 'Neanderthals' (Rodríguez-Vidal et al. 2014). That the many portable palaeoart objects from the Châtelperronian of Grotte du Renne at Arcy-sur-Cure, south of Paris, were used by 'Neanderthals' seems generally accepted now, although some archaeologists still have difficulties admitting that they also *made* them (Fig. 5). These finds include not only perforated jewellery items, but also grooved pendants (Leroi-Gourhan and Leroi-Gourhan 1964), which as Marshack (1991) points out are not typical of the Aurignacian. That site also yielded no less than 18 kg of black and red pigment fragments, many of them with use wear, from its Châtelperronian levels (Salomon 2009).

In addition there is a growing body of evidence of rock art that is likely to be of the EUP, dating roughly from between 40 and 30 ka ago and probably produced by Neanderthaloid people. This primarily Spanish corpus includes some rock art in the Cave of Nerja in Málaga, with dates of up to 35 320 ± 360 BP (Romero et al. 2012), from where Sanchidrián has reported apparent torch soot next to ichthyform paintings that dates from between 43 500 and 42 300 BP (Collado Giraldo 2015). At Tito Bustillo in Asturias, pictograms have been attributed to the Aurignacian (Balbín Behrmann et al. 2003) and may be as early as 37 700 BP (Pike et al. 2012: 1412). A red triangular motif in the cave of Altamira, Cantabria, has been suggested to be in excess of 36 160 ± 610 years old (Pike et al. 2012: 1410). Another Cantabrian cave, El Castillo, which has yielded some of the earliest Aurignacian evidence in western Europe, has produced minimum dates of up to 41 400 ± 570 BP from its rock art (Pike et al. 2012). This proposal would place the sampled red 'disc' motifs before the earliest Aurignacian occupation evidence at the site (Hedges et al. 1994). Although the use of uranium-thorium dates from very thin carbonate speleothem needs to be reviewed (Clottes 2012; Bednarik 2012), the growing number of proposals of rock art in south-western Europe having been created by robust *Homo sapiens* is entirely reasonable, considering the much earlier rock art production reported in other continents (Bednarik 2011).

Several more Spanish pictogram sites have been suggested to include motifs of the EUP that may be attributable to Neanderthaloids, including Pondra Cave in Cantabria (González Sainz and San Miguel 2001: 116–118) and five more sites in Asturias. These are the cave of La Peña (Fortea Pérez 2007), Abrigo de la Viña (Fortea Pérez 1999), El Conde Cave (Fernández Rey et al. 2005), yellow bovid figures and charcoal dots in Peña de Candama, and possibly the cave of El Sidrón (Fortea Pérez 2007). Finally, Maltravieso Cave at Cáceres, Extremadura, contains on panel 3 in its 'hall of paintings' some painted motifs that appear to be more than 37 ka old (Collado Giraldo 2015: 200).

European Pleistocene archaeologists need to adjust to this new scenario, and unless they can demonstrate that Chauvet was made by what they call 'moderns' or 'Cro-Magnons', they are obliged to equally consider the possibility that this art is the work either of 'Neanderthals' or of their descendants who experienced introgression rather than 'replacement'. Their breeding patterns were influenced by cultural selection: selection in favour of neonate features. On the basis of the present archaeological and palaeoanthropological evidence, the latter scenario is the far more likely: we have Neanderthal remains from the time Chauvet cave art was created, and we have no 'moderns'. Science works by falsification, and the proposition to be tested now is that the Chauvet art was created *not by 'moderns'*. The proposition of its Aurignacian age, too, can be tested — but not by facile and circular stylistic argument in which the stylistic diagnostics are not even properly defined.

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Continuing the wild goose chase: a response to d'Errico and Stringer

Robert G. Bednarik

In 2011 d'Errico and Stringer, former advocates of the 'replacement hypothesis' (aka 'African Eve model'), published a fascinating paper in the *Philosophical Transactions of the Royal Society* entitled 'Evolution, revolution or saltation scenario for the emergence of modern cultures?'. It deserves a detailed response because it addresses such an important topic. In this paper they attempted to "evaluate the scenarios proposed to account for the origin of modern cultures in the light of the earliest archaeological evidence for crucial cultural innovations, including symbolically mediated behaviours, in Africa, Asia and Europe". In this paper d'Errico and Stringer (henceforth 'the authors') signal a significant retreat from the two-species model, finally admitting that recent findings refute their own "long-standing model that proposes all living humans trace their ancestry exclusively back to a small African population". This tends to give the impression that this African Eve model had to be replaced because of new evidence, when in fact there was never any palaeoanthropological, archaeological or genetic evidence in its favour. It had simply been a hypothesis that began with the 1970s hoax of Protsch (1973, 1975), was adopted by Bräuer (1984) and popularised in the late 1980s (Cann et al. 1987; Stringer & Andrews 1988; Mellars & Stringer 1989). Despite its manifest lack of credibility it was adopted so widely that it became the de facto dogma of the discipline, opposed by very few commentators during the 1990s (Bednarik 1991, 1992, 1995 *et passim*; Brace 1993, 1999; Wolpoff & Caspari 1996; Wolpoff 1999; Eckhart 2000).

Nevertheless, from the perspective of those few, the authors' statement "[o]nce firmly separating us from the remainder of present and past hominids, genetic and behavioural boundaries are becoming less and less well defined" has no justification. What are these genetic and behavioural boundaries? They only existed for those who had been misled by Protsch's false model. For those embracing Weidenreich's model of human evolution, the boundaries introduced by the replacement hypothesis had never existed and robust and gracile forms of *Homo sapiens* had never been separate species. This is the defining error of the 'African Eve' theory, and while its advocates now concede that they were wrong, the model they now seek to replace it with is just as wrong. They refer to a significant interbreeding between robust and gracile H. sapiens, still maintaining that there were two separate populations, when the more parsimonious explanation of the genetic evidence is that robust populations were subjected to a process of gracilisation (or, perhaps more correctly, neotenisation) that is still underway today. Of course there were "intermediate" specimens and even populations, especially from c. 40 ka to 25 ka ago, as one would expect from a period of rapid somatic changes to the human species.

Those changes occurred in all human populations of the Late Pleistocene world, all during the same time interval, and without being connected to the transition from Mode 3 ('Middle Palaeolithic', MP) to Mode 4 ('Upper Palaeolithic', UP) technocomplexes. For instance in Europe, all early UP traditions (such as the Aurignacian, Châtelperronian, Uluzzian, Proto-Aurignacian, Olschewian, Bachokirian, Bohunician, Streletsian, Gorodtsovian, Brynzenian, Spitzinian, Telmanian, Szeletian, Eastern Szeletian, Kostenkian, Jankovichian, Altmühlian, Lincombian or Jerzmanovician) seem to be attributable to so-called Neanderthals or their 'intermediate' direct descendants (Bednarik 2008a). In the Levant, both MP and UP technologies occur with robust, intermediate and gracile groups. 'Intermediate' Late Pleistocene specimens occur literally in their hundreds across Eurasia, from Portugal to China (e.g. at Lagar Velho, Crô-Magnon, Miesslingtal, Mladeč, Pavlov, Předmostí, Podbaba, Dolní Věstonice, Cioclovina, Bacho Kiro, Peştera cu Oase, Peştera Muierii, Crete, Starosel'e, Rozhok, Akhshtyr', Romankovo, Samara,

Sungir', Podkumok, Khvalynsk, Skhodnya, Denisova, Balangoda, Tam Pa Ling, Jinniushan, Red Deer (Maludong), Longlin and Tianyuan Caves; and WLH-50 from Willandra Lakes or the two very different specimens from Narmada also clash severely with the simplistic African Eve notion), and yet the promoters of the replacement hypothesis ignored their existence. Not surprisingly, they are now obliged to withdraw their model, but they are replacing it with yet another frivolous construct, again burdening the discipline unnecessarily. The notion of two populations, one robust (the 'primitive Neanderthals'), the other gracile ('anatomically modern humans', a nonsensical concept; Latour 1993; Tobias 1996; Bednarik 2011a), who 'interbred' on occasion is another falsity. Such two peoples interbred no more than great-grandchildren interbreed with their great-grandparents. One group developed gradually into the other, through a process of introgressive hybridisation (Anderson 1949), allele drift based on generational mating site distance (Harpending et al. 1998), and genetic drift (Bednarik 2011b) through episodic genetic isolation. That is precisely why, during the early UP traditions, there are so many 'intermediate' hominin specimens: because they were intermediate between the earlier more robust and the later more gracile people.

What perhaps facilitated the establishment of the replacement model is that the change, while clearly being gradual, nevertheless occurred in a geological instant, in the course of a few tens of millennia. This, perhaps more than any other factor, generated the ready acceptance of this hypothesis. The observation that during the 'transition' (in reality, every evolutionary process is a series of transitions) robust and gracile physiologies co-occurred with intermediate morphologies, and the tendency of scholars of placing these into pigeonholes of species contributed to the erroneous model. Palaeoanthropologists have now created many dozens of hominin 'species', many of which are represented by single specimens. At the rate of 'discovering' new species we will one day have as many as we once had grizzly bear species (some 300, when in fact the grizzly is not even a separate species of *Ursus arctos*). *Homo sapiens neanderthalensis* and *Homo sapiens sapiens* are obviously of one species, as it had been assumed before the African Eve interlude and as the authors have finally conceded. Where the supporters of this model probably became lost is that they assumed that only one process could logically account for the relatively swift neotenisation in Final Pleistocene hominins. This is similar to their mistaken belief that the geographical movement of genes can only mean mass movement of people.

The fact that a much better, much more robust and much more elegant solution explaining the rapid gracilisation beginning about 40 ka ago has been available for years (Bednarik 2008a, 2008b, 2011a) is simply ignored by the authors. They are not concerned with finding a rational explanation for the massive changes evident, but are captivated by rationalising why their African Eve notion was a falsity. Instead of engaging in a constructive dialogue they explain why recent genetic evidence has refuted ideas that had no justification in the first place; these ideas were always false, and that had always been appreciated by some. The authors still invoke "cultural modernity", citing clichés such as "altruism, enhanced memory, complex language", seemingly unaware that altruism exists in insects, recursive language is demanded by maritime colonization at least a million years ago (Bednarik 1999, 2003, 2014a *et passim*), and they fail to explain what they mean with the third variable or how they propose to demonstrate it. Their list of the indications of human modernity is so naïve that one wonders why it was assembled:

> Exploitation of coastal environments; greater complexity of food gathering procedures, such as the use of nets, traps, fishing gear; complex use of fire for cooking, food conservation; ecosystem management; producing and hafting stone tools; invention of specialized tool-kits to adapt to extreme environments; higher population densities approaching those of modern hunter–gatherers; complex tools, the styles of which may change rapidly through time and space; structures such as huts that are organized for different activities; long-distance transport of valued materials; formal artefacts shaped from bone, ivory, antler, shell; musical traditions; sea crossing and navigation technology; personal ornamentation in the form of body painting and personal ornaments; art, including abstract and figurative representations; evidence for ceremonies or rituals; complex treatment of the dead (d'Errico and Stringer 2011: 1061).

Since we know absolutely nothing about the exploitation of coastal environments or the food gathering of coastal people of the entire Pleistocene, because the successive sea-level fluctuations have destroyed all evidence, the first few items are simply irrelevant. Besides, a great many species have learned to exploit coastal environments; there is nothing modern about it. Complex use of fire has been demonstrated as far back as 1.7 million years (Beaumont 2011). Credible evidence for food conservation and ecosystem management is unavailable from any Pleistocene context. The hafting of composite tools predates the UP greatly, and to suggest that we know something about population densities from the entire Pleistocene is simply false. We have evidence of huts from Lower Palaeolithic sites in France, Germany, Africa and India, in one case of stone foundations of an entire Acheulian village with a cemetery and latrine (Ziegert 2010). Artefacts of bone, ivory, antler and shell have been reported from hundreds of sites of the MP and LP. Evidence of musical instruments of the MP has been reported (e.g. Huyge 1990; Turk et al. 1995; Turk & Dimkaroski 2011) but one of the authors rejects it (d'Errico et al. 1998). Evidence for early sea crossings has been tendered since the 1960s, has been subjected to considerable attention since then (e.g. Bednarik 1999, 2003, 2014a, and dozens of other publications) and extends at least one million years into the past. Personal ornaments such as beads as well as what the authors define as "art" have been found from the LP and throughout the MP. And the 80 graves of the 400-ka-old cemetery excavated at Budrinna may simply be the response of sedentary to semi-sedentary groups to the needs of disposing of cadavers so as not to attract scavengers. In short, if this list of variables is all we can come up with in defining behavioural modernity it extends into the Early Pleistocene, and it beggars the question why the subject is raised in the context of the appearance of supposed anatomical modernity. Perhaps the authors could respond to Bednarik (2012) in order to begin a more mature

discussion of the topic.

The authors ask the very legitimate question, what is the earliest evidence for symbolic behaviour in the archaeological record. Their answer, however, suggests that they are so inadequately informed about the topic that their findings are inconsequential. To begin with, they provide no proof that any of the material finds they list are necessarily symbolic: for instance why would human interment or pigment use necessarily demonstrate symbolism? They recite a list of beads and portable engravings that implies that these are all the relevant finds they are aware of. Considering that a catalogue of Pleistocene palaeoart finds of all continents other than Antarctica lists thousands of motifs or objects of Modes 1, 2 and 3 industries (Bednarik 2013a, b, 2014b, c, d), it would have been best to omit their severely limited effort.

The notion that geographical movement of genes (or memes) can only mean mass movement of people is negated by introgression and the concept of cumulative mating site distances. These apply not only in the animal world, to species that have adapted to all environments from the Arctic to the tropics; they also determine hominin genomes. The presence of robust groups within the Arctic Circle (Norrman 1997; Pavlov et al. 2001; Schulz 2002) implies that all reasonably habitable regions of Eurasia were fully occupied by them about 130 ka ago; therefore the fantasies of mass migrations into unoccupied areas never had any currency. But the greatest failure of the countless replacement advocates, including these authors, has been their faith in the belief of the sharp separation of robust and gracile species, preventing them from seeing the most rational explanation for the rapid neotenisation that led to what they regard as modern humans. Instead of asking the important questions, they focus on trying to salvage as much as possible of their refuted hypothesis.

Here are the questions they really need to ask if they are to progress past their simplistic model: why has natural selection allowed the rise of many thousands of deleterious genetic conditions, ranging from neurogenerative to Mendelian disorders, mental illnesses and many more, since the appearance of gracile traits? Why has the presumed main indicator of hominin progress for millions of years, encephalisation, suddenly been reversed in the Pleistocene's last phase to allow a rate of brain atrophy 37 times the previous rate of brain size increase? Why have neotenous traits been selected consistently that provide no benefit or are clearly disadvantageous? Why has significant loss of physical strength and skeletal robusticity, especially of the cranium, been selected for? Why have characteristics of domestication, such as smaller brain size, shortened face, abolition of oestrus, general gracilisation and neotenisation, been selected for, when none of them has any Darwinian advantage? How did such conditions as exclusive homosexuality arise in the genome?

Other questions that need to be asked by these authors, and by many others, are these: if it is true that the direction of human development is established largely by cultural determinants today (as appears to be the case), and if this was not the case in the distant past (ditto), at what time would the dysteleological process of evolution have been replaced by the teleology of cultural development? This would be far more important than the sterile question of 'modern' origins. If it is true that 'modern humans' are the only species on the planet that has, in its selection of mating partners, distinctive preferences of age, 'attractiveness', facial symmetry, specific body proportions, gracility of bones; or hair, skin or eye colour, is it not necessary to consider at what time and why such exceptionally pronounced preferences were introduced? We know that in every extant human society males express a distinctive preference for females with marked neotenous facial features (large eyes, small nose and lower face, high forehead etc.). Since these mating preferences are among the very few substantive differences between us and other animals, and since they are absent in apes we need to assume that they were introduced at some point in time. At what time was that? These are legitimate questions if we are to consider the origins of 'human modernity' outside the simplistic and entirely sterile framework the authors have provided in the past and still pursue today. It is sterile because neither their replacement hypothesis nor their modified replacement hypothesis explains anything of importance. The domestication hypothesis explains in one sweep all of these aspects, and many more, and yet these authors make no attempt to even consider it — being interested in nothing other than to explain why they promoted the redundant idea that robust and gracile humans could not interbreed.

Planck (1950: 33–34) suggested that scientific progress is only possible when "its opponents eventually die, and a new generation grows up" that is familiar with new models. Will we have to wait decades again, as in the cases of the rejected ideas of Boucher de Perthes, Fuhlrott, de Sautuola, Dubois or Dart, before the failed hypothesis of modern human origins is laid to rest? I ask d'Errico and Stringer to respond to the questions posed in the preceding paragraph, and to tell us why they failed to consider a hypothesis that clarifies these and many other questions. A hypothesis that explains nothing of consequence can take up a great deal of space on paper, but in the end it is worthless to science, and propping it up beyond its use-by date is wasteful and counterproductive. The final issue is this: these authors are among the many that have succeeded in sending the discipline on a wild goose chase lasting a few decades. Do they want to be remembered for that by future generations?

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