Out of Africa I: The First Hominin Colonization of Eurasia

John G. Fleagle, John. J. Shea, Frederick E. Grine, Andrea L. Baden, and Richard E. Leakey (eds.)
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In the preface, the lead editor, Fleagle explains the purpose of Second Stony Brook Human Evolution Symposium and Workshop in 2005, on which this volume is based as:

> "For over two-thirds of our evolutionary history, from our divergence from chimpanzees more than 6 million years ago, until as recently as 2 million years ago, hominins were an endemic African group. Sometime near the beginning of the Pleistocene, just under 2 million years ago, this all changed, and archeological and paleontological evidence of early hominins appears in many parts of Eurasia. The papers in this volume address the many facets of the first hominin range expansion from Africa into Eurasia"

This is the Out of Africa I (OOA1) paradigm in its strictest form. It is certainly true that the earliest evidence for hominins outside of Africa dates to "just under 2 million years ago" but none of the earliest non-African sites (Dmanisi, Nihewan Basin, Atapuerca, Sangiran, Riwat, Pabbi Hills) show any close similarity to contemporary African sites. Thus, it is unlikely that the story is a mere "range expansion" at this time. The OOA1 paradigm, at least in this strict form, lags behind the new data and retains an unhealthy hold on thinking. Thus, with a few exceptions, contributions to this volume skirt the important issues and fail to break from what should be a fading paradigm. What I find most fascinating about the papers in this book is how the authors reach such different conclusions from the current data than I would. To be fair, the last word in this volume is given to Dennell, who is a major critic of the strict OOA1 paradigm, but in spite of this, the basic tenets of the OOA1 loom over most of the papers, including Dennell's.

The book is divided into 5 sections based on geographical regions: "The African background, Eastern Asia, South Asia, Europe and West Asia and Summary, synthesis and new directions." Each section has 2–4 papers for a total of 16 papers in the volume.

The papers on the faunal data are scattered through the volume. In the African section there are two papers, one by Leakey and Werdelin and the second by Lewis and Werdelin; in the East Asia section there is the paper by Ciochon. Patnaik and Nanda review the fauna from South Asia. Belmaker and Martinez-Navarro discuss the fauna from Europe and West Asia.

The contribution on Pleistocene mammals of Africa (Leakey and Werdelin) is a good review of the known data. Emphasis is placed on dispersal patterns of Catarrhines and the early Pleistocene Carnivore guild. This emphasis is because the authors consider that hominin dispersal would have had the same constraints as that for Catarrhines before the shift to meat eating. They only investigate the time period between 1.5 –2 Ma. Surely, if *Homo erectus* was an OOA species, it is the period before rather than after 2 ma that is relevant? Finally, after detailed discussion, the conclusion is that "our assessment based on fossil carnivore and cercopithecid assemblages suggests that there were few if any migration events out of Africa contemporaneous with that of *H. erectus* and that those that may have occurred (*Megantereon*, *Hippopotamus*, *Theropithecus*; Rook et al. 2004, Martínez-Navarro 2004) could have been due to factors distinct from those that led to the dispersal of *H. erectus*." The second paper by Lewis and Werdelin , considers the relationship between *Meganteron* and hominin dispersals in more detail. Their concluding section is inconclusive as they state that the answer to the questions posed is "We just do not know at present." I think these two papers just emphasize that the appearance of *Homo erectus* on three continents shortly after 2 Ma is not to be found in Africa at all, or at least not in Africa *after* 2 Ma.

Ciochon's paper, in contrast, is really paradigm breaking (or paradigm returning? as he is one of the few who supported early presence of hominins outside of Africa…). Either way, it is refreshing to read a paper in which new data have changed the interpretations held earlier. In this paper Ciochon retracts some of his own long held views and argues that hominins were not part of the *Stegodon Ailurpoda* fauna of southern China. He accepts that fossil teeth attributed to early hominins actually belong to late surviving ape lineages. He suggests that *Homo erectus* is associated with grassland ecosystems both to the north and south of the South China/Mainland SE Asia ape refugium (i.e., the *Stegodon-Ailurpoda* faunal region). This has already been published in an article in *Nature* (Ciochon 2009), but the treatment in this paper in the OOA1 volume is more leisurely and elaborate. This view is also held by Etler (2001, 2009), who recently (Etler 2011) suggested in a blog post, that Late Miocene apes outside of Africa showed a similar degree of affinity to hominins as do those in Africa, a view recently expressed by Wood and Harrison (2011). Hou and Zhao's paper on the archaeological evidence for the earliest hominin presence in China presents the stone tool evidence for many of the southern China sites also discussed by Ciochon. The artifacts illustrated in this paper do little to contradict Ciochon's thesis.

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Patnaik and Nanda's paper is a good review of the fossils from the Pinjor formation. One very good feature of this paper is the discussion of the paleovegetation and the combined inferences about the paleoecology. Also discussed is the dispersal of some species from India to Western Eurasia, which occurs mostly in the Early to Middle Pleistocene, while African herbivores dispersed to the Indian subcontinent in the period of 3–2.5 Ma. The only lacuna in this paper is the absence of any discussion of the relationships between the Javan fauna associated with *Homo erectus* and the Indian Pinjor fauna. Dutch paleontologists such as de Vos (1982 a, b, 1996, 2004, 2007) have repeatedly stressed the close relationships between them. In the context of Ciochon's paper, de Vos's inference that the *Homo erectus-Stegodon* fauna of Java arrived there via the Siva Malayan route rather than the Sino Malayan route is important. The Sino Malayan source area is the *Ailuropoda-Stegodon* region of Southern China while the Siva Malayan source area is the Indian sub-continent.

The separate papers by Belmaker and Martinez contribute to the discussion of the fauna of Europe and West Asia. Although they take different approaches, ultimately they are in fairly close agreement with an acknowledgement that few African species dispersed beyond the Levant and that the dispersal of African species was not an expansion of the Africa habitats but expansion of a few African species into the Mediterranean habitat. While Belmaker emphasizes the uniqueness of the hominin expansion, Martinez stresses the importance of the dispersal of African species beyond the Levant as these episodes are related to periods of significant ecological change. Both of them discuss the importance of meat eating as an adaption to the greater seasonality of the Western Eurasian region compared to that of Africa.

Lahr, in the African background section, discusses the dispersal of hominins within Africa, especially between East Africa and Northern Africa. The importance of the Sahara Desert as a barrier to dispersal is underlined. While her summary of the archaeological data is interesting, her 4C model (Causes, Conditions, Constraints, and Consequences), which appears in figure form twice (surely a mistake?) is overly simplified. A further problem is that her paleoenvironmental reconstruction is based on Late Pleistocene data. There are probably major contrasts in the paleoenvironment between the Lower Pleistocene and the Late Pleistocene.

Shea's article on "strategic perspectives" is overly theoretical for my taste. He discards cultural and biological variation and suggests "strategic variation" to explain the variability in the earliest stone tools. This discussion is very brief and does not do justice to the issues. Variation in stone tools could be explained by many factors. Personally I disagree with many of the facts which Shea has taken for granted such as "Early Acheulean stone tools are not among the earliest generally accepted paleoanthropological sites in Europe, Western Asia, India/Pakistan, Southeast Asia, or China." On the contrary, we have recently argued (Mishra et al 2010) that the earliest tools in India and SE Asia are Acheulian and this has recently found support in dates of 1.5 Ma from Attirapakkam for the Acheulian (Pappu et al 2011). The Acheulian site of 'Ubeidya actually is almost the earliest site in Western Asia. It is only in Europe and Northern China that the earliest sites are not Acheulian, and these are the regions which differ the most in ecology from Africa. Similarly, the "Oldowan" versus "Acheulian" dichotomy is probably not very useful as both likely encompass multiple entities. While the idea that biologically similar hominins might have different technologies is acceptable, does it follow that biologically dissimilar hominins could have the same technology? Although this second is often asserted I think it should be looked at more carefully before being accepted. Thus while Shea builds a nice story, the basic premises for it are shaky.

Potts and Teague's article is actually the only paper in the book which takes an overview of all the regions. They do a good job of summarizing the issues and consider early non-Acheulian stone tool assemblages equivalent to Oldowan. Focusing on the time horizon of 1.7 Ma they find that the hominin dispersal is not closely related to the dispersal of any other African species. The ability to adapt to new environments is stressed. The timing of the dispersal is considered unclear, but if it is close to the age of the earliest evidence in China, then it was very rapid. Potts and Teague give a strong defense of the Yuanmou site which they consider the oldest in China. They point out that the fauna at Yuanmou includes grassland species, indicating that *Stegadon-Ailuropoda* fauna was not there throughout the sequence. The final paleoenvironmental paper is Zaim, who provides a very good summary of the latest geological background to the important fossils from Indonesia.

The only paper in this volume which directly discusses the fossil hominins is Rightmire and Lordkipanidze who give a thorough review of the Dmanisi evidence. They argue that the Dmanisi data imply that *Homo erectus* evolved in Asia rather than Africa as the Dmanisi hominins show features more primitive than *Homo erectus*.

Separate papers by Chauhan and Petraglia evaluate the archaeological evidence from India in relation to the OOA1 hypothesis. Dennell, in the concluding chapter, also discusses the Indian evidence. Petraglia's and Chauhan's papers lack of serious discussion of the Acheulian is puzzling. In my view there *is* no non-Acheulian Lower Paleolithic in the Indian sub-continent (Mishra 2007). However, the conclusion drawn from this seems to be that therefore there is no Lower Pleistocene occupation of the Indian subcontinent at all! The assumption appears to be that all the Lower Pleistocene lithic technologies outside of Africa are non-Acheulian. Thus, Petraglia-Chauhan-Dennell seem to expect the earliest assemblages in India to be "mode 1" and since the evidence for "mode 1" is virtually absent (although Chauhan tries very hard to find it), they are forced to conclude that South Asia was not occupied at an early time period. The possibility that the earliest occupation of India was with Acheulian technology and is as early as that in Africa is not considered at all**.** Hopefully the recent (Pappu et al 2011) dating of the earliest levels with Acheulian at

Attirapakkam to 1.5 Ma will change this.

The set of papers on the Indian sub-continent suffer not just from a misunderstanding of the significance of the Acheulian but also by a lack of understanding of the nature of the sites and formation processes. The destruction of stone tools exposed to the surface is not properly appreciated. It has long been observed that Acheulian sites in India are found in abundance in areas of quartzite bedrock. Korisettar (2007) has revived this idea in the form of a suggestion that the "Purana basins" were the focus of human activity in the Lower Paleolithic. This idea is enthusiastically taken up by Petraglia, Dennell , and Teaford and Potts in this volume. The "Puranas" are Proterozoic geological formations dominated by quartzite lithologies, so that the abundance of sites in the Purana basins is actually the same as saying they are abundant in areas of quartzite. Contrary to Korisettar, I have explained the differential abundance of Acheulian sites by the differential survival of quartzite versus other rock types and not by hominin preference for a particular rock type (Mishra 1982), which is unlikely in view of the large number of rock types actually used. Dennell, grossly overestimating the survival of lithics, thus grossly underestimates the density and continuity of past human populations.

Africa is still the only continent where the pre-*Homo* genus, *Australopithecus*, is present and so OOA1 remains a robust hypothesis. In view of the diversity of the fossil hominin and archaeological records in regions outside of Africa, coinciding with the first appearance of the *Homo* genus, the timing, species involved, and cultural behavior of the OOA1 event requires close re-evaluation. The timing is especially important. The papers in this book do not examine these fundamental issues so that even in the case of excellent reviews, the time frame or paleoecological background for the discussion might well be irrelevant. Surprisingly, archaeological data are given little importance. Paleontological and paleoclimatic data is relied on to a greater extent than the data from archaeology and hominin fossils. Failure to appreciate the real significance of the absence of Lower Paleolithic mode 1 in India or the antiquity of the Acheulian in India also skews the whole outlook, and not just in relation to the Indian record.

REFERENCES

Ciochon, R.L. 2009. The mystery ape of Pleistocene Asia. *Nature* 459: 910–911.

de Vos, J. 1996. Faunal turnovers in Java in relation to faunas of the continent. *Odontologie* 1: 32–36.

de Vos, J. 2004. The Dubois collection: a new look at an old collection. *Scripta Geologica* 4: 267–285.

de Vos, J.and Sondaar, P.Y. 1982. The importance of the Dubois Collection reconsidered. *Modern Quaternary Research in South East Asia* 7: 35–63.

de Vos, J. Van den Hoek Ostende, L.W., and Van den Bergh, G.D. 2007. Patterns in Insular Evolution of Mammals: A Key to Island Palaeogeography. In: Renema, W. (ed.), *Biogeography, Time, and Place: Distributions, Barriers, and Islands*. Dordrect, Springer, pp 315–345.

de Vos, J., Sartono, S., Hardja-Sasmita, H.S.,and Sondaar, P.Y. 1982. The fauna from Trinil, the type locality of *Homo erectus*: a reinterpretation. *Geologie en minbouw* 61: 207–211.

Etler, D., Crummett, T.L., and Wolpoff, M.H. 2001. Longgupo: Early *Homo* colonizer or late Pliocene *Lufengpithecus* survivor in south China? *Human Evolution* 16(1): 1–12.

Etler, D.A. 2009. Mystery ape: other fossils suggest that it›s no mystery at all. *Nature* 460: 684–684.

Etler, D.A. 2011. http://sinanthropus.blogspot.com/2010/08/its-time-to-sink-genus-australopithecus.html

Korisettar, R. 2007. Toward Developing a Basin Model for Paleolithic Settlement of the Indian Subcontinent: Geodynamics, Monsoon Dynamics, Habitat Diversity and Dispersal Routes. In: Petraglia, M. and Allchin, B. (eds.), *The Evolution and History of Human Populations in South Asia*. Dordrecht, Springer, pp 69–96.

Mishra, S. 1982. On the Effects of Basalt Weathering on the Distribution of Lower Palaeolithic Sites in the Deccan. *Bulletin of the Deccan College Postgraduate and Research Institute* 41: 107–115.

Mishra, S. 2007. The Indian Lower Palaeolithic. *Bulletin of the Deccan College Postgraduate and Research Institute* 66-67: 47–94.

Mishra, S., Gaillard, C., Hertler, C., Moigne, A.-M., and Simanjuntak, H.T. 2010. India and Java: Contrasting Records, Intimate Connections. *Quaternary International* 223–224: 265–270.

Pappu, S., Gunnell, Y., Akhilesh, K., Braucher, R., Taieb, M., Demory, F., and Thouveny, N. 2011. Early Pleistocene Presence of Acheulian Hominins in South India. *Science* 331: 1596–1600.

Wood, B. and Harrison, T. 2011. The evolutionary context of the first hominins. *Nature* 470: 347–352.