The World

Lord Avebury's Virtual Journey through Time

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After the Ice: A Global Human History, 20,000–5000 BC (2006) Steven Mithen. Harvard University Press, Cambridge, Massachusetts. Xiii + 622 pages, 47 color, 4 halftones, and 21 line illustrations; 12 maps. $18.95. ISBN 0-674-01999-7 (paperback).

This is a book of a quite unusual style for a prehistorian writing about past human societies. It has a description of virtual travel throughout the entire world, from Asia, Africa, Europe, Australia, to the Americas by a prominent archaeologist from the early years of the field. It is almost impossible for a single individual to properly review the whole book, and it seems to be more reasonable to concentrate on areas with which I am more familiar, such as parts of Europe and Asia (constituting mostly the territories of Russia and neighbouring East Asian countries), and to some extent North America. For the rest of the world, I would rely on the author's information and interpretations.

The volume under review consists of a preface, 53 chapters with extensive endnotes for each chapter where additional and more specific information is given, a comprehensive bibliography, and numerous illustrations and maps. Chapters are not too long but reasonably detailed, and this is a definite advantage for non-academic readers who may, thus, quickly become acquainted with regions and problems under consideration. Maps give a clear understanding where the sites mentioned by the author are located.

The time frame of this volume is from the height of the last glaciation, about 20,000 BC, to the emergence of first civilizations soon after 5000 BC. Throughout these 15,000 years, humanity experienced general climatic amelioration with some cold spells like the Younger Dryas at ca. 10,800-9600 BC (Mithen 2006:12; hereafter only page numbers are given when this source is considered). The emergence of cities, agriculture, animal domestication, and pottery, and the peopling of the New World occurred in this time frame.

As the readers' guide through time, a figure well-known to archaeologists and anthropologists is chosen: Sir John Lubbock (1834-1913), also known as Lord Avebury (Bahn 2001:43). He was a banker, politician, and scholar in Victorian England. In 1870-1900 he was a member of the British Parliament, and made the first Baron Avebury in 1900 (*Merriam-Webster's Biographical Dictionary* 1995:648; *Debrett's Peerage and Baronetage* 2003:P93); Mithen mistakenly indicated this as happening in 1890. His best known scientific book is *Prehistoric Times* (1865, London: Williams & Norgate; seventh edition, 1913). Lubbock is responsible for the introduction of two important archaeological terms, "Palaeolithic" and "Neolithic" (Darvill 2002:235).

The general approach of the book is to characterize the environment, physical anthropology, artifacts, economy, and spiritual life of the inhabitants of different parts of the world at ca. 20,000-5000 BC, with a focus on the most important innovations which took place in early human societies. By doing so, the author points the readers' attention to the most fascinating features, such as the waterlogged site of Ohalo on the Dead Sea coast with its rich organic remains of the late Upper Palaeolithic (pp. 20-26); the world's oldest walled city of Jericho (pp. 56-61); one of the most important Mesolithic and Neolithic sites in Europe, Lepenski Vir (pp. 159-163); the mysterious Mesolithic burial ground of Oleny Ostrov [Deer Island] in northwestern Russia (pp. 168-171); one of the oldest sites in the Americas, Monte Verde (pp. 229-235); the valley of Oaxaca in Mexico (pp. 274-285); Niah Cave on equatorial Borneo Island (pp. 348-352); the extremely remote Zhokhov Island in the Arctic Ocean inhabited at ca. 6400 BC (pp. 381-383); Mesopotamian sites that existed just before the emergence of Sumer civilization (pp. 420-440); and the fertile oasis of Wadi Kubbaniya in the Sahara Desert (pp. 444-452).

Beyond the "educational" part of the book briefly mentioned above, there is an "academic" aspect which is the subject of this review. Here the author is saying: "I have tried to write a book that makes the evidence from prehistory accessible to a wide readership while maintaining the highest levels of academic scholarship" (p. xi). As mentioned in the epilogue (p. 506), the most current evidence taken into account for the compilation of this volume is dated to around mid-2003. Therefore, Mithen should have taken into account anything published before this time, although that is not always the case, as is demonstrated below.

In order to review this book, one should make a decision about its aim, either "academic" or "educational". In my opinion, Mithen's volume is an academic-based narrative written for a wide scientific community, particularly for undergraduate and graduate college students (especially those who are not directly involved in archaeology and anthropology programmes), and for the general public. In the following paragraphs, an evaluation of the "academic" part of some chapters is given, with the addition of new relevant information which has come to light since 2003.

In the introductory chapters 1-2 and endnotes to them, I find a statement which appears oversimplified. The accelerator mass spectrometry (hereinafter AMS) radiocarbon (hereinafter ^{14}C) dating method is given precedence over 'traditional' liquid scintillation counting (hereinafter LSC). This is the so-called "AMS myth" in which AMS is claimed to be a more advanced and powerful research tool than the 'routine' LSC ^{14}C dating. Why myth? Because the AMS method for the detection of the amount of ^{14}C atoms in a given carbon sample is to a major extent only a *technical* advance. The opinion in the endnotes to chapter 2 that the AMS method allows the "greatest degree of accuracy" and "a much greater degree of precision" (p. 517) in relation to the traditional LSC method is an inaccurate statement. The degrees of precision and accuracy in LSC are generally higher than in AMS, and this is why the LSC method was used for high precision measurements of ^{14}C content in tree rings (e.g., Stuiver 1993:93-104). The greatest advantage of the AMS method is that it requires about 1000 times less carbon than the LSC method, which is especially important in cases of small artifacts (often from museum collections) or tiny fragments of valuable organic matter such as adornments, textiles (the Shroud of Turin is perhaps the best-known example), and human remains. Also, technical details such as the poisonous chemicals required in the LSC method hamper its widespread use due to strict safety regulations in many countries. Indeed, AMS is the fastest growing field in radiocarbon studies, and it is gradually replacing the LSC method (e.g., Jull and Burr 2006).

In chapter 4 Mithen cites Early Natufian communities in the Near East as the cultural complex associated with the oldest domesticated dogs (pp. 34-35). This view is out-of-date, however, because in northern Eurasia the earliest dogs are now known from central and eastern parts of Europe. At the Bonn-Oberkassel site in northern Rhineland (Germany), the direct AMS ^{14}C date of a fully domesticated dog (*Canis familiaris*) is ca. 12,300 BP (Street and Terberger 2004; see original report of ^{14}C date in Hedges et al. 1998). This find is briefly mentioned in Clutton-Brock (1995:10) as dated to ca. 14,000 BP and the paper by Clutton-Brock (1995) is acknowledged by Mithen (p. 519) although the Bonn-Oberkassel case is ignored. An even older direct AMS ^{14}C value of ca. 13,900 BP for dog is known from the Eliseevichi I site in the central Russian Plain (Sablin and Khlopachev 2002; Sablin and Khlopachev 2003). The modelling of modern dogs' DNA back to the final Late Pleistocene, ca. 13,000 BP (corresponding to about 15,000 years ago) suggests that East Asia was the place of first domestication of dog (Savolainen et al. 2002). It contradicts to some extent fossil evidence which seems to be common when DNA data are given precedence over 'traditional' palaeontological data (e.g., Rohland et al. 2005). Therefore, the Early Natufian "cradle" of domesticated dog as accepted by Mithen, is an outmoded hypothesis by modern standards.

In chapter 7, the 'agricultural' trajectory of Neolithisation is described (versus the 'pottery' scenario, see below). The oldest fig trees in the world are mentioned as growing in the village near the town of Jericho (p. 56). The latest information from Levant shows that the earliest cultivation of fig may be dated to ca. 9400 BC (Kislev et al. 2006); but see Denham 2007), which is very close to the time of Jericho's foundation, ca. 9600 BC.

Continuing the survey of the Near East, in chapter 9 Mithen cites the finding of textiles in the town of Beidha to be considered the oldest in the world: "The people of Beidha are dressed elegantly in fabrics made from spun yarn; they are weaving the earliest form of linen, dyed green and turned into tunics and skirts" (p. 74).

This is, in fact, erroneous. The dates of occupation at Beidha are (uncalibrated ^{14}C values) ca. 8940-8550 BP (level 6), ca. 8640 BP (level 5), and ca. 8810-8730 BP (level 4), leaving the date of ca. 9130 BP as an outlier (Kuijt and Bar-Yosef 1994:240). The volume edited by Bar-Yosef and Kra where these dates are published is known to Mithen (see reference to paper by S.K. Kozlowski on page 592). However, he only mentions in the endnotes (p. 523) that charcoal dates from Nahal Hemar are of ca. 8270-8100 BP, and that organic artifacts (knotted net, twined fabric, and cordage) are much older, ca. 8690-8500 BP Mithen ignores the older ^{14}C dates on organic remains like linen, 9120 ± 300 BP (BM-2298) and 8850 ± 90 BP (Pta-3625) (Kuijt and Bar-Yosef 1994:240). His wish to maintain "the highest levels of academic scholarship" (p. xi) is not apparent in this case.

Another example of his reference to an outmoded hypothesis may be found in chapter 13 where the Last Glacial Maximum (hereafter LGM) in northwestern Europe is considered (pp. 111-112). Sites with ^{14}C dates of ca. 20,000-18,000 BP are known in Rhineland (Wiesbaden-Ingstadt), as well as in the Alps (Kastelhöhle-Nord) and its northern piedmonts (Mitterle Klause) (Street and Terberger 1999, 2000; Terberger and Street 2002; see latest data in Verpoorte 2004). Data on all of these sites were published before 2003. Therefore, at the LGM it is clear that people settled not only "southern France and Spain" (p. 111) but also more northern territories of Central Europe.

In chapter 19, the story of the discovery and interpretation of the Mesolithic burial ground at Oleny Ostrov [Oleneostrovski Mogilnik] in northwestern Russia is recounted. I think Mithen is quite sarcastic in saying about the Soviet archaeologist V.I. Ravdonikas that "As he [Ravdonikas] worked in Stalin's Russia and archaeological observations were made to confirm the pattern of social evolution as laid out by Frederick Engels, Ravdonikas had to justify it. … His solution—and the one still favoured by so many archaeologists today when lost for an explanation—was 'ritual' " (p. 169). Perhaps Mithen does not realize that if Ravdonikas had not followed the ideological mainstream of the late 1930s in the Soviet Union, he would probably have continued in his career as chief Gulag archaeologist! It seems to me that Ravdonikas' step was quite clever for that time.

As for the "ritual" explanation of the nature of the Oleny Ostrov burial assemblages, Mithen, himself, accepted this scenario when he was trying to explain the enigma of the Mesolithic occupation of Oronsay Island in the southern Hebrides (offshore Scotland): "I found myself grasping at the archaeologists' last resort: Oronsay must have been preferred for some ideological reason that remains quite unknown to us" (p. 205). The inconsistency is obvious: while Ravdonikas' understanding of the Oleny Ostrov phenomenon is pure ideological, Mithen's Oronsay case seems to be "scientific".

Mithen cites a paper by Price and Jacobs (1990) as a primary source of ^{14}C dates for the Oleny Ostrov cemetery (p. 534). This is incorrect: *the first* ^{14}C dates were released in spring 1989 by Mamonova and Sulerzhitsky (1989:24), and although the article is concerned mainly with the Cis-Baikal region of Siberia, 15 ^{14}C assays run on human bone collagen from Oleny Ostrov's burials give dates between ca. 9910 BP and ca. 6790 BP.

Ignorance of the primary literature denies proper attribution of the priority of study of this unique burial ground, and it is necessary to highlight that Mamonova and Sulerhitsky's (1989) results should be properly acknowledged. Sometimes it is not easy to say who actually was first. In the recent case of direct ^{14}C dating of Chinese Late Pleistocene hominids (Keates et al. 2007; Shang et al. 2007), *the first* direct ^{14}C on a presumably early modern human's femur from Ordos Plateau (Keates et al. 2007) gave recent dates (ca. 250 BP only!) and were to some extent a disappointment; later, an attempt by Shang et al. (2007) was more successful, giving a Late Pleistocene age (ca. 34,400 BP) of the modern human from Tianyuan Cave near Beijing. The Oleny Ostrov case is straightforward, however, with Mamonova and Sulerzhitsky (1989) clearly the first in the ^{14}C dating of it. Once again, Mithen's "academic scholarship" failed to recognize it.

In chapter 25, the issue of the peopling of the New World is considered, with the Monte Verde site in Chile cited as *the earliest* evidence of humans in the Americas, dated to ca. 12,500 BC. Mithen states, "For the need of this history—and those of American history for at least the next decade—acceptance of occupation at 12,500 BC is momentous enough. It means that the Clovis First scenario is dead and buried" (p. 234). However, some comments are necessary. The time difference between the main cultural component of Monte Verde (MV-II) and other early sites in North America is not so great as to claim Monte Verde as the oldest settlement. Carbon fourteen dates on organic remains from component MV-II are from ca. 11,920 BP to ca. 13,570 BP (see George et al. 2005:767), with an average of ca. 12,500 BP (e.g., Bever 2006:597). New mastodon bone ^{14}C dates from component MV-6 are about 12,450 BP (George et al. 2005:770). The variation of organic ^{14}C dates from this single component is about 1650 ^{14}C years, and the most reliable of them vary within 500 ^{14}C years (Dillehay and Pino 1997).

It is still poorly understood by many scholars that the actual accuracy ["real accuracy" *sensu* Krenke and Sulerzhitsky (1992); see also Kuzmin and Orlova (1998:24-25)] of the ^{14}C dating method is not less than several hundred years; it is important to keep in mind that real accuracy is unrelated to the precision of ^{14}C dating measurements. Thus, it is impossible to determine with the help of ^{14}C dates which site is older if the age difference between them (in the case of Palaeolithic or Palaeoindian complexes) is less than about 1000 ^{14}C years. We should bear in mind that the earliest archaeological sites in Alaska are now securely dated to ca. 11,800-11,600 BP, and possibly to ca. 12,360 BP (Bever 2006); and the oldest Clovis sites are dated to ca. 11,600 BP (e.g., Waters and Stafford 2007). Therefore, the difference between the Monte Verde site in South America and the universally accepted earliest sites in North America (i.e., Clovis and Nenana complexes; see Bever 2001; Waters and Stafford 2007) is about 800-900 ^{14}C years and perhaps even less. This is not enough in my opinion to claim that Monte Verde is *the oldest* site in the New World.

The author proposes the 'birthplace' of Clovis complex is in the forests of eastern North America (p. 245), in spite of the fact that the earliest Clovis sites are known in the western United States on the Great Plains: Aubrey (ca. 11,600 BP) in Texas and Blackwater Draw (ca. 11,300-11,100 BP) in New Mexico (e.g., Holliday 2000; Haynes 2002). None of the Clovis sites east of the Mississippi are older than ca. 11,000 BP (Haynes 2002:12-13; Waters and Stafford 2007). Therefore, the attempt to shift the place of Clovis origin toward the east contradicts primary data.

In chapter 27, in the discussion of Clovis hunting of Late Pleistocene megafaunal species, the author touches on the existence of Holocene mammoths at Wrangel Island in the Siberian Arctic (pp. 252-253). These most recent woolly mammoths were originally thought to be dwarfs (Vartanyan et al. 1993; Lister 1993). However, Garutt, Averianov and Vartanyan (1993) challenged this conclusion a few years later when they published a more extended description of the new sub-species, *Mammuthus primigenius vrangeliensis* (Averianov et al. 1995). It turned out that even though the size of Wrangel's mammoths was smaller than the usual *M. primigenius* individuals, it was still not a dwarf but a *small* size mammal ["*Melky*" mammoth in Averianov et al. (1995); "melky" means "small", see Russko-Angliisky Slovar (1998:286)]. Unfortunately, the reference to the "dwarf" Holocene mammoth from Wrangel Island persists in some publications, including Mithen's book. Latest research on possible dwarfing of mammoth populations revealed that small size mammoths are known not only at Wrangel Island in the Holocene, but also in mainland Siberia in the final late Pleistocene (Tikhonov and Vartanyan 2001; Reumer et al. 2002; see details in Tikhonov et al. 2003). Surprisingly, the most recent finds of Holocene woolly mammoths from the Pribilof Islands in the Bering Sea dated to ca. 5700 BP (Yesner et al. 2005) shows that these latest American mammoths were of normal size! Thus, the suggestion of woolly mammoth dwarfing in the Holocene is now definitely out-of-date.

Chapters 39 to 41 are devoted to China, Japan, and Arctic Russia, and they deserve special attention as the reviewer is familiar with the primary data which were used to describe human existence in East Asia and northeastern Siberia. In the beginning of this section, "Greater Australia and East Asia", the map with archaeological sites, glaciers' margins, and coastlines (pp. 302-303) shows the vast continental ice sheet existed at ca. 20,000 BC (i.e., 17,000 BP). This is another outmoded theory dated to the 1960s and 1970s (e.g., Arkhipov 1998; Grosswald and Hughes 2002); latest data indicate the absence of large-scale ice sheets in Siberia at the LGM (e.g., Mangerud et al. 1999, 2002; see summary in Svendsen et al. 2004).

In chapter 39, "Down the Yangtze" (pp. 359-369), the origin of rice farming and pottery in China is the main topic. The date of the earliest Chinese pottery as presented by the author seems to be in conflict with existing information. For example, he says that "This slab technique had first been used at Diaotonghuan at around 10,000 BC to produce the oldest known pottery in China" (p. 366). In the endnotes (p. 557), it is added that "Pottery of a similar date has been found elsewhere in China, notably in Yuchan Cave on the southern edge of Hunan Province (Lu, 1999)." The issue of age of the earliest Chinese pottery is quite complex (see review in Kuzmin 2006:364-366); some opinions indicate that it may be dated to ca. 15,200 BP (equal to ca. 16,570 BC; see Reimer et al. 2004:1049). Moreover, the 'chronometric hygiene' of ^{14}C records from the earliest pottery sites in China (Kuzmin 2006) allows for the acceptance of the first pottery production at Miaoyan and Yuchanyan [Yuchan Cave] sites at ca. 13,700-13,300 BP (about 14,500-13,800 BC; Reimer et al. 2004:1050-1051).

The Diaotonghuan Cave case is the central one for determination of the timing of pottery manufacture and rice farming in China (pp. 363-364). However, chronometric data for this site are very scanty. Wu and Zhao (2003:18-19) provide only one ^{14}C value from stratum D, ca. 15,090 BP. The rest of the cultural strata with pottery, including ones with rice phytoliths, are still not ^{14}C-dated, to the best of my knowledge. The age estimates for the Diaotonghuan Cave sequence are quite 'young', ca. 11,000-8000 BP for zones (or strata in Wu and Zhao 2003) D and E (the latter is below zone D; see review in Kuzmin 2006:365). Zhao (1998) assumed that the age of the earliest domestic rice phytoliths in stratum E is ca. 10,000-9000 BP. Therefore, the age of Diaotonghuan Cave cultural layers, assumed to be ca. 12,000-2000 BC (p. 363), is quite speculative, and more research is needed to get solid data.

The age of rice domestication is one of the hottest topics in modern East Asian archaeology. Recently, Jiang and Liu (2006) found evidence of rice cultivation in southern China at ca. 8900 BC (ca. 9600 BP). Fuller et al. (2007), however, challenged the view that this was completely domesticated rice, and proposed an alternative model in which rice of full domestication occurred only in the fifth millennium BC (ca. 6000 BP). The latest data came from Liu et al. (2007) in which they disagree with Fuller et al. (2007) and offer instead the date of ca. 9000 cal BP (about 8000 BP or 7050 BC) for the initiation of rice cultivation which later led to domestication. Therefore, we can tentatively accept the age of the earliest rice cultivation in China as ca. 7000 BC—much later than the time of pottery emergence in East Asia (see Kuzmin 2006). The assumption that pottery and rice in China appeared at almost the same time—"The near simultaneous invention of pottery and the first cultivation of rice is unlikely to have been coincidental—the vessels were most likely used to steam and boil the grain" (p. 366)—is not supported by primary evidences.

It is now clear that in East Asia pottery preceded agriculture, as opposed to the Near East where ceramics were invented *after* the emergence of plant cultivation. The author acknowledges different trajectories of Neolithisation: "In Japan and Sahara the invention of pottery preceded the start of farming, whereas it occurred simultaneously with the origin of rice farming in China; its invention in western Asia came about long after farming towns had begun to flourish" (p. 505). However, as it is shown here, the beginning of rice farming and pottery emergence in China are unrelated, and the East Asian 'way' of Neolithisation (first pottery, and only afterwards farming) is typical for China, Japan, and neighbouring regions.

The last issue in this chapter is the timing of the separation of Japanese Islands from the mainland after the LGM, when water levels rose due to melting of continental ice sheets. In the end, it is stated that "A detailed chronology for sea-level rise around the coast of Japan and the precise date for when its connection with the mainland was breached are currently unavailable" (p. 558). Here Mithen is quite mistaken because such data have existed for a long time (e.g., Naruse 1981; Korotkii 1985;

Oba et al. 1991; see brief review in Nunn 1999:173-185), and in Japan even the atlas of sea-level changes was published a while ago (Ota et al. 1987)! In one of the most recent summaries (Park et al. 2000) it is shown that the western part of Tsushima (Korea) Strait remained open even at the height of the LGM, ca. 20,000–18,000 BP; I also assumed this when analyzing the bathymetric maps of the Sea of Japan (Kuzmin 1997). Another statement—"This issue [sea-level changes] is complicated by the tectonic activity around the coasts of Japan that has also had a major influence on changing sea levels and interpretation of sedimentary evidence" (p. 558)—is also out-of-date. Detailed investigation of coastal tectonics in Japan has been undertaken since the 1950s, and the results produced were readily summarized (e.g., Berryman et al. 1992; Ota and Yamaguchi 2004). It is clear that tectonic movements during the last 85,000 years or so did not influence severely the coastlines of major straits separating Japan from mainland (e.g., Oba et al. 1991).

In chapter 40, "With the Jomon" (pp. 370-380), various aspects of the earliest pottery cultural complex of Japan are described. Once again, the old *cliché* appears on page 372: "Jomon pottery is, in fact, the earliest pottery in the world." Data on Late Glacial pottery (ca. 13,300–10,300 BP) from the Amur River basin, located in the Russian Far East neighbouring Japan and China, were first published in 1997 and in more detail in 2000-2001 (Kuzmin and Jull 1997; Kuzmin et al. 1997; Kuzmin and Orlova 2000; Kuzmin and Keally 2001; see the updates in Kuzmin 2006; Nesterov et al. 2006), and they are now finally accepted by leading Japanese scholars (e.g., Kobayashi 2004:20, 190). The earliest Jomon pottery is now dated to at least 16,000 BC (Nakamura et al. 2001; Kuzmin 2006; Taniguchi 2006) compared with ca. 13,000 BC (p. 372). In fact, the earliest Chinese pottery could be even older than Jomon pottery, but more data is required for confirmation (see Keally et al. 2003; Kuzmin 2006).

As for the possible connection between vegetation changes and the emergence of pottery in Japanese Islands, it is mentioned that C.M. Aikens suggested the "simultaneous spread of broad-leaved woodland and pottery into the northern islands of Japan, both appearing on the northernmost island of Hokkaido at around 7000 BC" (p. 372). However, this is now challenged by new data about much older pottery from Hokkaido dated to ca. 14,400–14,100 BC (Yamahara 2006). It seems that pottery appeared in Hokkaido when environmental conditions were characterized by the dominance of boreal forests with minor (if any) admixture of broad-leaved elements (e.g., Tsukada 1986). The correlation between a sharp increase in pottery quantities at the Jomon sites and the spread of broad-leaved taxa such as oak and chestnut in Japan at ca. 10,000 BP (i.e., around 11,500 BC) is now suggested (Taniguchi 2006).

One of the central points of this chapter is the possible function of Jomon pottery (pp. 372-373). Mithen argues against a utilitarian usage by showing that when pottery appeared in the northern part of Honshu Island (region of Tohoku) at ca. 14,500 BC at the Odai Yamamoto 1 site (on page 373 it is printed as "Odaiyamamoto"), the environment was "no more than a sparse covering of pine and birch" (p. 373). Some comments are necessary. New data allows for establishing the age of pottery at the Odai Yamamoto 1 site at ca. 16,000 BC (Nakamura et al. 2001). The environmental conditions at that time were not as severe as suggested. The northern part of Tohoku in the Late Glacial was covered with conifer forests although some broad-leaved taxa (beech) also began to appear (Tsukada 1986:27-34). The statement, "the theory that Japanese pottery was invented to store and cook acorns and other produce of broad-leaved woodlands cannot be correct" (p. 373) is to some extent incorrect. Even though there were not many nut resources around the Odai Yamamoto 1 site at ca. 16,000 BC, the stable isotope data show clearly that carbonized adhesions (a burnt substance covers the inner and outer parts of potsherds and forms lines indicating the past water level in the pot) represent terrestrial matter which is most probably food remains (Nakamura et al. 2001:1137). Therefore, the utilitarian function of the earliest Japanese pottery for storing and cooking food is very possible, despite the fact that acorns and other nuts were hardly available to the site's inhabitants.

As for the late spread of rice agriculture to the northern Honshu and delayed arrival of the Yayoi complex which follows Jomon in the prehistory of Japan (p. 380), Mithen overlooks information on rice cultivation in later Jomon phases in the Tohoku region. D'Andrea et al. (1995) provided evidence of prehistoric cultivation of rice in northeastern Honshu at ca. 2800–2500 BP, using direct AMS ^{14}C dates of rice caryopses, which corresponds to calendar ages of ca. 1040-700 BC (median values of calibrated ages with ±sigma; Calib Rev. 5.0.1 software, available at http://www.radiocarbon.org). It is evident that in Late Jomon people of Tohoku already practiced rice agriculture, i.e. before the arrival of Yayoi populations from the south. Recently, it has been suggested that the boundary between Jomon and Yayoi periods should be shifted from the traditionally accepted ca. 300 BC to ca. 750–400 BC (Imamura 2003). However, even though this is correct, the rice from the Kazahari site in Tohoku (D'Andrea et al. 1995) precedes the Yayoi of Japan.

In chapter 41, "Summer in the Arctic", one of the major topics is the Dyuktai cultural complex of the Upper Palaeolithic in central Yakutia, northeastern Siberia (pp. 382-385). Mithen traces the origin of this culture to the Transbaikal region south of Yakutia where the Studenoe 2 site with microblade tools and wedge-shaped cores has been excavated. This 'Transbakailan' connection of the Dyuktai culture looks strange because the Studenoe 2 investigators (Konstantinov 1994; Goebel et al. 2000) do not derive the Dyuktai complex from the late Upper Palaeolithic assemblages of Transbaikal. The ^{14}C dating programme of Studenoe 2, which was begun by Konstantinov (1994) and Goebel et al. (2000), has been continued; new results generally confirm the age of the site around 17,900–17,200 BP (Buvit et al. 2003, 2004) and also show that the particular component 4/5, with a total of six hearths, existed until ca. 14,500 BP (Kuzmin et al. 2004).

Generally speaking, the origin of Dyuktai culture is now rarely discussed, and most of the summaries on the Upper Palaeolithic of Siberia lack such discussion (e.g., Morlan 1987; Abramova 1989; Goebel 2004). Based on the presence of bifaces in Dyuktai, the primary investigator of the complex, Mochanov (1977:228-229; 1978:65), derives it from the Middle Palaeolithic assemblages of Europe, Central Asia, and China where bifacial tools are common. Indeed bifaces are found at many Middle Palaeolithic sites in eastern Europe (see Hoffecker 2002:94-95, 104-105) and in the Altai Mountains of southern Siberia (Derevianko and Shunkov 2002, 2004). Derevianko (1996, 1998) considers the Selemdzha [Selemdga] cultural complex of the Amur River basin in the Russian Far East to be the origin for the Dyuktai complex.

The critical question in the discussion of the Dyuktai complex's roots is the age of the earliest Dyuktai sites. Yi and Clark (1985) and Abramova (1989:232) assume the lower limit of the Dyuktai assemblages with bifaces and wedge-shaped cores to be ca. 18,000 BP, as opposed to Mochanov's (1977, 1978, 1984) determination of it as being ca. 35,000 BP, and possibly as late as ca. 26,000-23,500 BP (Vasil'ev et al. 2002:510). Recently, the Yana RHS site with bifacial tools was found in northern Yakutia (Pitulko et al. 2004), dated roughly to ca. 27,000 BP. The assemblage lacks wedge-shaped cores and is not related to the Dyuktai complex according to Pitulko et al. (2004:56). Thus, the origin of Dyuktai culture remains obscure; however, it is clear that emergence of the Dyuktai has nothing to do with the late Upper Palaeolithic microblade assemblages of the Transbaikal.

The issue of frozen mammoth bodies found in Siberia is also discussed in chapter 41. However, there are some factual mistakes. First, "The earliest recorded discovery is a mammoth from Berezovka, located in the far northeast of Siberia" which was made in 1900 (p. 384), is not the earliest one. It is well-documented that the first such find known to scientists was made in 1797 at the mouth of the Lena River; this was the so-called "Adams' mammoth" finally brought to Imperial Russian Academy of Sciences headquarters in 1808 (Garutt 2001:16); publication followed in a few years (Tilesius von Tilenau 1815). Second, the expedition to get the Berezovka mammoth in 1901 could not depart from Petrograd (p. 384), because at that time the official name of this city was Saint-Petersburg (it was renamed to Petrograd in 1914 due to the beginning of the first World War and the rise of anti-German feelings). As for

Wrangel Island as the place where "the last of the woolly mammoths [were] to walk upon planet earth" (p. 386), a second Holocene mammoth refugium is now known in the Bering Sea at St. Paul Island in the Pribilof Islands (Guthrie 2004; Yesner et al. 2005).

In the book, there are some small factual errors. Paul Martin's affiliation is the University of Arizona not Arizona State University (p. 213); Bluefish Cave is situated not in Alaska, USA, but in Yukon Territory, Canada (colour photo after p. 240); Japanese scholars listed as "Suago Yamanouchi and Hiroyuki Sato" (p. 372) are in fact Sugao Yamanouchi (see, for example, Aikens and Higuchi 1982:99) and Tatsuo Sato (see Pearson et al. 1986:162, 514; Kobayashi 2004:19; Habu 2004); the paper by Taylor, Haynes, and Stuiver (1996) was published in *Antiquity* but not *American Antiquity* as indicated on page 606; in reference to Vartanyan, Garutt, and Sher (1993) on page 607, the word "dwarf" in the paper's title is missing.

Besides this, there are several typographical mistakes in the book which derive, perhaps, from a not very careful reading of galley proofs. I would mention the most striking ones: "Miginik", p. 358 (instead of "Mogilnik"); "Aitkins", pp. 372, 373, and 559 (instead of "Aikens); "Studenhoe", pp. 383 and 389 (instead of "Studenoe"); "BC", endnote 16, p. 533 (instead of "BP"); "Antev", p. 548 (instead of "Antevs"—see, for example, Waters 1992:8; Bever 2001:128); "late-pleistocene", p. 555 (instead of "Late Pleistocene"); "Yanoi", p. 560 (instead of "Yayoi"); "levallois", p. 566 (instead of "Levallois"); "Biglow", p. 586 (instead of "Bigelow"); "Cainozoic", p. 598 (instead of "Cenozoic").

The mistakes, errors, misinterpretations, and other flaws indicated above can easily be fixed if following editions of this quite interesting volume are released. I hope that comments of this review will be helpful to the author in order to make his book better from the standpoint of academic standards. □

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