Review Essay

What do we know about African iron working?

David Killick

Les Routes du Fer en Afrique (http://www.unesco.org/culture/dialogue/iron/html_fr/index_fr.shtml)

The Origins of Iron Metallurgy: Proceedings of the First International Colloquium on The Archaeology of Africa and the Mediterranean Basin/Aux Origines de la Métallurgie du Fer: Actes de la Première Table Ronde Internationale d'Archéologie (L'Afrique et le Bassin Méditerranéen). Jean-Paul Descoeudres, Eric Huysecom, Vincent Serneels & Jean-Louis Zimmerman (eds.). *Mediterranean Archaeology* volume 14, 2001. (ISSN 1030-8482; 309 pages, figures, 8 plates. Price Australian $ 55,-).

Aux Origines de la Métallurgie du Fer en Afrique: Une Ancienneté Méconnue. Afrique de l'Ouest et Afrique Centrale. Hamady Bocoum (ed.). Paris: Éditions UNESCO, 2002, ISBN 92-3-203807-2. (240 pages, 13 figures, 87 photographs. Price EUR 22,-.)

The UNESCO project "Les Routes du Fer en Afrique"

In 1988 the General Assembly of the United Nations designated the next ten years as the Decade of Cultural Development. As its contribution to this campaign, UNESCO's Division of Intercultural Dialogue invited proposals for educational initiatives based upon cultural themes whose scale was larger than that of individual nation states. The most widely known of these initiatives was the acclaimed international touring exhibition on the histories and cultures of the regions along the central Asian Silk Route that formerly linked China to the Mediterranean. Among other proposals submitted was one entitled „Les Routes du Fer en Afrique", proposed in 1991 by the Mozambique National Committee for UNESCO, with the active support of France, Belgium, Portugal, Venezuela and the Organization of African Unity.

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The planning document for „Les Routes du Fer en Afrique" emerged from a conference held in Maputo in December 1991. Ten of the eighteen participants were from southern Africa (Mozambique, Swaziland, Zimbabwe); the remainder were from the UNESCO Secretariat, Senegal, Burundi, Algeria, Zaïre and Belgium. The recommendations of the conference are summarized on the project web site and also in the volume edited by Bocoum (pp. 219-224). The major proposed activities were the following:

- to investigate the role of iron in the history of Africa;
- to create a bibliography of prior studies, both published and unpublished (e.g. student theses);
- to solicit financial support from industry for new research ;
- to undertake new research upon all aspects of iron in Africa, specifically including the geology of iron ore deposits, smelting and forging technology, economic aspects (agriculture, trade, currencies), social roles (e.g. bridewealth, symbols of power), comparative linguistic studies of terms for iron and iron working, and the role of iron in the colonial and post-colonial industrialization of Africa; and
- to consider whether indigenous African techniques of iron production could be revived in remote rural areas as a cost-effective alternative to importing iron from outside the continent.

The goals proposed by the Maputo Conference were accepted by the Division of Intercultural Dialogue at UNESCO, which appointed an international Scientific Committee of nineteen individuals to direct the project. Among these were six archaeologists (Hamady Bocoum, Pierre de Maret, Jean Devisse, Lech Krzyzaniak, Mohamed Ould Khattar and Kléna Sanogo); the other appointees were five historians, two linguists, two sculptors, a conservator, a geologist, an engineer, and a former Minister for Culture in Nigeria. Thirteen of the nineteen were resident in Africa.

UNESCO then commissioned a feasibility study, which was submitted by Susan Legêne of the Royal Tropical Institute in October 1993. This recommended the preparation of a bibliography, a travelling exhibition, a documentary film, and publications and radio programs aimed at schools and the general public. All of these would be intended primarily for African audiences. A very limited role was proposed for academic inquiry, with only a single conference to establish the "state of the art", to discuss theoretical and methodological issues, and to prepare a publication.

The feasibility study was circulated for comment to several authorities on the subject of indigenous African iron working, one of whom was myself, and bibliographies of the relevant literature were commissioned. A limited selection of references, and a link (no longer functional) to the on-line bibliography prepared by Duncan Miller and Tim Maggs, can be found on the project web site, as can a directory of researchers in the field of African metallurgy with their contact information and relevant publications up to 1994. Plans for a travelling exhibition were also begun, under the co-presidency of Hamady Bocoum and Pierre de Maret, assisted by a "Commissariat Scientifique" of no fewer than thirty-one scholars, all but three of whom were from Francophone nations.

In February 1995 the project sponsored a workshop in Abuja on the theme „25 centuries of iron working in Nigeria". Three papers presented at the workshop (those by Joseph Jemkur, Edwin Okafor and Isaac Akinjogbin) are included in French translation in the volume edited by Bocoum. In June 1999 UNESCO was one among several sponsors of the conference „L'Afrique et le Bassin Méditerranéen: aux Origines de la Métallurgie du Fer" at the University of Geneva, and since published as a special issue of *Mediterranean Archaeology* (volume 14, 2001, though it did not appear until December 2002). Twenty of the twenty-nine contributions to this issue discuss iron metallurgy on the African continent.

In November 1999 an exhibition entitled "Les Routes du Fer en Afrique" was presented at UNESCO headquarters in Paris, with financial support from UNESCO, the French steel company USINOR and several other sponsors. Although this exhibition is still advertised on the project web site as a travelling exhibition, my own inquiries have turned up no evidence that it has travelled beyond Paris. Much of the text and a selection of the images from the exhibition can however be downloaded from the project website as a 50-page Adobe pdf document. This has five sections. The first is a display of African ironwork as art, juxtaposing historic forged iron objects with the work of living artists from Burkino Faso and Madagascar. The second, entitled "Les débuts du fer en Afrique", presents as established fact the claim that iron working was invented in Niger in the third millennium BC. (This claim derives from the research of Gérard Quéchon and Alain Person, which will be critically examined below). The third section, "Savoir le Fer", provides basic information on iron ores in Africa, on ancient and modern techniques of mining them, and a few illustrations of indigenous African smelting techniques. The longest section is devoted to the role of iron in society. It touches on the past importance of iron in agriculture and warfare, on the economic and ritual roles of ironworkers, and on the continuing importance of blacksmiths, working with recycled steel, in Africa today. The fifth section, "Lire le Fer", is a very brief discussion of the aims and methods of archaeometallurgy. Tacked onto the back, without explanation, is a schematic diagram of a state-of-the-art USINOR steel plant. A symposium was held in conjunction with the exhibition at which papers by Hamady Bocoum, Pierre de Maret, Gérard Quéchon, David Aremu and Bruno Martinelli were presented. All of these are included in the volume edited by Bocoum.

In April 2000 some members of the Scientific Committee joined UNESCO officials and selected African politicians in Addis Ababa to issue a list of recommendations, addressed to African governments, for further research into African iron working, for conservation of important iron working sites, and for study of the revival of indigenous African iron working for rural development. Another conference was scheduled for late 2001, but it is not clear whether this ever took place, as the „Routes du Fer en Afrique" web site on the UNESCO server has not been updated since July 2001. It appears that UNESCO is no longer funding the project.

As this summary makes plain, few of the ambitious goals proposed by the 1991 Maputo conference or in the 1993 feasibility study have been realized. Apart from the exhibition, which was aimed at a general audience, the major products of "Les Routes du Fer en Afrique" thus far have been papers generated by scholarly conferences. In the remainder of this review I examine the two published works that derive from conferences funded in whole or in part by UNESCO.

The origins of iron working around the Mediterranean and the Nile

This special issue of *Mediterranean Archaeology* is particularly welcome because it combines fourteen papers on iron working in sub-Saharan Africa with up-to-date summaries of the evidence for the earliest iron working in the Aegean, Anatolia, Egypt, France, Spain and North Africa. For the last two decades Africanists have relied upon the relevant chapters in *The Coming of the Age of Iron* (Wertime & Muhly 1980) for summaries of research outside Africa. This work has served us well, but may now be retired.

Two chapters in *Mediterranean Archaeology* examine the supposed role of the Hittites in the innovation of iron metallurgy. Eric Jean provides an annotated list of the earliest iron objects recovered in excavated contexts in Anatolia, while Jana Souckova-Siegelová summarizes accounts of iron in Hittite and Old Assyrian documents. Iron was certainly known much earlier than Hittite times. A dozen iron objects, including two remarkable sword blades, have been recovered from deposits of Early Bronze Age II and III (2800-2100 BC), and some three dozen, including several samples of steel, from the Late Bronze Age (ca. 1700-1200 BC), which coincides with the span of the Hittite Empire[1]. Jean emphasizes that iron of any kind was very rare in comparison to copper and bronze. Souckova-Siegelová notes that the many references to iron in Hittite documents are almost all from royal records, and show that iron was in Hittite times a rare and very valuable material reserved for royal use as ritual objects, or given as gifts to other kings. Not until the New Hittite era (1400-1200 BC) were iron weapons worn by palace elites, and iron objects are not as abundant as those of bronze in non-elite Anatolian sites until the Middle Iron Age (after 850 BC). Both authors agree that earlier writings, which attributed Hittite military success to the possession of iron weapons, have no basis in fact.

Indeed, Anatolia lagged behind both Cyprus and Greece in the widespread adoption of iron, as Jean-Louis Zimmermann shows in his chapter on the Aegean. Although the earliest objects of iron in Greece and Cyprus are dated only to the thirteenth century BC, iron appears to have come into widespread use by the tenth century. Metallography has revealed that a few of these weapons and tools were of quenched and tempered steel. There is unfortunately no chapter on the earliest iron in the Levant and Palestine – an omission that detracts considerably from the utility of this volume.[2]

Michel Valloggia's survey of Egyptian inscriptions emphasizes (1) the lack of specificity in Egyptian hieroglyphic and hieratic terms for metals, which makes it difficult to determine when iron was first smelted in Egypt, and (2) that there are no certain references to the working of smelted iron (as opposed to iron meteorites) before the sixth century BC. He makes a strong argument that the magnificent iron-bladed dagger, with gold hilt and scabbard, from the tomb of Tutankhamen (14th century BC) was not made in Egypt, but was a gift from Anatolia. Michel Wuttmann, who contributes a short chapter on the archaeological evidence, concurs with this assessment. He also lists all reported finds of iron before the advent of the New Kingdom. Most of these were found in dubious context or were mere piles of rust. Only two pieces have trustworthy chemical analyses; both contain more than 7% nickel and were probably forged from iron meteorites. Tutankhamen's tomb contained nearly two-thirds of all iron objects from contexts older than 1300 BC, but unfortunately none of these have been subjected to scientific analysis. His rule marks the beginning of a period in which iron objects occur more frequently, though it is not yet known what proportion of these are local products and what are imports. The first iron tools are in contexts dated after 1000 BC, but it was not until the 26th and 27th Dynasties (663-405 BC) that tools were commonly

1 Throughout this review historical dates (or dates derived from historical dates by crossdating) are cited as calendar years BC or AD, uncalibrated radiocarbon ages are cited as radiocarbon years BP, and calibrated radiocarbon dates as calendar years cal BC or cal AD. All calibrations cited were made with the computer program OXCAL 3.9 (Bronk-Ramsey 2003) at two sigma (two standard deviations, 95.4% confidence interval).

2 Reviews of the evidence for the origins of iron in the Near East, the Indian subcontinent and China may however be found in the volume edited by Pigott (1999).

made of iron. Wuttmann notes that we know little more about Egyptian iron technology now that we did in the 1960's.

Patrice Lenoble provides a superb, though depressing, commentary on the history of research into iron working at Meroe. Ninety years after Sayce's ill-conceived labelling of Meroe as "the Birmingham of ancient Africa", Lenoble's highly critical review shows that we still understand almost nothing about iron working in ancient Nubia. Neither the chronology of iron working at Meroe nor the scale of production have been firmly established, still less the questions of state control of the industry, or of its possible environmental impact. Nor do we even know how iron was used, except for its deposition in royal funerary rites. Lenoble argues convincingly that the abundance of iron arrows in some royal tombs is a symbolic statement of the power of the ruler, and cannot be taken as an index of the abundance of iron in Meroitic society at large.

Although little progress has been made in the study of Egyptian or Nubian iron working since the publication of *The Coming of the Age of Iron*, there have been many recent finds at the western end of the Mediterranean. Proponents of an indigenous invention of iron working in West Africa have long emphasized the lack of evidence for Phoenician iron working in North Africa, but that objection is no longer valid. The evidence from Carthage, summarized in a chapter by Hans Georg Niemeyer, now includes an entire iron working quarter used between the fifth and second centuries BC (excavated by Serge Lancel), and two separate dumps of tuyères and slag, both dated to the eighth and seventh centuries BC (excavated by Niemeyer and by F. Rakob). Chemical and mineralogical analyses of the earlier slags from are presented in a chapter by Ingo Keesmann. He concludes that both the copper and the iron slags from these deposits derive from secondary processing (crucible melting of copper, forging of iron). No furnaces were found in association with the earlier dumps, but Niemeyer includes a reconstruction of a slightly younger (late seventh/early sixth century BC) Phoenician iron smelting furnace that he excavated at Toscanos in Spain. This is reconstructed as a domed slag-tapping furnace.

Carme Rovira Hortalà's chapter is a comprehensive discussion of the earliest iron in the Iberian Peninsula. A large number of finds have been published since 1980, and allow the first two centuries of the use of iron to be traced in some detail. There are rare finds of iron objects in Late Bronze Age contexts, dated to the tenth and ninth centuries BC. Most scholars think that these items were traded from the eastern Mediterranean. On the south and south-eastern coast of Spain iron objects first appear in quantity in the tombs of Phoenician colonists during the eighth century BC. Iron has been found in both Phoenician and non-Phoenican contexts in present Portugal and in the interior of Spain that are dated to the seventh and sixth centuries. In Catalonia and southern France, around the vicinity of modern Marseilles, the first iron objects date to the eighth century BC, but precede the first appearance of Phoenician trade goods. Scholars are divided over whether these are Greek trade goods or whether they reflect an *in-situ* development of iron technology.

A characteristic item of material culture has been excavated from the earliest Phoenician metallurgical sites in both Carthage and Iberia, and is well illustrated in the chapters by Niemeyer and Rovira. This is a unique type of fired clay tuyère, with rectangular cross-section enclosing two parallel channels of circular section, apparently designed for the simultaneous operation of two pairs of bellows. I will return to this item below after reviewing the chapters on iron in sub-Saharan Africa

The origins of iron working in sub-Saharan Africa

The special issue of *Mediterranean Archaeology* (*MA* hereafter) contains eight chapters (not counting summaries) on the origins of iron working in Africa, and the UNESCO volume edited by Hamady Bocoum (*UN* hereafter) has seven. There is some duplication; the chapter by Alain Person and Quéchon in each volume is essentially identical, and that by Gérard Quéchon differs only in the preface.

The chapters in *MA* attempt a survey of the evidence for the spread of iron working throughout sub-Saharan Africa. The coverage is not comprehensive, as there is no chapter on the earliest iron in Eritrea, Ethiopia or Somalia, though the data from crucial regions are as yet too scarce to allow any firm conclusions to be drawn. Marie Claude van Grunderbeek, Emile Roche and Hugues Doutrelepont review the evidence excavated in Rwanda and Burundi between 1960 and 1986.[3] Their discussion of the earliest radiocarbon dates

[3] Those who do not read French will soon have the option of van Grunderbeek's excellent Ph.D. thesis, to be submitted in English to the University of Cape Town.

from this region is more cautious than in their earlier articles. They now reject the date of 3615 ± 205 BP for the Muganza I furnace, and caution that the lack of visible stratigraphy makes it difficult to be certain that three other dates, ranging from ca. 3200 BP to ca. 2850 BP, really were associated with the iron slag and Urewe ceramics excavated with them (p. 276). Two other dates of 2815±165 BP and 2635±95 BP are in certain association with smelting residues, but the huge standard deviations result in calibrated age ranges of 1450-500 cal BC and 1000-400 cal BC respectively. Clearly neither of these dates is of much use in determining when iron working actually began in this region. The technology employed in the earliest furnaces in Rwanda/Burundi – truncated conical shafts of clay "bricks" erected over a slag pit. This is the same as in the adjacent region of Buhaya, Tanzania, where Schmidt (1997) suggests that iron working may have begun as early as 600 cal BC, though he acknowledges that this dating is far from secure.

Even if the origins of iron working in this region are still obscure, it should be stressed that in no other part of sub-Saharan Africa has iron working in the first millennium cal BC been as carefully studied, or so fully published, as in Buhaya and Burundi. Unfortunately there is almost nothing to which we can compare these studies. Research on early iron working technology in the upper Nile, Ethiopia, Eritrea or in the Arabian Peninsula has barely begun, so there is simply no point as yet in debating whether iron working was independently developed in, or diffused into, the regions around the Great Lakes.

Although Pierre de Maret (UN, p. 125) states that that there is a "consensus assez général" that iron working began throughout Central Africa (Cameroon, Gabon, the Central African Republic and Rwanda) by the late ninth century BC, some of those whom he cites appear not to agree. I have already noted the reservations of van Grunderbeek and colleagues regarding the earliest dates from Rwanda. Bernard Clist's abstract (*MA*, p. 269) places the start of iron working in Cameroon at ca. 450 BC, and that in Congo (Brazzaville) and Gabon at around 200 BC, while Richard Oslisly (*MA*, pp. 263-268) favors a start in Cameroon at ca. 2600 BP, reaching Gabon around 2500/2400 BP and Congo around 2200 BP. Clist, Oslisly and de Maret are drawing on the same corpus of published radiocarbon dates, so why do their estimates for the first iron working differ so much? The differences reflect their individual decisions about the presentation of radiocarbon dates. Clist and Oslisly are both citing uncalibrated radiocarbon dates, and ignoring the associated standard errors, while de Maret appears to be arbitrarily picking the older end of the two-sigma probability range of calibrated dates. A great deal of confusion would be avoided if archaeologists in general, and Francophone archaeologists in particular, would consistently use the established conventions – BP (and only BP) for uncalibrated radiocarbon ages, and cal BC and cal AD (or cal BCE and cal CE, if preferred) for calibrated dates.[4]

De Maret would like the first use of iron in Central Africa to be even older than this, and proposes (UN, p. 125) that the sudden disappearance ("disparition brutale") of stone tools on open sites in Congo (Brazzaville) and Cameroon after 3500 BP is attributable to the adoption of iron. Forced to confront the complete absence of evidence for iron or iron working in these regions before the first millennium BC, he argues that the metal was imported (from where?) and that recycling of iron and corrosion have left none for the archaeologist to find! His hypothesis has the great advantage of being completely untestable, but the form of argument seems better suited to theology than to archaeology.

Nothing useful is known about the beginning of iron working in Congo (Kinshasa) or Angola, but Duncan Miller (*MA*, pp. 229-234) picks up the trail again in southern Africa. There is secure evidence of iron working in eastern half of southern Africa dated between the second and sixth centuries cal AD, but the earliest known evidence from the western half dates to the sixth century cal AD. These may represent two separate introductions of iron technology – the former from Tanzania via Zambia and Zimbabwe, the latter from Congo via Angola – but the evidence is as yet inadequate. Miller's chapter draws upon a substantial quantity of new data, much of it generated by himself in more than fifty publications over the last twelve years. He notes that there appears to be little change in the techniques for smelting or forging iron over the 1800 years after their earliest occurrence, and offers some preliminary thoughts on why these technologies were so stable over time. He concludes by noting the difficult middle course that responsible scholars of African technology must steer between those who see the record as one of technological stagnation and those

[4] Archaeologists should also state which calibration chart, or software package, was used to generate the calibration, and whether the calibrations are at one or two sigma.

who make wildly exaggerated claims for ancient African achievements in science, mathematics and technology.

There is very little new data on early iron working in West Africa in either volume, but the UNESCO volume does perform a valuable service by bringing together several important studies in an attractive package at a very reasonable price. This includes a French-language summary of Edwin Okafor's excellent 1992 Ph.D. thesis for Sheffield University on the development of iron working around Nsukka, Nigeria. Unfortunately Okafor has not, since his return to Nigeria, had access to either the funding or to the scientific facilities that would have enabled him to apply his training to new research.

The most controversial chapters are those by Gérard Quéchon (one written with Alain Person, one without) on their discoveries in the Termit massif of eastern Niger. As long ago as 1974 Quéchon and Jean-Pierre Roset published a radiocarbon date (on charcoal) of 2630 ± 120 BP (Dak-145) for one of a group of 22 very small slag-pit iron working furnaces at Do Dimmi 15, and a date of 2925 ± 120 BP (Dak-147) for charcoal found with iron objects on a deflated surface site (Quéchon & Roset 1974). Although these dates were noted with interest, claims for an independent invention of iron working in Niger (Quéchon & Roset 1974; Diop 1976) were not widely accepted for lack of supporting evidence. In the mid-1980's Danilo Grébénart claimed to have discovered even earlier evidence for metallurgy in Niger. His research west of Agadez included the excavation of more than two dozen fired clay structures, from which he obtained some thirty radiocarbon dates. Using these dates, the morphology of the structures, and partial chemical analyses of the residues excavated from them, he defined four phases of metallurgy in the Agadez region (Grébénart 1985: Figures 275 and 276). These were Cuivre I (*ca* 4200 BP-*ca* 3000 BP), Cuivre II (*ca* 2800 BP- *ca* 2000 BP), Fer I (*ca* 2500 BP – *ca* 2000 BP) and Fer II (after *ca* 2000 BP).

Grébénart's claim that Cuivre I represented an independent invention of copper metallurgy in Niger was almost immediately contradicted by petrographic studies of samples of supposed slags from Cuivre I structures, which showed that these were just soil partly vitrified by intense heat (Killick *et al*. 1988). Our results appear to confirm the view of Suzanne Bernus (1983: 169) that the irregular "furnaces" from which these samples came, and the charcoal for the radiocarbon samples, were actually the remains of burned-out tree stumps and roots from savannah forest killed by the dessication of the northern Sahel between 4000 and 3000 BP. I warned that unexpectedly early radiocarbon dates for metallurgy in the Sahel may reflect the use of old wood, or old charcoal, and suggested two ways to detect, or avoid, "old wood" errors (Killick 1987; Killick *et al.* 1988). The first is to compare dates on wood charcoal with thermoluminescence dates on samples of fired clay from the same furnace; the second is to avoid dating bulk wood charcoal wherever possible by dating samples of annual plants instead.

The first recommendation has been ignored, but Person and Quéchon, aided by Jean-Pierre Saliège of the University of Paris radiocarbon laboratory, have found a way to implement the second by extracting charred annual plants (grass, seeds or chaff) from potsherds collected from surface sites around Termit. In their essentially identical chapters in the UA and MA volumes they list 23 radiocarbon dates on annuals extracted from ceramics, along with eleven conventional radiocarbon dates on charcoal. The ceramic temper dates have very poor precision (90-300 radiocarbon years at one sigma) but there is no reason to question their accuracy.[5] They range from *ca* 7000 BP to *ca* 2000 BP and, with a few exceptions, correspond to the four very broad ceramic groups defined by Quéchon and his colleagues. Objects of iron and copper have been recovered on sites with pottery of type "post-néo phase 1", which yielded eight ceramic temper dates ranging from 3300 ± 120 BP to 2430 ± 110 BP, or with type "post-néo phase 2", with two ceramic temper dates of 2270 ± 90 BP and 2095 ± 200 BP.

All of the Termit pottery temper dates are from surface scatters. In five instances it was possible to compare dates on pottery temper with dates on wood charcoal from the same surface scatter. The comparison is presented as a table in both volumes (UN, p. 110; MA, p. 251). Although Quéchon finds the results of the comparison "très convaincants" (MA, p.250), this is not at all obvious from the table. In two cases (Gara Tchia Bo 176 and Termit Ouest 8B) the dates from bulk charcoal and ceramic temper are within one sigma of each other, though the standard deviations on the dates are large;

5 These dates were obtained by decay-counting, not by accelerator mass spectrometry (AMS), and the poor and variable precision reflect the fact that only small (and variable) amounts of carbon could be extracted from the sherds. Much better precision could now be obtained by dating organic temper by AMS, which requires less than a milligram of carbon per sample.

in a third (Gara Tchia Bo 20) the pottery temper date is within two sigma of one charcoal date, but separated by more than four sigma from the other; in the fourth (Do Dimmi 16) the two dates are barely within three sigma; and in the fifth case (Gara Tchia Bo 75) pottery and charcoal dates are more than five sigma apart, and each is at least two thousand radiocarbon years younger than expected.

These dates are nevertheless acclaimed by several of the contributors to the UNESCO volume as proof that iron working was invented in sub-Saharan Africa. The chapters by Person and Quéchon provide graphical comparisons of uncalibrated radiocarbon dates older than 1000 BP that have been reported as associated with iron objects, or with evidence of iron working, in Niger, Nigeria, Cameroon, Sudan (Meroe) and Rwanda/Burundi. This exercise shows that the oldest radiocarbon dates are from Termit, and Person and Quéchon seize the opportunity to insist that an independent invention of iron working in Niger is proved.

Their claims are accepted without comment by de Maret (UN, p.125) and are extended in the brief chapter by Marie-Louise Maes-Diop that concludes the UNESCO volume. Her table of the earliest appearance of iron in various regions of the world (UN, pp. 189-190) leads her to claim that iron working at Termit is at least as old as any in the world. This is a pointless exercise, as her table makes no attempt to distinguish between iron forged from meteorites and iron smelted from the ore. Both the preface and the back cover of the UNESCO volume proclaim that "l'industrie du fer est un marqueur lourd de la civilisation", a sentiment that Maes-Diop clearly endorses. But even if one accepts this statement (was the Incan empire, which lacked iron, not a civilization?) it can only apply to the mastery of iron *smelting*, for the forging of iron-nickel meteorites requires no advanced technological skills. Historians of metallurgy are always careful to distinguish between meteoritic iron and smelted iron, but Maes-Diop does not appear to be aware of the importance of this distinction. Nor, it appears, has Quéchon even considered the possibility that iron objects from Termit might not be smelted iron, for there is no mention in these chapters of any metallographic or chemical study of these artefacts.

The two chapters by Hamady Bocoum (MA, pp. 235-245; UN, pp. 93-103) also accept, and try to reinforce, the claims of Person and Quéchon. Both briefly review the early history (to 1980) of the debate between those who have concluded that iron metallurgy diffused to sub-Saharan West Africa from Phoenician North Africa (Mauny, van der Merwe) and those who have argued for independent invention (Lhote, Diop)[6], but this review is merely background to Bocoum's main concerns. These are to refute the two major objections that have been raised about claims for independent invention of iron smelting in West Africa. His first target is my suggestion that that some radiocarbon dates from Niger may be misleading because early metallurgists may have used old wood or old charcoal for fuel. Bocoum argues that since the available radiocarbon dates from ceramic temper and from charcoal on surface sites around Termit and Egaro agree, there is no "old wood" problem in Niger, and thus the chronology of metallurgy proposed for the Agadez region by Grébénart should be reinstated (UN, p. 96; MA, pp. 239).[7] If there is no "old wood" problem, he argues, then the best way to determine when iron working began in Africa is by statistical examination of the whole corpus of radiocarbon dates associated with iron objects or evidence of iron working (UN, p. 99).

Bocoum's second aim is to explain how the smelting of iron could have developed in Niger within a neolithic context. Person and Quéchon (whose training, like that of Grébénart, is in paleolithic archaeology) offer no explanation at all, but Bocoum has sufficient knowledge of archaeometallurgy to understand that some plausible explanation must be offered. His solution is to reinstate Grébénart's "Cuivre 1", and with it Grébénart's proposal that copper smelting provides a technological bridge in Niger between the open firing of ceramics and the reduction of iron ores using furnaces and forced-air blast (MA, pp. 241-242; UN, pp. 99-100).

Evaluating claims for an independent invention of metallurgy in sub-Saharan Africa

Since van Grunderbeek and collaborators are now reluctant to accept the earliest dates for iron working in Rwanda and Burundi, the argument for an independent invention of iron working in sub-Saharan Africa

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6 The chapter by Joseph Fazing Jemkur in the UNESCO volume provides a fuller history of this debate, but does not register any of the publications on iron working at Carthage that appeared during the 1990's.

7 Bocoum does however accept my argument that some of the supposed furnaces in Grébénart's "Cuivre 1" are in fact burned-out tree stumps (UN, p. 95, footnote 2).

hinges upon the evidence from Niger. How conclusive is this evidence? It should be noted that the radiocarbon dates for Termit were originally published a decade ago (Paris *et al.* 1992), and that no new data has been presented since then. The set of chapters arguing for independent invention in Niger should therefore be seen as a renewed effort to convince those who were not persuaded the first time around.

The major objection to the claims for Termit is still the lack of proof that the radiocarbon samples are truly associated with iron and copper samples. Unfortunately all of the Termit and Egaro sites except the smelting site Do Dimmi 15 have been deflated by the action of wind on bare soil, so that the sherds, charcoal and metal samples now lie together on the ground surface. Quéchon argues that each of these sites was originally a single component (UN, p. 113), but his more detailed arguments for this position are still not persuasive. I have already above noted that the agreement between radiocarbon dates on both charcoal and ceramic temper from the same scatters (UN, p. 110; MA, p. 251) is not as close as Quéchon states; in only two of six cases are all the dates from a site within two standard deviations of each other. No site has more than one date on pottery temper or more than three dates in total.

Quéchon's second argument for treating these surface scatters as single component sites is that the metal objects are always found with the same range of pottery types. The scarcity of illustrations of pottery from these sites makes it impossible to check this claim, but even if true it does not prove that these are single component sites. There is another plausible explanation. By about 4000 BP the Termit region was very arid, and there would have been few sources of water. These would have been the locations at which broken pottery would have accumulated, and would have been visited repeatedly over time, but the pottery broken on these visits has since been scrambled together by deflation. This would explain why the paired pottery and charcoal dates are in agreement at some sites but not at others.

It is unfortunately impossible to distinguish between these possibilities from the available data, but there is no necessary association between the metal objects, the dated sherds and the charcoal samples in surface scatters.[8] Metal objects can only be conclusively dated by radiocarbon when: (1) they are found with short-lived carbon samples (annual plants, or organic temper in pottery) in undisturbed stratified contexts, none of which have yet been discovered at Termit or Egaro; or (2) in the case of iron objects, when they are directly dated by AMS radiocarbon dating. The second option would only be feasible if these samples still contain small patches of steel, but has been successfully performed in much more humid environments than that at Termit (e.g. Kusimba *et al.* 1994). It would be worth attempting on the samples from Termit.[9]

There are other problems with claims for independent invention of metallurgy in Niger by 3300 BP. Let us suppose that the earliest iron objects recovered at Termit and Egaro are indeed of this age. If the objects were smelted from ore, as Quéchon, Person and their supporters assume, where is the evidence for their production? Iron slag and fired furnace ceramics are virtually indestructible, and would be difficult to miss in this barren and deflated landscape. Yet the only evidence of smelting or forging recovered at Termit or Egaro is a crescent-shaped array of 22 very closely spaced furnace bottoms at Do Dimmi 15 (UN, Photo 73; MA, Plate VIII:2) from which dates of 2630 ± 120 BP, 2500 ± 70 BP and 2065 ± 60 BP were obtained.[10] And if iron was really smelted at Termit by 3300 BP, why is there no radiocarbon evidence for iron or iron working before 2500 BP in the many excavated sites to the south and west of Agadez (Grébénart 1985: 263-348)? These sites are only 300-450 km west of Termit.

Bocoum's argument for even earlier copper metallurgy in the Agadez region is equally problematic. He asserts that the ceramic temper dates from Termit show that the radiocarbon date (on charcoal) of 3650 ± 50 BP from Afunfun 175 Furnace 8 is not an "old-wood" date, and thus that iron technology in Niger developed out of a prior copper working technology. But the dates from Termit cannot prove that any date, or set of dates, from the Agadez region is acceptable or unacceptable;

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8 A recent review of the archaeology of Niger reaches a similar conclusion (Haour 2003: 217), as does a extensive analysis by Susan McIntosh (in press) of the claims for early metallurgy at Termit.

9 Cook *et al.* (2003) have shown that even steel that appears to be totally corroded may yield reliable radiocarbon dates, as the cementite (iron carbide) phase of steel is much more resistant to corrosion than the low-carbon (ferritic) phase.

10 The spacing and similarity of the furnace bases suggests to me that they the product of a single group of metalworkers and were produced in a short span of time, perhaps a single year. It is possible that the older dates reflect some "old wood" effect, so it is unfortunate that thermoluminescence dating was not attempted.

each piece of evidence must be evaluated on its own merits. Although fourteen of the eighteen fired clay structures excavated at Afunfun 175 appear to be burned-out tree stumps or fallen trunks (Killick *et al.* 1988) – an interpretation that Bocoum accepts - Furnace 8 is certainly an artificial furnace or firepit, as are Furnace 3, which gave a date on charcoal of 3680 ±50 BP, and the undated Furnaces 6 and 18 (Grébénart 1985: 113-160). The reported chemical compositions of the "slags" excavated from these structures (Grébénart 1985: 158-160; Killick *et al.* 1988) show that almost all are baked soil, but two samples (one each from Furnaces 6 and 8) contain more than 1% copper and are certainly evidence of some form of copper metallurgy.

So far so good, but when one broadens the focus from Furnace 3 to the site as a whole, the picture begins to blur. The radiocarbon dates on Furnaces 3 and 8 are both within one standard deviation of those on adjacent "furnaces" (2, 4, 5, 7 and 12) that are clearly fired earth casts around former tree stumps or trunks. There is also evidence of later metallurgical activity on this site. Furnace 6 yielded a nodule of tin bronze (15% Sn), while Furnace 1 (almost certainly a burned-out tree base, with three radiocarbon dates between 4140 ± 90 BP and 3680 ± 60 BP) contained several pieces of iron slag, one of which was directly dated to 1500 BP from an entrapped piece of charcoal (Killick *et al.* 1988).

With this additional information in hand, the most likely site history for Afunfun 175 is as follows:

(1) A small forest once grew on this site, but burned at some time after 3100 ± 70 BP (the youngest radiocarbon date obtained by Grébénart on a fired clay structure). Combustion of some bases and fallen trunks was incomplete, leaving variable amounts of charcoal within the fired clay exterior casts of the former trees.

(2) Later in time, metal-using groups visited the site and constructed firepits and furnaces to work copper and iron. At least some of these activities made use of charcoal from the burned-out trees. One of these visits took place around 1500 BP, during which one of the burned-out stumps served as a dump for some iron slag. The timing of the other visits cannot be established by radiocarbon dating, as the radiocarbon age of the samples is that of the trees that furnished the charcoal - not the age of the furnace or firepit.[11]

In spite of the confident assertions of Quéchon, Person, and Bocoum, there is therefore no proof that iron was independently invented in Niger, or anywhere else in sub-Saharan West Africa. It is equally true there is no proof that iron smelting was introduced from anywhere else. We still lack conclusive evidence for either position. There are however a couple of lessons that we can learn from this review. One is that attempts to infer the place and time of the earliest metalworking by graphical or statistical manipulation of all available radiocarbon dates for metalworking are absolutely useless. Nothing is gained by analysis of data of dubious quality. Some archaeologists in Europe are developing numerical systems for ranking radiocarbon dates, using such criteria as reliability of association, sample material and precision (Pettit *et al.* 2003). African archaeologists would be wise to follow this lead, and to use only those dates of the highest rank in arguments about origins. None of the earliest dates that we currently possess for metallurgy from Rwanda/Burundi or Niger would qualify for high ranking.

But even when we do find samples of carbonized annual plants in indisputable stratigraphic association with the metals or metalworking debris, we will not be able to avoid the flattening of the radiocarbon calibration curve in the mid-first millennium BC. ***Table 1*** and ***Figure 1*** both show the calibrated age ranges at two sigma (95.4% probability of inclusion) for dates of fairly good precision from 2200 BP to 2800 BP. Note that from 2300 BP through 2600 BP the calibrated age range is never less than 300 calendar years, and may be as much as 500 years. Almost all of the earliest radiocarbon dates for the earliest iron metallurgy in Rwanda/Burundi, Buhaya, Congo, Central African Republic, Gabon and Cameroon fall in this range, and many of the reported dates older than 2600 BP extend throughout this interval after calibration because they have large standard deviations (>100 radiocarbon years).[12]

The inescapable conclusion is that we will never be able to trace the spread of iron working through West, Central and north-eastern Africa by radiocarbon dating. Unfortunately the only alternative methods -

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[11] The ages of these furnaces and firepits could be established directly by thermoluminescence dating of fired clay, but this technique was not employed by either Grébénart or Quéchon in their respective research projects.

[12] For example, the much cited date from Do Dimmi 15 in Termit of 2630 ± 120 BP calibrates at two sigma to a calendar range from 1050 cal BC to 400 cal BC.

Radiocarbon Age	Calibrated Age Range (2 sigma)
2200±50 BP	390-110 cal BC
2250±50 BP	400-190 cal BC
2300±50 BP	510-200 cal BC
2350±50 BP	800-350 cal BC (88.0%), 300-200 cal BC (7.4%)
2400±50 BP	770-610 cal BC (28.6%), 600-390 cal BC (66.8%)
2450±50 BP	770-400 cal BC
2500±50 BP	800-480 cal BC (87.7%), 470-410 cal BC (7.7%)
2550±50 BP	820-510 cal BC (94.4%), 440-410 cal BC (1.0%)
2600±50 BP	900-870 cal BC (1.8%), 840-750 cal BC (68.4%), 720-540 cal BC (32.2%)
2650±50 BP	920-760 cal BC
2700±50 BP	980-950 cal BC (2.4%), 940-790 BC (93.0%)
2750±50 BP	1000-810 cal BC
2800±50 BP	1130-820 cal BC

Tab. 1. Hypothetical radiocarbon dates from 2200±50 BP to 2800±50 BP, calibrated at two standard deviations (95.4%) with OXCAL 3.9 (Bronk-Ramsay 2003). Where the calibrated ranges are discontinuous, the probability for each portion is listed, with the probability of all portions summing to 95.4%.

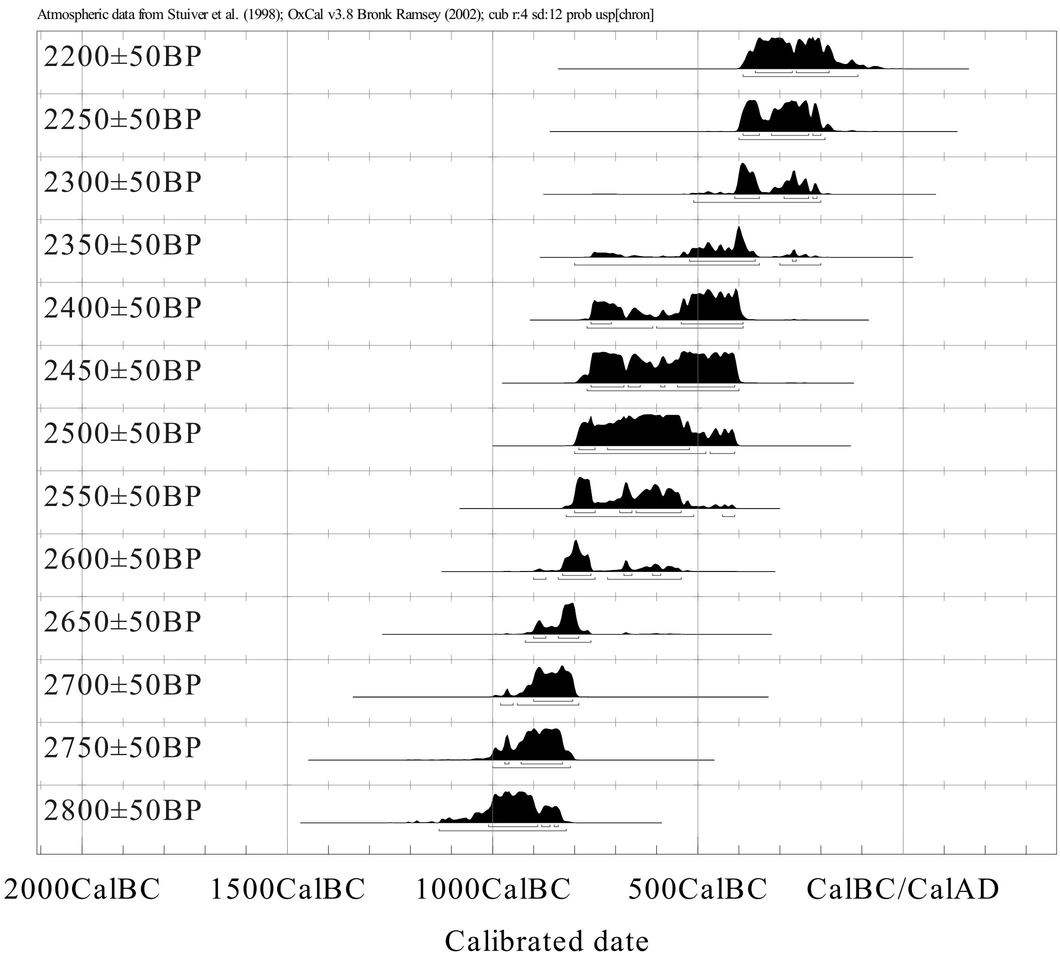

Fig. 1. text

thermoluminescence (TL) and optically-stimulated luminescence (OSL) – are at present no more precise than radiocarbon in this time range (GODFREY-SMITH & CASEY 2003).[13]

Does this mean that we can never know whether the knowledge of iron working was independently developed in sub-Saharan Africa, or whether it spread from elsewhere? This is not necessarily the case. We could still find evidence of iron in excellent stratigraphic association with short-lived radiocarbon samples, precisely dated to before 2600 BP, though at present there is not a single case that satisfies all these conditions. But even if iron working did spread through the northern half of sub-Saharan Africa during the radiocarbon "black hole" between 2300 BP and 2600 BP, we may still be able to track its spread if we can make use of information other than radiocarbon dates. Even if we can never know the precise absolute ages of sites in this interval, we may still be able to infer relative ages from the associated pottery. This of course will require that detailed sequences of ceramic styles be established in each area by excavation and patient analysis of pottery from deeply stratified sites, as Graham Connah did at Daima, as Susan McIntosh has done in the middle Niger and middle Senegal River valleys, and as the Frankfurt team are doing in Burkina Faso and northern Nigeria. Francophone archaeologists in sub-Saharan Africa have shown little interest in constructing ceramic sequences, but they will have to do so from now on if they wish to understand the archaeology of the first millennium BC.

We may also be able draw some inferences from the comparative study of iron smelting technology. As noted above, there is now firm proof of Phoenician iron working at Carthage and in the Iberian Peninsula by at least the eighth century BC. We do not yet know whether the seventh/sixth century Phoenician smelting furnace from Toscanos, Spain (illustrated by Niemeyer in MA, p.87, Figure 3) is typical, but it is clearly very different from the oldest known iron smelting technology in sub-Saharan Africa. Almost all published iron smelting furnaces of the first millennium cal BC from Rwanda/Burundi, Buhaya, Nigeria, Niger, Cameroon, Congo, Central African Republic and Gabon are slag-pit furnaces, which are so far unknown from this or earlier periods in the Middle East or North Africa. Early Phoenician tuyères, which have square profiles enclosing two parallel (early) or converging (later) narrow bores are also quite unlike those described for early sites in sub-Saharan Africa, which are cylindrical with a single and larger bore. While this comparison is hardly conclusive, it does suggest that the earliest Phoenican technology was probably not the model for the earliest iron working in sub-Saharan West Africa. We cannot yet make comparisons with other potential source areas for lack of data. These include the Arabian Peninsula, pre-Axumite Eritrea, Ethiopia, and Greek colonies in North Africa. Nor is anything known about early iron working technology in the southern Sudan, which may yet prove to be earlier than that in Nubia.

Later prehistory, history and ethnography of African iron working

The UNESCO volume also includes several regional reviews of research on later iron working in West Africa, all in French. David Aremu's chapter provides an inventory of known iron working sites in Nigeria, followed by a synthesis of historic iron working procedures derived from his own oral histories, and a summary of the rituals dedicated to the deity Ogun (god of fire and of iron) in Yoruba-speaking regions. This is complemented by a short chapter by Isaac Adeagbo Akinjogbin on the recent history of Yoruba iron working. Joseph-Marie Essomba's chapter, reprinted from an earlier work, summarizes archaeological and oral historical studies of iron working in central Cameroon. Those who have dismissed the possibility of an "old wood" offset for radiocarbon dating in the more humid areas of Africa (e.g. DE MARET & THIRY 1996) should examine the twelve dates obtained by Essomba on wood charcoal from a single furnace at Oliga (UN, pp. 137-138). These range from 1860 ± 70 BP to 2820 ± 70 BP!

Some of the richest and most insightful studies of recent African iron working have been written by the anthropologist Bruno Martinelli, who has for more than thirty years been blending ethnography, oral history and archaeology to recreate the social and economic roles of iron in west African societies. His chapter in the UNESCO volume (pp. 165-188) is a superb study of specialization in iron working within the Yatenga kingdom, which extended from the Bandiagara escarpment of Mali, east of Mopti, into the plains of modern Burkina Faso. Martinelli notes the emergence, probably in the

[13] Recent developments in OSL dating promise much better precision in the near future, but will require *in-situ* measurement of radioactive dose rates for each sample by gamma-ray spectrometry (Jean-Luc Schwenninger, personal communication, February 2004).

18th century, of an endogamous occupational caste in the center of Yatenga dedicated to full-time specialization in the production of steel, which their kin traded to the neighbouring Dogon, and as far away as modern Ghana.

These steel bars were, Martinelli asserts, welded by blacksmiths in these various regions to their own locally-forged iron, forming composite tools with steel cutting edges (*tranchants*). To make this steel, the Yatenga ironworkers developed the tallest furnaces known in Africa, varying from five to six and a half meters in height. These were charged with about 2000 kg of charcoal and ore, and smelted very slowly by natural draft to produced 150-200 kg of bloom per charge over a period of five to seven days. Former iron workers built a furnace of this type for Martinelli and smelted two charges, one for three days and the other for four, each of which is fully documented. It has been already been established that the long slow combustion in natural draft furnaces favors the production of steel blooms (Killick 1991), but this is the first documentation of a specialist industry in sub-Saharan Africa dedicated to the production of steel. Unfortunately Martinelli's fine field work does not appear to have been followed by full metallurgical study of the smelting process and products. If blacksmiths throughout the region were indeed routinely welding steel edges to iron tools, this would be the first documented case of this technique in sub-Saharan Africa. This needs to be confirmed by metallographic analysis of tools from archaeological and historic contexts.

Martinelli's chapter is nicely complemented by a reconstruction of smelting by natural draft among the neighboring Dogon. Eric Huysecom provides a summary in MA (pp. 73-82), with colour plates, and has also made an excellent film (Huysecom & Agostini 1996) that shows the whole process from the digging of a 17.5 m shaft to extract the ore, the rebuilding of a ruined furnace, and the smelting process itself. The natural draft furnace was about 2.3m high and consumed a charge of about 460 kg of charcoal[14] and ore in 40 hours to produce a bloom of about 70 kg. Temperatures were measured, but it appears that the equipment was not working correctly, as the peak temperature reported when slag was flowing (1021°C) is below the melting point of any possible mixture of iron oxide, silica and alumina![15] Metallographic analysis of the products showed, not surprisingly, that the bloom contained a generous proportion of steel. Evidently, the Yatenga specialists did not have a monopoly on steel production in this region, as Martinelli seems to imply, but they do seem to have manufactured it on an unusually large scale.

Most of the papers reviewed here are the product of Francophone scholarship. The special issue of *Mediterranean Archaeology* does however include three reflections by senior Anglophone scholars, each of whom has made major contributions to our current understanding of African metallurgy in all its aspects – technological, economic, social and conceptual. In her chapter, the historian Eugenia Herbert argues that "...the preoccupation with dates and origins obscures questions of greater historical interest" (p. 42). As examples of more interesting work, she focuses upon two recent studies – *Métallurgie et politique en Afrique centrale* (Dupré & Pinçon 1997) and *Iron Technology in East Africa* (Schmidt 1997). Both are given a sympathetic but critical reading, concentrating on whether the evidence can bear the weight of the ambitious social and political models employed in each case. While she salutes these scholars for asking the right questions, she laments that "(p)erhaps it will never be possible to write a history of African metallurgy that satisfies the historian's inordinate greed for both generalization and specificity" (p. 48). Perhaps not, but the same can be said for almost any work of precolonial African history. Is Jan Vansina's *Paths in the Rainforest* really richer in evidence than the work of anthropologists like Martinelli, Schmidt or Nicholas David on African metallurgy? I think that the reverse may be true. Each of these anthropologists employs all of the evidence and the techniques of the historian of precolonial Africa (written documents, oral histories, structural analysis, linguistic evidence), but they are also able to draw upon archaeological and scientific evidence, which very few African historians are competent to evaluate.

14 The video shows that this charcoal was produced by carbonizing an entire standing dead *Prosopis africana*, with a basal diameter of more than one meter. If samples of the charcoal have been retained, it would be most instructive to run a series of radiocarbon dates on these, and to publish the distribution of dates obtained.

15 Osborn & Muan (1960, plates 6 and 9). The actual temperatures were probably similar to those obtained by Martinelli (UN, p. 182). His graph shows temperatures above 1400°C at tuyère level while slag was flowing from the furnace.

Nicholas David's long chapter revisits many earlier studies of African iron working in search of an appropriate balance between what he labels "naturalist" and "antinaturalist" approaches. Broadly speaking, the former have been more interested in documenting the technology and economics of African iron working, while the latter have concerned themselves more with symbolism and ideology – with what the words and gestures of iron-workers may reveal about their concepts of social order and disorder. None of the works that he surveys, including my own, have in his opinion managed to get the balance between these approaches quite right, though he suggests that those of Schmidt, Avery and Childs in Buhaya come closest. (He could also have mentioned his own, but modesty prevailed). How should scholars of African iron working reconcile their naturalist and antinaturalist tendencies? David suggests (as Alain Gallay does, quite independently, in his chapter) that the conceptual framework of the French "Anthropologie de Techniques" school is perhaps the best available at present – a proposal with which I am in full agreement.

Peter Schmidt's contribution to MA is aimed as much at historians of Europe and of the Middle East as at Africanists. He argues that the European passion for classification has distorted our understanding of the history of technology in Africa. All African iron working processes have been lumped together as variants of the "bloomery process", thus assigning all African smelting processes to a rung near the base of a global evolutionary ladder of metallurgical progress. In fact, as Schmidt notes, students of African metallurgy have documented an amazing variety of processes, many with no known counterparts on other continents – "a spectrum of variation of such diversity as to suggest that the term "bloomery" no longer does justice" to the range of evidence (p. 220). Earlier writers, blinded by their urge to classify, managed to miss most of the evidence for innovation in African metallurgical technology. This was an error that Schmidt himself has done much to correct. His is a profound critique, and one with which a growing number of archaeometallurgists worldwide would tend to agree. Schmidt goes on to argue that continuous innovation was the normal state in African iron smelting, with each iron worker improvising off a preexisting repertoire of techniques – much, I suppose, like a jazz musician improvising off a standard melody. I'm not sure that I agree with this latter point; iron working can succeed only within a very narrow window of temperature and gas composition, which tends to impose rather strict limits upon individual departures from a successful process.

Archaeometallurgy

We owe much of our current knowledge of the variety of African iron smelting techniques to interdisciplinary collaboration, in both the field and the laboratory, between anthropologists or archaeologists on the one hand, and metallurgists or geologists on the other. Chemical or metallographic analyses of African iron and iron smelting residues had been undertaken intermittently since 1904, but the analysts had, in most cases, no appreciation of the cultural context of the samples. They produced technical reports that the archaeologists or anthropologists could not understand, and thus were usually reproduced verbatim as appendices. Collaboration in the field, whether in ethnographic or archaeological context, was a crucial step forward. When these first began, from the late 1960's (Nikolaas van der Merwe with Revil Mason at Phalaborwa, Ron Tylecote with Bernard Fagg at Nok, Don Avery with Peter Schmidt in Buhaya) there was much mutual misunderstanding of aims and methods. But each learned from the other, and the result was the birth of archaeometallurgy as an area of specialization.[16]

There are too many areas of specialization in archaeology for any one person to master them all, so specialists owe generalists an explanation, in non-technical language, of what they know and how they know it. Each of these volumes contains an exceptionally clear and informative essay on the archaeometallurgy of iron working. Any archaeologist who has ever wondered how to distinguish smelting slags from forging slags, or wrought iron from steel, should obtain a copy of Philippe Fluzin's excellent chapter in the UNESCO volume. This is illustrated with about thirty color photographs and is by itself worth the cost of the book. I hope that the English translation of this volume, promised for 2002, will eventually appear so that Anglophone archaeologists can also have the benefit of Fluzin's experience. The *Mediterranean Archaeology* volume contains a chapter, in French, co-authored by Fluzin with the Swiss archaeometallurgist Vincent Serneels. This concentrates more upon the interpretation of slags than does the UN chapter, reflecting Serneels' special expertise in this area, but unfortunately is much less useful to archaeologists because it provides only a single photographic plate.

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16 I do not mean to imply that archaeometallurgy developed solely in Africa; similar developments were taking place at the same time in Europe, the Soviet bloc, the Near East and South America.

The present state and future prospects of research on African iron working

The two volumes reviewed here complement each other to a substantial extent, and together provide a very useful snapshot of the present state of research of African iron working. The volume edited by Bocoum will undoubtedly be more widely read in Africa because it is distributed by UNESCO, is attractively produced with abundant illustrations (many in color) and is inexpensive. The less accessible special issue of *Mediterranean Archaeology* is however the more useful for African archaeologists because it covers almost all of the continent, includes much comparative material from beyond Africa, and contains the valuable overviews by Gallay, Herbert, David and Schmidt. The UNESCO volume suffers in comparison because many of its chapters are recycled from previous publications or were written in the mid-1990's, so some of the information given – particularly that relating to the Phoenicians - is out of date. The bibliography in the UNESCO volume is also unreliable, with many incomplete or otherwise inaccurate citations, particularly for works originally published in English.

The central theme of all of the products directly created by the UNESCO project "Les Routes du Fer en Afrique" – the book edited by Bocoum, the Paris exhibition and the web site – is that it there is definite proof that iron working was independently invented in West Africa by 3300 BP. I have argued here that the supporting evidence falls far short of proof. These claims rest entirely upon interpretation of radiocarbon dates, and in all cases there are potential problems with the radiocarbon evidence. In some there may be an "old wood" problem and in others there is no proof that the metal objects are of the same age as the dated pottery sherds that lay with them on deflated surfaces. Furthermore, some of the Francophone archaeologists of Africa who have contributed chapters to the UN and MA volumes do not appear to fully understand how to interpret calibrated radiocarbon dates. Calibrated dates are ranges of calendar ages, and there is absolutely no scientific justification for selecting the older end of the range as the "real" date.

In summary, I have suggested that the quality of the evidence is so poor that we cannot yet establish when iron working began in sub-Saharan Africa. Statistical manipulation of the current corpus of radiocarbon dates for the earliest metallurgy is absolutely useless – as statisticians are fond of saying, garbage in means garbage out. We must accept the limitations of the evidence, and keep searching until we find short-lived carbon samples in undeniable stratigraphic association with certified iron working debris. Even then we must respect the inherent limitations of radiocarbon calibration. Some portions of the calibration curve yield narrower intervals of calendar age, some yield wider ranges, and if our radiocarbon dates fall between 2350 and 2600 BP, we must simply accept that we can do no better than a 300-year range of calendar age.

Meanwhile there are more interesting issues to be investigated. The publication of these volumes effectively marks the end of the era of ethnographic and oral historical studies of African iron working, since almost all of the potential informants are now dead.[17] Studies of the symbolic and conceptual frameworks of African iron working, which have been such a prominent feature of the literature of the last thirty years, will not completely disappear, but will be largely restricted to reworking the case study material that has already been collected. From this point on almost all studies of African iron working will be archaeological.

The main thrust of future archaeological work should, I suggest, be directed towards reconstructing economic aspects of past African iron working. For any given region, like Meroe or the Senegal River valley, we need to reconstruct how much iron was produced, by what technical processes, over what period of time, how production was organized, where the product went, and how it was consumed. These are the answers that will be of most value in the long run to African archaeology and African history. Work on the origins and spread of African iron working will and should continue, but is likely to be limited by our inability to date sites with useful precision in some portions of the radiocarbon calibration curve.

All of these tasks require that archaeologists be able to work closely with specialists in dating techniques and in archaeometallurgy. A major failure of the UNESCO project is that it appears to have invested little effort in identifying priorities for future research, or in assessing the resources and technical facilities that would be required. The facts of the matter are that very few archaeologists who live in Africa have access

[17] The only region of the African continent where iron is still being smelted by indigenous techniques is in the very remote Dime territories west of the Omo River in Ethiopia (R. Haaland, personal communication).

to the technical assistance that they would need to undertake research on either the origins or the economics of iron working, and that the situation is deteriorating. The reopening of the Dakar laboratory has doubled the number of radiocarbon facilities in sub-Saharan Africa, but otherwise the picture is bleak. Several African students have done postgraduate theses in archaeometallurgy at European universities, but have been unable to make use of their training after returning home for lack of access to funding and technical facilities. The only specialist archaeometallurgy laboratory in sub-Saharan Africa, at the University of Cape Town, has been crippled in a thoughtless act of academic sabotage.[18] In retrospect, UNESCO's money would have been better spent in Africa, upon assessment of research infrastructure, than in funding an expensive meeting and exhibition in Paris.

Nor is the picture much brighter overseas. Many of the leading scholars in this field have retired, or are close to retirement, and there are few students in training who could replace them. Since most research on African iron working will, for the foreseeable future, be conducted with foreign funding, the scarcity of trainees in Europe and America should be a cause for concern. The years between 1965 and 2000 will be seen in retrospect as the Golden Age for studies of African iron working, and many of the accomplishments of this era are cited in the volumes reviewed here. What will the next thirty-five years bring? From the present vantage point, I fear that I identify with the pessimism of the ancient Greek writer Hesiod (8th or 7th century BC), who, in looking back to the glorious past ages of gold, silver and bronze, lamented that he himself should have to live in the age of iron.

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[18] Throughout the 1990's the director, Duncan Miller, raised all salary and research funding for the laboratory by submitting proposals directly to private foundations. The University of Cape Town ruled in 2000 that all proposals should be submitted by the University, not by individuals, and has since 2002 declined to submit any proposals on Dr. Miller's behalf.

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