Book Review

A. Uesugi (ed.) *Iron Age in South Asia*. B. Research Group for South Asian Archaeology, Archaeological Research Institute, Kansai University, Osaka, 2018 [ISBN 978-4-9909150-1-8]

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Introduction

More than 20 years ago, Gregory L. Possehl and Praavena Gullapalli, in an important review essay entitled "The Early Iron Age in South Asia" (1999: 153), part of an excellent general comparative volume on the archaeometallurgy of southern Eurasia (Piggot 1999) wrote what follows: "[...] regional manifestations [*like the late Bronze age cultures of Swat, the Painted Grey Ware, the Pirak assemblage, and the Megalithic complex: note by the authors*] are seen as possible outgrowths of a series of local Bronze Age traditions that seemed to have an awareness of iron. An adequate understanding of the technological processes involved in the production of early iron will yeld much information regarding the transition to the Iron Age, but such an understanding has yet to be reached".

These somehow prophetic words came to our mind when we read a recent contribution by Akinori Uesugi published in 2018. Uesugi's rare attempt of combining holistically disparate source of evidence such as early iron production, beads and trade, terracotta figurines, urban patterns and variations of distinctive (if notoriously quite broad) ceramic classes, is quite commendable, as are the beautiful illustrations of his paper. A. Uesugi is a very active colleague, who was always extremely kind, and ready to exchange ideas and materials with his colleagues, including us. Some of the Pakistani colleagues, especially at the Peshawar University, will also remember him as one of the members of the team led by Prof. H. Kondo of Tokai University during the 2004-2005 joint fieldwork in the Gomal Plain. In following years, Uesugi extensively contributed to important archaeological research in the crucial site of Farmana, in Haryana.

The contribution in question is part of an edited e-book *Iron Age in South Asia* edited by the same A. Uesugi, which is the second volume of the «South Asian Archaeology Series» published in 2018 by the Research Group for South Asian

Archaeology of the Archaeological Research Institute of the Kansai University (Japan). The volumes of the series are all available in open access on academia.edu.[1]

The contribution (or chapter) by A. Useugi is the first of that volume, and it is titled "An Overview on the Iron Age in South Asia" (pp. 1-49). This is followed by five chapters, four of which, dedicated to the earliest evidence of iron metallurgy in specific geographical regions (North India, Central India, South India: Telangana and Andhra Pradesh, South India: Kerala) are authored by scholars from different Indian institutions.[2] The last contribution "On Base metals in Vedic Culture" is by Dr. T. Yamada (Osaka University).

The e-book is well composed, wonderfully illustrated, definitely a useful volume, and a necessary reading for all those interested in the crucial and debated matter of the introduction of early iron metallurgy in South Asia. There are some points of disagreement, though. Let us proceed point by point, moving from general to particular issues.

General issues

The first point is that there is no need of re-confuting the idea that iron technologies were brought to the Indo-Pakistani Subcontinent from abroad, quoting old outdated works such as Gordon 1950, Wheeler 1959 or Banerjee 1965, as Uesugi does at pages 1-2.[3] Nobody believes anymore that Bronze age smiths of the lands of contemporary Pakistan and India died waiting for the superior input of Achaemenid craftsmen to learn from them about iron. The independent development of proto-historical iron metallurgy of the Subcontinent, luckily enough, is beyond question: the point has been clearly stated since long, as everybody can see from the above quote.

Another important terminological/chronological issue of disagreement is the use, in the author's text and chronological tables of figs. 1 and 6, of a 'North Indian Iron Age' divided in six arbitrary chronological blocks (Period I = 1300-1000 BC; II = 1000-500 BC; III = 500-250 BC; IV = 250-0 BC; V = 0-300 AD; VI = 300-600 AD). In our opinion, such last-day expedient labeling might further blur the already fluid and still controversial periodization of the late prehistoric/early historic periodization of Punjab and Khyber Pukhtunkhwa, with little general gain.

[1] The first volume of the series was *Excavations at Madina, District Rohtak, Haryana, India* (2016; ed. by M. Kumar, A. Uesugi, V. Dangi). The third volume was *A Study of Animal Utilization Strategies from Early to Late Harappan Periods in Haryana* (2017; ed. by A. Uesugi). The fourth volume of the series (2018) is *Current Research on Indus Archaeology* (ed. by A. Uesugi).

[2] In order: Prof. (Asst) Vivek Dangi, Department of History, All India Jat Heroes' Memorial College, Rohtak, Haryana; Dr V.G. Sontakke, Assistant Director, Department of Archaeology, Nagpur, Maharashtra; Prof. K.P. Rao, Professor, Department of History, University of Hyderabad, Hyderabad, Telangana; Prof. (Asst) Abhayan G.S. [*sic*], Department of Archaeology, University of Kerala, Thiruvananthapuram, Kerala.

[3] See Kosambi 1963 (on the basis of written sources); the same point was raised by the authority of Chakrabarti 1977; and then, again and again, by Sahi 1980; Tripathi 2001; Tewari 2010; see also Prokop and Suliga 2013; Yatoo 2015, and also the broader discussion in Gullapalli 2009: 440-442.

A third issue is related to the very label of 'Iron Age'. One thing is to use it as a conventional term in the prehistory [of Europe and the Mediterranean], to the indicate the age immediately after a specific *terminus post quem* which is conventionally seen as the introduction of iron metallurgy. Another thing is to use it to define the historical eras throughout which iron was extensively in use, as it were their peculiar marker. In that sense we should write a history per "mega-epochs" where that "Iron Age" is finally succeeded by a "Plastic Age" starting around the 1930s. If Iron Age is used in the sense of Uesugi, we should talk of Iron Age when dealing with the complexity of the economic production of the mid-2nd century CE Roman and Parthian empires, or the elaborated diplomatic relationship between the Byzantines, the Sasanians, the emerging Arab power and the early Slavic states. This is actually what is proposed here, when 'Iron Age Period VI' would describe the post-Kushan times and the *grandeur* of the Gupta cultural period. We witnessed something similar also in Iran, where archaeologists dealing with the phases corresponding to the times of the Achaemenid Empire, used to talk of a local "Iron Age". Maybe there is little sense in describing the Persian wars as a conflict between the two "Iron Age cultures": one of which, by the way, created the wonders of Persepolis, the other one the Acropolis of Athens and the Parthenon.

Taking into consideration Uesugi's 'Iron Age Period IV', this is nothing but a new label for the strictest interpretation of the "Early-Historic period" of the old South Asian historiography and archaeology. The complexity of the cultural differences and hybridization dynamics that occurred in the period marked by the "invisible visibility" of the Mauryas, the establishment of the Indo-Greek kingdoms, the monetary reforms of the Saka, cannot simply merged in one single period. In our excavations at Barikot we were trying to represent such complexity by avoiding even the obsolete term of "Early-Historic".

In fact, our temporary terminology follows and partly modifies the proposed sub-division of the "Early Historic" archaeological period in South Asia (Smith and Mohanty 2016: 688–689). The so far defined "proto-urban phase" in Swat (on the evidence from Barikot see below), overlaps the Swat Graves Complex (c. 1200–900 BCE), and conceptually corresponds to the "Regionalisation Era" of Coningham and Young 2015. The "initial urban phase" in Swat (500-200 BCE) can be associated to "the increased social complexity of cities and political entities (e.g. Mahājanapadas in Northern India)" (Smith and Mohanty 2016: 688) and is comparable to the term "Integration Era" (Coningham and Young 2015). Excavations at Barikot (see e.g. Olivieri and Iori 2020) prove that there is a real cultural/structural phase marked by cultural material associated to the Indo-Greeks (metrology, epigraphy, ceramics, technology, economy), then to the Saka princely states, and the establishment of the Kushana control and the expanded role of Buddhism.

Too much for a single term, too complex for associating all this only with "iron". Actually, if we were supposed to select a single word to indicate a material marker which - from the Northern Neolithic to the spread of Buddhism - really

reflected the complexity of two millennia of northern South-Asian history, that term rather than "iron", would definitely be "rice"!

Other few untenable labels and omissions

It is quite peculiar that an important synthesis on early iron in India and Pakistan like Possehl and Gullapalli 1999, with a list of calibrated radiocarbon measurements (in many instances critically discussed) was not even mentioned in Uesugi's summary.[4] But such absence gets a revealing light considering that the role of the so- and wrongly-called "Gandharan Grave Culture" in Possehl and Gullapalli's paper is discussed in three full pages (including the list of radiocarbon dates at p. 166), while in Uesugi's essay the same label only appears in form of dots on the corner of three maps (his fig. 7), never to be mentioned in the text. Swat and the nearby areas of the present-day Khyber Pukhtunkhwa, which had always been considered a nuclear area of early iron-working and metallurgic innovation (Chakrabarti 1977) simply has disappeared.[5]

"Gandharan Grave Culture" has never been an acceptable archaeological category. Once more, we repeat that the historical entity of Gandhara has no bearing for the late Bronze age/Iron Age cemeteries and settlement phases of the Swat, Buner, Dir and Chitral valleys and other regions of the lower Hindukush range, and attaching this label to an archaeology still under construction, in cultural regions which are largely unexplored and minimally published, is deeply misleading (Vidale and Micheli 2017; Zahir 2016).

Uesugi's map of fig. 4 summarizes a lot of information on the 'Distribution of major iron ore sources in South Asia and chronometric dates for the early iron or early Iron Age sites': graphically very appealing, it forms the core of the author's argument (the deep antiquity of the Subcontinent's transition to iron technologies), but it has its problems.[6] R. Tewari (2003, 2010), building upon a painstakingly progressing recognition of the real antiquity of iron working in India (among others, Singh 1962; Kosambi 1963; Hegde 1973; Ray and Chakrabarti 1975; Chakrabarti 1977; Bhardwaj 1979; Gaur 1981; Rajan 1991; Tripathi 2001, 2014) notoriously proposed a groundbreaking view on metallurgical innovation and the introduction of iron technology in continental India, hypothesizing a long and slow process rooted in the first half of the 2nd millennium BC and spreading with accelerating speed after c. 1300 BC.

[4] Even though now certainly outdated.

[5] Tewari 2010 dedicated a whole page of his paper in *Man and Environment* to support the noticeable antiquity of iron working at Gufkral, Charsadda and Pirak, and the Swat valley. More recently, J.P. Upadhyaya (2019) excludes the north-western territories from the Indo-Pakistani Subcontinent.

[6] Not less than 10 different early-iron sites (Lahuradeva, Abhaipur, Atkha, Malhar, Raja-Nala-Ka-Tila, Jushi, Mangalkot, Pandu Raja Dhibi, Eran, Ramapuram) are referred to "Tewari 2013", but there is no such article in the references, nor we found mentions of it in the web; it must be a typing error. The mentioned article presumably should be the latest update quoted as Tewari 2010 (see at page 49).

This picture and changes obviously have opened very intriguing possibilities of research. In fact, as stated by Johansen (2014: 258) "The discovery of an increasing number of iron production sites from the Ganges-Vindhya region of North India, radiocarbon dated to the early 2nd millennium B.C. suggests that South Indian ironworking traditions may have their origins in the exchange of goods and knowledge between northern and southern India during the mid 2nd millennium B.C." (so far, so good: even though Killick and Fenn 2012: 565, still, for caution, considered "controversial" the proposed evidence of iron working in India in the time frame 1800-1300 cal BC). In Uesugi's fig. 4 some of the reported dates are very early. For example, Nagaraja Rao 1971 is reported as the ground on which the earliest iron-bearing context of Hallur, Karnataka is dated to 2153-1640 cal BC. In contrast, in the text the same evidence (like in Tewari 2010: 83 and in fig. 1) is dated to c. 1200 BC; while still in 2003 R. Tewari in Tab. 1 had envisaged a range between the 14th and the late 9th centuries BC (see also Possehl and Rissman 1992). Similarly, at Brahmagiri, other early dates (between 2140 and 1940 cal BC) "obtained from wood collected by Wheeler", and published with great uncertainty in Morrison 2005, are reported without any comment. Even at Pirak, Baluchistan, the appearance of iron in Period III is dated *tout court* 1200 BC, while a more balanced evaluation of the stratigraphic and radiometric evidence rather suggested the 9th century BC (Possehl and Gullapalli 1999: 157; Kuz'mina 2007: 434; Chakrabarti 1977: 174). In the case of Gachbowli, Telangana, the author was more conservative and reported two dates obtained from pottery by the means of optical stimulated luminescence (OSL) - respectively 1975- 2315 BC (2145 BC) and 2605-2985 BC (2795 BC) - translating them, for some reason, as "2200 BC" (check Thomas et al. 2008).[7]

Such un-selective process resulted in pushing up the chronology of the onset of Indian iron metallurgy, maybe far beyond the objectivity of the data. By the way, most of the dates which appear in the map of his fig. 4 rather consistently seem to place a growing production and use of iron artifacts between the 15th and the 12th centuries BC (see also the dates for iron in megalithic South India in Tewari 2010: 84, and those obtained at Dadupur at p. 85; *contra*, Mandal 2009). It is in such framework, in evident course of consolidation, that the absence of the archaeological evidence we recently obtained in Swat makes a problem.

Early iron artifacts from the Swat valley

Most of the dates from Swat were presented (and preliminarily published as a poster) at the 14th International Congress on AMS Congress in Ottawa in August 2017 (Olivieri et al. 2019), and at the EASAA Conference in Naples in July 2018 (forth.). In detail, 26 samples of organics (94%: carbonized seeds) were taken from two

[7] In Thomas et al. 2008 does not appear the site-name of Gachbowli, but rather the acronym GLBD 3 and 4, reportedly "[...] pottery samples excavated from a Megalithic burial ground located within the campus of the University of Hyderabad". The authors further state that "Considering the scarcity of iron and absence of other metals in the burial pit and similarity of big pots to Neolithic types, the site appears to represent the early Megalithic phase and archaeologically it may be dated to BC 800 and BC 2000" (*ibid*.: 782).

nearby stratigraphic trenches and measured for 14C isotopic ratio determination by AMS at the CIRCE laboratory, Caserta (Italy) under the direction of Filippo Terrasi and his team. This series of absolute dates adds to the very substantial sequence of new 14C absolute dates obtained on human remains from the Swat late Bronze age graveyards excavated in the past (Silvi Antonini and Stacul 1972; Vidale et al. 2015, 2016; Vidale and Micheli 2017; Narasimhan et al. 2019, Supplementary materials).[8]

The solid framework thus obtained allows a new chronological definition of the earliest (at least, so far) appearance of iron in Swat. Fig. 1 illustrates some early iron objects from recent excavations (Trench BKG 12 W, near the Indo-Greek city wall, and Grave 19 of Udegram). The chronological range is between the 12th and the end of the 9th centuries BC.[9] The objects are a fragment of a curved sickle (Fig. 1,1), a well-shaped dagger with a short tang and central rounded rib (Fig. 1, 2), a piece of bangle (Fig. 1, 3), and three pins (Figs 1, 5-7). Other shapeless or badly preserved items are not illustrated. The dagger is quite similar to another iron dagger or knife blade found, together with another large iron axe-like tool, in the furnishings of a coeval grave urgently rescued in the 80s at Aligrama. The furnishings of this grave have been published in a previous issue of this Journal (Lant and Caldana 2019). Fig. 1, 5 is a perfect replica of a type of pin better known in copper and found in other graves of the same cemetery and chronological horizon (Vidale and Micheli 2017: 402).

The record of graveyards and settlements thus includes weapons, agricultural tools and personal ornaments, which gradually came to replicate their copper prototypes. G. Stacul had estimated that iron objects appeared in c. 7% of the graves he had excavated in Swat (1966: 60). In this light, Uesugi's statements that in the northern areas iron was exclusively utilitarian, while in southern India only it was deposited in graves as a mortuary offer (2018: 4), does not stand the test.

Thus, looking to Uesugi's fig. 2 ('Chronological developments of iron tools in North India') sickles, blades and flat axes should be moved backwards in time from c. 1000 BC to the upper threshold of the 12th century BC. The point is that the early iron objects of late Bronze age Swat witness a fully developed iron metallurgy (if apparently limited in scale of production) in the 12th century BC. This might give further credit to the quite early radiocarbon dates previously obtained for Gufkral in Kashmir (Sharma 1992; Possehl and Gullapalli 1999: Table 6.E; Yatoo 2015; Tewari 2010: 82).

New evidence from Swat and previous data from Kashmir, ultimately, at present question the old adage "Iron in inner India is earlier than that in the Indian borderlands" (Chakrabarti 1977: 183). More and more, the development of protohistoric metallurgy in the Subcontinent resembles a multi-focal, continental

[8] Uesugi's paper is dated 2018: Narasimham et al. was published in *Science* in September 2019, but a pre-print was availble on bioxRiv since March 2018 (doi: https://doi.org/10.1101/292581).

[9] This solves for good an old controversy - see D. P. Chakrabarti's (1977: 177) previous conclusion that "[...] All that one can in the present stage of knowledge is that the first iron in the graves falls somewhere in the first half of the first millennium".

process pulsating with an almost perfect simultaneity from the lower Hindukush central-southern peninsular India - and, at least from mid 2nd millennium BC onwards, with growing strength (thus retro-dating Uesugi's fig. 2 'Introduction phase').

The need of a new stage of archaeometallurgical studies

Finally, going back to the implications of Possehl and Gullapalli's initial quote, it is clear that an iron-based metallurgy so far scrutinized through the lens of archaeological craft indicators, in absence of new important analytical efforts, constantly runs the danger of serious misunderstandings.

To illustrate this, let us consider Uesugi's fig. 5, the drawings of two iron lumps from the excavations of the metalworking site of Mahet, Uttar Pradesh (c. 3rd century BC). According to the author, here were found "[...] shallow pits with burnt clay (and) a number of wrought iron that has profiles of vessels (crucibles) with a flat or round base and straight sides [...]. These examples quite apparently exhibit that this wrought iron was produced using crucible iron smelting technology" (2018: 3).

Perhaps this might not be entirely correct. Pre-industrial "wrought iron" is a refined, very low-carbon and non-molten bloomery product, which does not need any crucible[10], and whose surface carburization may have had an important role in the early spread of the metal (Erb-Satullo 2019: 577-580; for an example of well understood archaeological records of the bloomery smelting process in north-eastern Indian contexts of 2000 years ago, see Prokop and Suliga 2013). In our experience and for the reasons above, as far as one can judge from the forms of the artifacts (not very clearly understandable) the two pieces in Fig. 5 rather than blooms might be slag flowed in the bottom of some kind of smithing oven or furnace. On the other hand, the slag disk on top seems to have on the base (?) the round imprint of the inner base of a vessel. In such a case, could it be silicatic copper slag from a crucible smelting process? We might be wrong, but in absence of archaeometric evidence every interpretation is as possible as arbitrary.

Copper smelting slag, in many contexts, being formed to a great extent by complex iron silicates, are hardly distinguished on visual grounds from iron smelting and smithing slag (for example, in absence of chemical analyses, the items visible in

[10] We find particularly clear the following explanation: "Wrought iron has been used for thousands of years, although the methods of manufacturing it have changed. The process essentially involves heating iron and removing slag in order to achieve the correct composition. The wrought iron can then be reheated and hammered, rolled, or otherwise worked into various forms. Wrought iron was originally manufactured as charcoal iron, which was smelted directly from the iron ore in a furnace known as a bloomery. The ore was heated and separated from the slag, but it was not allowed to melt as this would allow carbon to dissolve into it, forming pig iron. The resulting wrought iron retained enough slag to be malleable, ductile and strong. However, the quality did vary, depending on the type of iron ore that was being used and how much slag was left in it at the end of the process. It could have very different levels of strength and corrosion resistance." (IGNR 2017).

Uesugi 2018: Fig. 5, lower right, could be iron or copper smelting tapping slag, or iron smithing byproducts - Bachmann 1982 *docet*). This would also question (or would put in another light) the hypothesis of a connection between similar materials records and the famous, later (?) crucible steel or "wootz" technology of southern India (among others, Srinivasan 2007, 2017; Srinivasan and Ranganathan; Lowe 1990).

In conclusion, Possehl and Gullapalli (1999: 153) were more than right: we need a quite superior knowledge of ancient Indian iron-working innovations, and without a minimum of investments in chemical and metallographic analysis, many wide-scope questions and interpretations (and the relative communications) will remain suspended and useless. On the field, the use of portable XRF machinery (Hunt and Speakman 2015) might provide a partial but handy solution to this problem. In laboratory, dedicated analytical investigations like those carried out on materials from Guttur, Kodumanal and other south Indian sites (in Srinivasan and Ranganathan 2001; Srinivasan 2007; see also Gullapalli 2009: 453-454; and others) will be mandatory in the next future.

Another phase of proto-urbanization?

A last comment: the late prehistory of the Swat valley for a long time has been interpreted in terms of the marginal evolutionary trajectory of disjoined small-scale rural communities, somehow passively receiving influences, first from mainland China, then by the Indus valley, and eventually from Central Asia (Stacul 1987). The last 20 years of intense work in the valley had the effect of changing the picture. The recent discovery of a massive stone and rammed earth construction, certainly a boundary wall defending the late Bronze age settlement of Barikot (10th-9th centuries BC; Olivieri et al. 2019), together with the evidence of an extensive coeval settlement system along the valley and (now) a highly developed early iron metallurgy, would rather point to a powerful early urban hub. Of course, in order to substantiate similar views, we would proceed to large-scale digs of the concerned settlement phases[11].

Is the present evidence a symptom of a 1.5 urbanization phase between the first (early Bronze) and the second ("Early Historic") waves? Maybe. In 2005, our colleagues excavating in the Bannu basin, Pakistan (former North Western Frontier Province, now Khyber Pukhtunkhwa), wrote that "...By sometime in the early first millennium B.C., Akra was already a large, and possibly urban, center. Large, possibly urban, entities with local economies and extensive trade and exchange

[11] The protohistoric layers of Barikot were sounded outside the Indo-Greek walls, and in single limited trench, were occupation surfaces and the top of the wall were exposed at c. 8 m of depth. Their exploration is hindered by the amount of subsequent sediments. For the time being, our main efforts will be the publication of the test trenches at Barikot, and of the settlement of Aligrama, excavated to some extension but so far published only in minimal part.

networks were already in existence in this region from ca. 900 B.C." (Magee et al. 2005: 734-735). This, too, today looks like a piece of noticeable archaeological insight.

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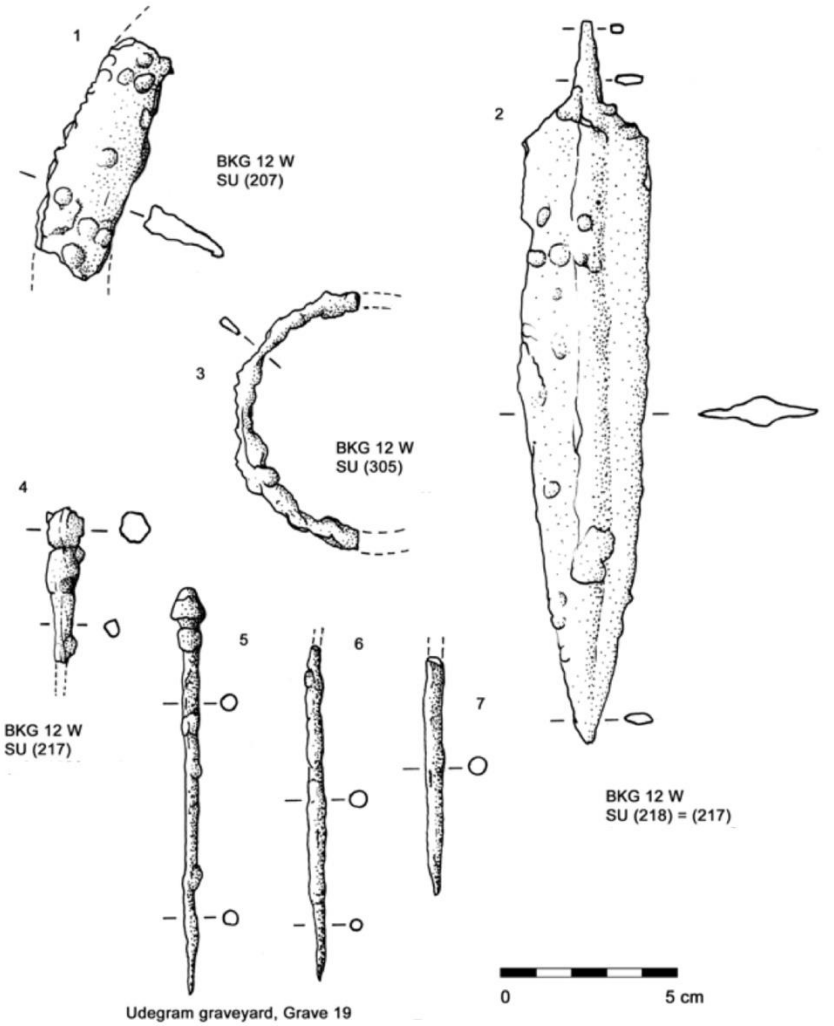
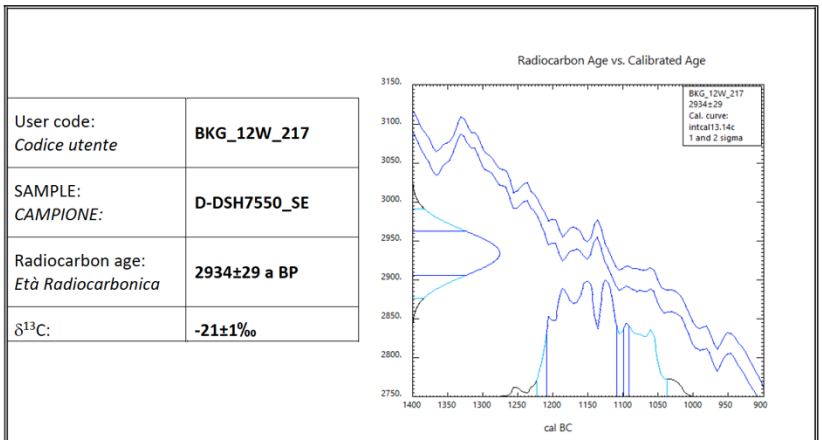


Fig. 1 - Early iron finds with well controlled stratigraphic contexts from recent excavations in the Swat valley. 1, fragment of sickle, from Barikot, Trench BKG 12, SU (207); 2, dagger, Barikot, Trench BKG 12, SU (218) = (217), BC 1223-1036 cal 2σ 100% – BC 1208-1109 ca cal 1σ 94.3%; 3, fragment of a bangle, Trench BKG 12, SU (305), BC 1131-1011 cal 2σ ; 4, head of a pin, from Barikot, Trench BKG 12, SU (217), BC 1223-1036 cal 2σ 100% – BC 1208-1109 ca cal 1σ 94.3% (Olivieri et al. 2019); 5-7, from the graveyard of Udegram, Grave 19, found on the cranium and in the filling, BC 928-802 cal 2σ (Vidale et al. 2015).



User code: *Codice utente*	**BKG_12W_217**
SAMPLE: *CAMPIONE:*	**D-DSH7550_SE**
Radiocarbon age: *Età Radiocarbonica*	**2934±29 a BP**
$\delta^{13}C$:	**-21±1‰**

Calibration data set: intcal13.14c # Reimer et al. 2013

confidence level *livello di confidenza*	cal AD/BC age ranges *intervalli di età calibrata*	relative probability *probabilità relativa*
68.3 (1 sigma)	cal **BC 1208 - BC 1109**	94.3%
	BC 1098 - BC 1090	5.7%
	-	
95.4 (2 sigma)	cal **BC 1223 - BC 1036**	100.0%
	-	

Fig. 2 – Trench BKG 12, SU (217) = (218)
BC 1223-1036 cal 2σ 100% – BC 1208-1109 ca cal 1σ 94.3% (Olivieri et al. 2019)