

A re-analysis of the Qiemu'erqieke (Shamirshak) cemeteries, Xinjiang, China

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Excavation of the Qiemu'erqieke cemeteries in the 1960s revealed the earliest known Bronze Age culture in northern Xinjiang. Burial practices and grave goods show important connections to the Eurasian steppes. The sites have never been fully published and there has been much speculation about the exact nature of the Qiemu'erqieke finds. This paper sets out a highly detailed re-analysis of the available data and presents some new perspectives on the sites, their chronology and external parallels.

Introduction

In 1963, thirty-two burials were discovered and excavated along the Qiemu'erqieke (Shamirshak)¹ river valley in northern Xinjiang (Figure 1) (Xinjiang Institute of Archaeology, Academy of Social Science 1981). The excavation of the Qiemu'erqieke cemeteries is important for the archaeology of Xinjiang, and specifically the Zhunge'er (Junggar) Basin, but there is also a much wider significance. The Qiemu'erqieke sites represent one of the most easterly manifestations of the Eurasian steppic Bronze Age, which in turn provides evidence for cultural interaction further to the east with the peoples on the fringes of the Chinese heartland. The cultural traditions represented by the pottery and burial practices suggest some similarities between Qiemu'erqieke and steppic Bronze Age cultures, specifically the Afanasievo, Okunevo and Karasuk cultures in the Upper Yenisei region of southern Siberia (Chen Kwang-tzuu and Hiebert 1995).

¹Place names within the People's Republic of China are given in Chinese Pinyin with the common Turkic or Mongol spelling in brackets at the first occurrence. Spelling of local names is not consistent throughout English language publications and the names here may vary from those provided elsewhere.

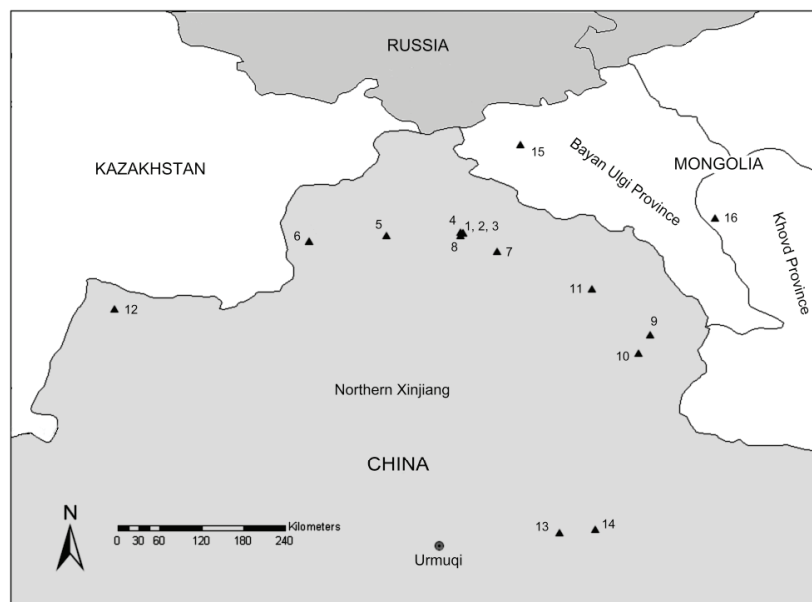


Figure 1: Map showing general location of the Qiemu'erqieke cemeteries and the distribution of Qiemu'erqieke Phase I. Sites indicated by solid triangle: 1, 2, 3 Qiemu'erqieke; 4 Kayinar; 5 Kuobo'er; 6 Burial at Burjin; 7 Kuyirekebayidengkuolasi; 8 Kalatasi; 9 Chagan'guoleng; 10 Basikekeren; 11 Burial at Fuyun; 12 Sasibulake; 13 Burial at Jimusar; 14 Kan'erzi; 15 Ulanhussum; 16 Bulgansum.

The Qiemu'erqieke cemeteries are located against the southern foothills of the Aletai (Altay) mountains and can be most closely linked to cultures to the north. The Afanasievo culture became established in southern Siberia by the late 4th to early 3rd millennium BC (Table 1). Related sites have been found from the southern Urals to western Mongolia but most are centred around the Upper Yenisei in the Minusinsk basin to the north of the Sayan mountains (Gryaznov 1969: 46 ff.). The Afanasievo peoples were predominantly pastoralists, possibly with some agriculture supplemented by hunting. Some settlement sites have been found, while the remains of wagons in the cemeteries imply a fairly high degree of mobility. Use of metals was mainly limited to simple smelting of native copper with knowledge of gold and silver working (Okladnikov 1990: 80). Afanasievo ceramics are dominated by round-bottomed jars with full body incised decoration

Table 1. Chronology of the Steppic Bronze Age (Eastern Steppe - Altai) (adapted from Frachetti 2008: 23, Figure 6 and Gorsdorf *et al.* 2004)

CULTURE	APP. DATE	ARCHAEOLOGICAL PERIOD
Afanasievo	3200 - 2500BC	Chalcolithic-Early Bronze Age
Okunevo	2500 - 1700BC	Middle Bronze Age
Andronovo	1700 - 1400BC	Late Bronze Age
Karasuk	1400 - 800BC	Final Bronze Age
Saka	800 - 250BC	Iron Age

(Gryaznov and Krizhevskaya 1986:21, fig. II). The Okunevo follows the Afanasievo in the Upper Yenisei and dates broadly from the mid-3rd to early 2nd millennium BC. Full bronze technology emerges and there are changes and developments in burial practices. In the ceramic repertoire, incised decoration continues but the round-bottomed jars give way largely to those with flat bases (Gryaznov 1969: figs 9, 10). By the early to mid-2nd millennium BC sites associated with the much more widespread steppic Andronovo cultural complex appear in the northern part of the Minusinsk Basin, and from the later 2nd millennium BC down to the beginning of the 1st millennium BC the mountains and steppe around the Upper Yenisei and to the south are dominated by Karasuk sites (Gryaznov 1969). These are distributed from central Kazakhstan across into Mongolia, with related local variants in north-west Xinjiang. Based on the ceramic evidence, penetration of Andronovo cultural influence into western Xinjiang appears to be limited to the western Tianshan and the Ili River valley, although metal objects and other artefacts travelled further east and south (Kuzmina 2007: 254).

The Qiemu'erqieke sites have also been linked with the eastward migration of Indo-European speaking peoples, specifically those who brought the ancestral version of the Tocharian language into the Tarim Basin. It has been suggested that they might provide a possible "missing link" between the Afanasievo culture, Qiemu'erqieke and the sites of Gumuguo (Xinjiang Institute of Archaeology, Academy of Social Science 1995: 92-102) and Xiaohe (Xinjiang Institute of Archaeology 2003) in the Taklamakan desert of southern Xinjiang (Mallory and Mair 2000: 307; Lin Meicun 2002). Xiaohe and Gumuguo are the earliest Bronze Age cemeteries

in the Taklamakan. Xiaohe has been dated by C14 to around 1800 BC, while Gumuguo has a range of calibrated dates that centre roughly from the end of the 3rd into the early 2nd millennium BC (Mallory and Mair 2000: 336). The cemeteries are remarkable for their organic preservation but also for the almost total absence of ceramics, which appear to have been replaced largely by basketry. The sites have been linked to early movements into China from Eurasia (Kuzmina 2007:251-266, 2008:88-98, Renfrew 2002, Frachetti 2002, Li Shuicheng 2002, Mallory and Mair 2000, Linduff 2000, Mei Jianjun 2000, 2003, Higham 2002, Lin Yun 1986). The connections with the Afanasievo and Qiemu'erqieke cultures have been made on the basis of the early date, some parallels between the shapes of the basketry and northern ceramics, and the identification of the Xiaohe and Gumuguo populations as Caucasoid (Xinjiang Institute of Archaeology 2003; Mallory and Mair 2000:137).

Previous analyses of the Qiemu'erqieke Cemetery Data

A clear understanding of the evidence provided by the Qiemu'erqieke cemeteries is a key issue for study of prehistoric cultural relations between East Asia and Eurasia. However, this is presently not the case. There are problems with the chronology of the sites and there is confusion over the correct name of their location. In the preliminary excavation report published in 1981 (Xinjiang Institute of Archaeology, Academy of Social Science 1981) the cemeteries containing the thirty-two burials were collectively called Ke'ermuqi (克尔木齐) Cemeteries, a somewhat confusing adaptation of the real name of the river valley and the local council.² The correct name, Qiemu'erqieke, is the close pronunciation in Chinese Mandarin of the Turkic name Shamirshak, referring both to the local council (切木尔切克) and the river valley running from north to south across the administrative district. Qiemu'erqieke Council is located on the southern slopes of the Aletai mountain range, and administratively belongs to Aletai City or Aletai D.C. (previously a county before 1984), in the Xinjiang Uighur Autonomous Region, China. However, the name Ke'ermuqi used in the preliminary excavation report became popular in Chinese publications. Later, and still more

²In Chinese archaeological field practice, it is the normal rule to use the closest village or council for naming a site at the first time of discovery.

confusingly, another name, Qie'ermuqieke (切尔木切克), was also used. Both names refer to the original excavated cemeteries. In this paper the name of the local council Qiemu'erqieke (Turkic Shamirshak) will be used to emphasise the linguistic origin, to rectify the incorrect names in Chinese publications, to assist in properly identifying the geographical location of the cemeteries, and to ensure that the correct name enters non-Chinese scholarly literature.

Apart from the issue of the name, data analysis and chronology are also problematic. Interpretations of the contexts and chronology of the Qiemu'erqieke cemeteries have developed gradually over more than twenty years. The first summary of the excavations (Xinjiang Institute of Archaeology, Academy of Social Science 1981) was published almost two decades after the excavations took place in 1963. The findings presented in the report were based on limited information and underdeveloped analysis. The report suggested that the Qiemu'erqieke cemeteries are multi-period and range in date from Han (2nd century BC) to Turkic (4th - 7th centuries AD). The identification of Turkic burials is based on the stone anthropomorphic monoliths found in this area. In the past these have been indiscriminately identified as Turkic, but more recent studies have shown them to occur back into much earlier periods as well (Telegin and Mallory 1994, Wang Bo 1995, Kovalev 1999). The identification of Han Dynasty burials was based on a single red clay wheel-made vessel found in burial M4 (see Table 2). In the preliminary report a far-ranging comparison is made with Wan'gong Cemetery in Inner Mongolia, more than 3000 kilometres east of Qiemu'erqiek. This speculation, however, was questioned soon after the publication of the report (*e.g.*, Wang Bo 1995).

A new interpretation proposing a connection between the early remains at Qiemu'erqieke and the Bronze Age cultures in the Eurasian steppe began with Shui Tao (1993), although his parallels were quite general. Based on deeper analysis of various finds in the Qiemu'erqieke cemeteries, Chen Kwang-tzuu and Hiebert (1995:269-272) suggested connections between the early Qiemu'erqieke contexts and the Afanasievo and Okunevo cultures, as well as a "close affinity" with the early Andronovo culture (Table 1). They concluded that the early remains from the Qiemu'erqieke cemeteries should be dated around the mid-second

Table 2. List of burials in Qiemu'erqieke Cemeteries (Reconstructed from Table 1 and Table 2, Xinjiang Institute of Archaeology, Academy of Social Science 1981) (continued)

M7	m1	10.5 x 4.4	Unclear	(S)2.18 x 2.74 x 1.36	20°	Bronze arrowhead 1	-	1 ceramic vessel, 4 stone arrowheads, 6 stone vessels, 1 jade artifact, 1 coal artifact				Small cist with earlier burial underneath the main burial
	m2			(S)1.84 x 1.8 x 1.15	20°	-	-	2 stone arrowheads, 2 stone vessels, beads				
M8	1			(S)2.6 x 1.45 x 1.55	20°	-	-	2 stone vessels				
M9				(S)2.82 x 1.08 x 1.25	60°	-	-	-				Robbed
M10				(S)3.75 x 2.5 x 1.85	10°	-	-	-				Robbed
M11				(S)1.4 x 1.9 x 1.3	270°	1 Spearhead, 1 drill	-	2 stone vessels, 1 pottery vessel				
M12				(S)0.75 x 0.75 x 0.4	315°	-	-	-				Robbed
M13	m1	18 x 10.5	Unclear	(S)2 x 1.55 x 1.05	15°	-	-	-				Robbed
	m2			(P)0.8 x 2.1 x 0.4	330°	-	-	-				Stone slab covering
	m5			boundary	?	-	-	-				Stone slabs for cover and
				(P)0.65 x 2 x 0.2	325°	-	-	-				alls
M15				(S)2.95 x 1.46 x 1.4	20°	-	-	1 pottery vessel 1, stone vessel				Small cist inside the large one
				Possible 2				7 vessels (clay and stone) found inside the stone enclosure				
	m1	11.1 x 19.7	East	(S)0.9 x 1.06 x 1.5	0°	-	-	-				
M16	m2			(S)1.6 x 1.82 x 1.2	340°	-	-	3 stone vessels				Three bodies, flexed, supine
	m3			(S)0.15 x 0.55 x 0.45	0°	-	-	-				
	m4			(S)0.4 x 2 x 0.55	315°	-	-	-				
	m5			(S)0.4 x 1.7 x 0.3	315°	-	-	-				Flexed
	m6			Irregular pit	?	-	-	-				Possible a ceremonial pit

Table 2. List of burials in Qiemu'erqieke Cemeteries (Reconstructed from Table 1 and Table 2, Xinjiang Institute of Archaeology, Academy of Social Science 1981) (concluded)

M27				(P)2.3 x 1.06 x 0.80	67°	Small ring	Knife 1, adze 1	-			
M28					20°	Small ring	Damaged objects	Ceramic jar 1	Supine, extended		
M29				(P)2.85 x 2.1 x 0.60	20°	Nails	Nails				Small cist with scattered bones
M30				(P)3 x 1.6 x 1.8	42°	Triangular section Arrowhead 1, Ring	-				
M31				(P)2.1 x 1 x 0.80	20°	-	Knife 2, belt hook 1				
M32				(P)2.1 x 0.70 x 0.90	20°	-	Belt hook				

millennium BC, based on the available C14 dates for early Andronovo in east Kazakhstan (Chen Kwang-tzuu and Hiebert 1995:272). Their study has influenced later scholars (Lu Enguo *et al.* 2001, Lin Meicun 2002, Mallory and Mair 2000).

In recent years, several attempts have been made to summarise the various characteristics of the archaeological contexts based on the primary report (*e.g.*, Han Jianye 2005, Guo Wu 2005). It has been pointed out that some burial rites found in Qiemu'erqieke cemeteries appear to show connections to the steppic Bronze Age cultures of western Siberia such as the Afanasievo, Okunevo, Andronovo and Karasuk (Jettmar 1950, Okladnikov 1959, Gryaznov and Krizhevskaya 1986:15-23, Koryakova 1996, Koryakova and Epimakhov 2007). Inspired by the identification of Qiemu'erqieke Phase I from the new discovery of a single cist burial located in the general vicinity of the original Qiemu'erqieke cemeteries (Zhang Yuzhong 2005, 2007), Lin Yun (2008) has reinterpreted the early phase of the Qiemu'erqieke cemeteries, providing an estimated date of around 2000 BC. He suggests that the early burials from Qiemu'erqieke cannot be included simplistically in any known regional Bronze Age cultures, even though some similarities indeed existed between them. He concludes that the early remains of Qiemu'erqieke should be considered a new local Bronze Age culture, partially overlapping chronologically with the Afanasievo, Okunevo and Karasuk. Shao Huiqui (2008) has also attempted to separate the early burials from the three cemeteries in Qiemu'erqieke with very encouraging results.

There is little consistency in the patterning of the material remains from Qiemu'erqieke as they were presented in the initial report (Xinjiang Institute of Archaeology, Academy of Social Science 1981). There is an apparent mixing of different periods. The artifacts, including items of stone, ceramic, bronze and iron, cannot easily be classified as one homogenous cultural tradition. Ceramic vessel forms vary widely. They include a wheel-made, flat-based, long-necked jar made from fine reddish clay with a plain surface (Xinjiang Institute of Archaeology 1981: fig. 3.3), a hand-made, flat-based jar with punctate decoration at the rim (Fig. 7.14), goblet-shaped vessels, possibly used as lamps (Fig. 7. 6,7) and a straight-sided, flat-bottomed jar with incised decoration at the

rim (Fig. 7.12). The dominant forms, however, are ovoid, round-bottomed jars made from coarse grey clay with sophisticated patterns of incised decoration (Fig. 7: 15-19, 22). Unless a robust analysis of the contexts and chronology of the cemeteries is undertaken, the cemeteries will remain the subject of general and speculative arguments. The purpose of this paper is to study the evidence in greater depth and detail than has yet been attempted, specifically to clarify the chronology and associated variation in material culture of the Qiemu'erqieke cemeteries.

Re-examination of the Qiemu'erqieke Cemetery Data

The first steps in re-examining the Qiemu'erqieke cemeteries are to look closely at each individual burial, identify any apparent relationships between burials, and to analyse carefully every detail of the cemeteries available in the preliminary report (Xinjiang Institute of Archaeology, Academy of Social Science 1981). This has not yet been done in past studies although there are some generalised summaries (*e.g.*, Han Jianye 2005, Guo Wu 2005, Lin Meicun 2002). One reason for the difficulty in re-examining the cemeteries is the poor presentation of data in the initial report, which has severely limited the amount of information gleaned from the material in subsequent studies. The study in this paper is based mainly on, but is not limited to, the report published in 1981. Some information appearing in later publications has provided valuable supplementary data. The tables attached to the preliminary report (reconstructed as Table 2), which seem to have been largely overlooked in previous studies, contain crucial information although they are still apparently incomplete.

One solution to this problem would be to consult the original excavation records, but in China tracing any original documents such as fieldwork notes, diaries, or databases for work that was conducted before the 1980s is extremely difficult. The Qiemu'erqieke cemeteries were excavated in 1963, nearly half a century ago, and it is very unlikely that the original notes have survived the subsequent years of political turbulence in China.³ It seems that the problems of dating cannot be resolved without further fieldwork. This is unlikely

³The main political event of this era was the Cultural Revolution lasting from 1966-1976.

to occur in the near future as there is strict regulation of archaeological research in China, confining fieldwork primarily to rescue excavations. Under these circumstances, the preliminary report published in 1981, the descriptions in the tables attached to the report, and fragmentary information appearing in various later publications have become, for the time being, the only bases for any attempt at further study of the Qiemu'erqieke cemeteries.

It is not clear as to exactly what was discovered during the excavation at the Qiemu'erqieke Cemeteries because the initial report contains very limited information in the text but it is possible to supplement this by carefully comparing the data from the tables with the text and illustrations. It is also worthwhile comparing the tables with various publications which contain relevant information that was unpublished in the initial report. Since the original excavations, several isolated burials from the region have been examined. The publications of these graves show that they are similar to those from Qiemu'erqieke as, for example, the sarcophagus burials found at Alepabulake (Wang Bo *et al.* 2005) and Kuoboer (Zhang Yuzhong 2005, 2007) in Buerjin County, Aletai.⁴ These new discoveries not only supplement the contexts of Qiemu'erqieke but they also provide relatively reliable information including details of burial rites and clearly associated artefacts.⁵ Used as a baseline for this new study, these burials, although only individual examples, make it possible to identify discrete clusters of material culture within the Qiemu'erqieke data.

In analysing the sketch of the excavation plan, it becomes clear that Qiemu'erqieke comprises three sub-cemeteries lying along the Qiemu'erqieke River, each located next to a modern village: Brigade 1, Brigade 2 and Water Mill (Figure 2). In the excavation sketch plan (Figure 2), some burials are marked by rectangular symbols delineated by broken lines. The map key states that these indicate rectangular stone enclosures which may contain one or more

⁴Alepabulake burial has a cist coffin possibly associated with a stone pot (Wang *et al.* 2005) and Kuoboer burial has a well constructed cist associated with two typical hand made vessels, one an olive shaped jar and the other resembling the 'oil burners' found in the Qiemu'erqieke cemeteries (Zhang 2007, 2005)

⁵Unfortunately, these burials were only recorded following rescue excavation after they had been disturbed by local farmers.

burials. Cist burials are represented by squares with a solid line, which are differentiated from pit burials represented by circles with a solid line. Shaded squares with broken lines show unexcavated burials and small dark trapezoidal symbols on one side of the stone enclosures show the location of anthropomorphic stone stelae.

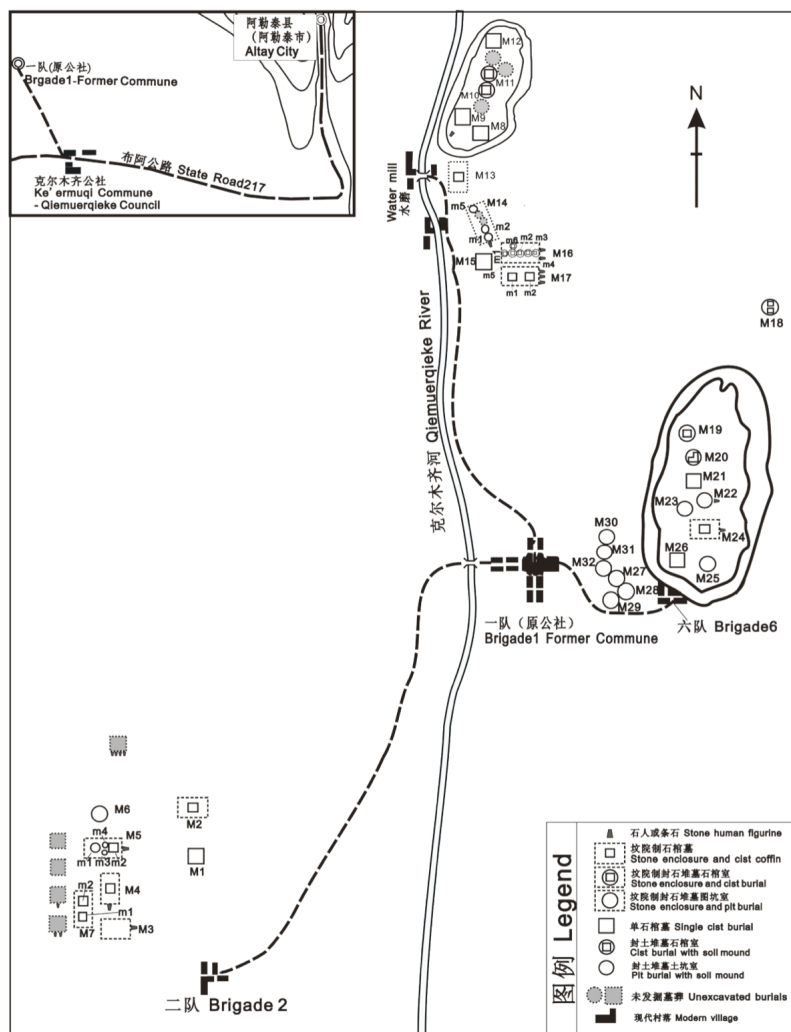


Figure 2: Qiemu'erqieke cemeteries excavated in 1963 – village names are those used before the 1980's and have been changed since then (Redrawn from Xinjiang Institute of Archaeology, Academy of Social Science 1981:23, Figure 1).

The artefacts found in the three cemeteries are listed in two tables appended to the original report (Xinjiang Institute of Archaeology 1981). The first table includes all the burials with enclosures and the second contains isolated single burials. The two categories distinguished in these tables illustrate the initial attempt by the excavators to distinguish the different burials. For purposes of clarity the two tables have been joined together and reconstructed in this paper (Table 2). It can be seen clearly that Cemetery I (near Brigade 2) contains burials M1- M7⁶, Cemetery II includes M8 – M17 and Cemetery III comprises M18-M32.

Cemetery I

Cemetery I next to the village of Brigade 2 is in the southwest area (lower left in Figure 2) on the west bank of the river. This cemetery includes burials numbered from M1 to M7 in the excavation plan (Figure 3). The only graves in this cemetery containing iron artefacts are M5-m1 and M4. M4 also contained the distinctive red wheel-made jar (Xinjiang Institute of Archaeology 1981:27)⁷, while M5-m1 contained a bone belt hook. These artefacts are unusual in terms of the most common finds in the other graves which contain bronzes, certain forms of stone vessels, stone arrowheads and grey or buff ware ovoid ceramic jars (Fig. 7.8, 11, 12). Differences between the two groups of burials, the Bronze Group and the Iron Group, can also be seen in the orientation of the graves and the enclosures. The enclosure of M4 is oriented to the south with the cist burial at 10° and the grave of M5-m1 is oriented to 110°. This contrasts with the remaining graves which have enclosures facing east and cists at 20° orientation (Table 2).

Body postures are not easy to determine since some burials were robbed before excavation and skeletons were not always in their original position but, based on available description from the table, at least two different body postures can be defined. Bodies lying on their sides with tightly bent legs can be equated with the burials containing bronzes: the Bronze Group represented by M1, M5-m2 and M5-m4. By

⁶“M” is the first letter in Pinyin of the Chinese word for tomb.

⁷ The pot found in M4 was described in detail in the preliminary report but not in the table.

contrast, bodies placed straight and face up are found only in the burials with iron objects: the Iron Group, as for example, in M5-m1 (Table 2). These distinctive features suggest that at least two different groups of burials can be distinguished in Cemetery I. The Bronze Group should include M1, M2, M3, M5-m2, M7-m1 and M7-m2 and the Iron Group should comprise M4 and M5-m1.

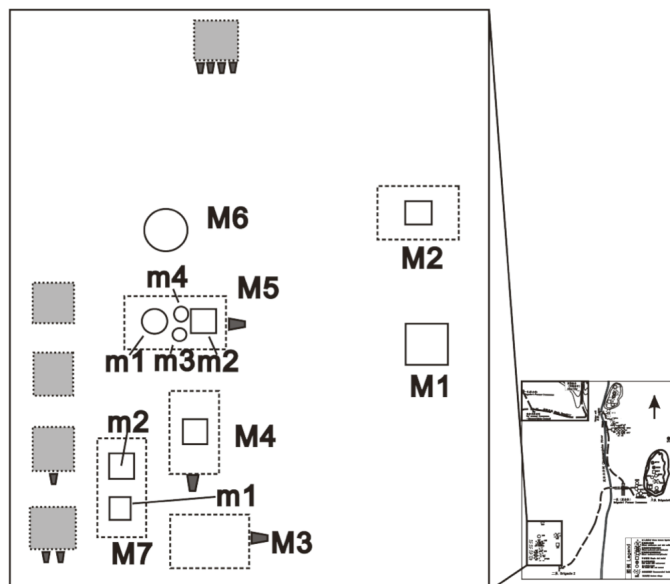


Figure 3: Cemetery I.

This division works well except for the apparent anomaly of M5 which appears to contain graves from two separate groups. Various explanations may account for this. A simple explanation would be that the same enclosure was re-used from one period to another, but a closer look at the details in the description of M5 from the original report shows that there may be some problems with the recording of the enclosure itself. The report defines an enclosure as being constructed of large stone slabs laid on their sides to form a rectangular shape (Xinjiang Institute of Archaeology, Academy of Social Science 1981). However, the text of the report states that no stone slabs were found for the M5 enclosure. M5 was described as being located on the top of a small hill. The presence of an anthropomorphic figure to the east may have made the excavators assume that there had once been an

enclosure. The rectangular enclosure depicted in the plan is in fact an arbitrary drawing of the top of the small hill. The M5 enclosure, therefore, was not in existence during the excavations. It is possible that there may once have been an enclosure that was robbed out by the time of the excavations.⁸⁹ This may have made it impossible for the excavators to determine the exact location and limits of the original enclosure and so in their drawing they incorrectly included burial M5-m1 in the stone enclosure M5. If this assumption is correct, then the four burials in M5 may have no connection between them, or at least M5-m1 has no connection to the other burials of M5.

Cemetery II

The burials in the north area near the modern village of Water Mill (top centre in Fig. 2) are grouped within Cemetery II which comprises burials M8 to M17 (Figure 4, Table 2). The plan shown in Figure 2 suggests that there are two subgroups of burials. Burials M8 to M12, together with three unexcavated burials, form the first subgroup on the top of a small hill (Fig. 4) called Cemetery IIa. The remaining burials M13 to M17 extending out across the south area next to IIa form the second subgroup, Cemetery IIb (Fig. 4). This sub-division may have no real significance as Cemetery IIb could be the extension of Cemetery IIa after Cemetery IIa became crowded. For the purpose of analysis, however, it is useful to differentiate between these two groups of burials.

Cemetery IIa contains five burials, but three out of those five burials have no associated artefacts. In the text of the original report burials M9 and M10 are described as being robbed (Table 1) so the artefacts in those two burials, if there were any, were looted. Burial M12 contained nothing except some fragments of bones. Only two burials, M8 and M11, had artefacts, including stone pots, ceramic vessels and bronze items. No iron objects were found in them. The description of the handmade pottery is very similar to that of the ceramics associated with the Bronze Group of Cemetery I. The body in M11 was placed on its side with the legs drawn up and the cist in M8 was orientated to 20°, both features finding parallels in

⁸⁹Local people usually collect such stone, including the stelae, to use in household construction.

the Bronze Group of Cemetery I. An exception was the 270°

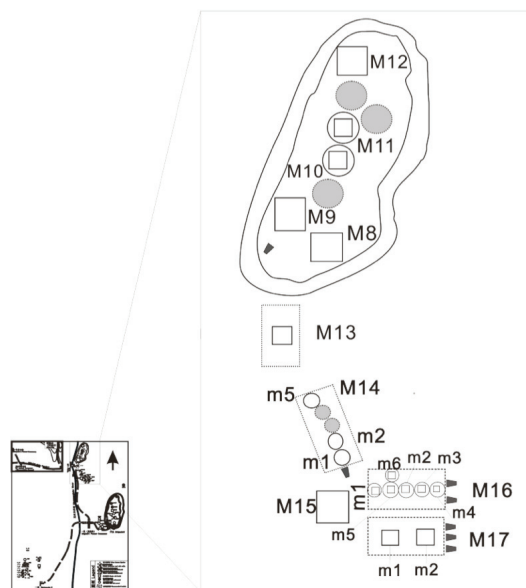


Figure 4: Cemetery II.

orientation of the cist in burial M1, which may suggest some variation in orientation preferences. The position of the body, face upwards with bent legs, is distinct from that of the body placed on its side with legs drawn up, as was found in the Bronze Group of Cemetery I. The copper spearhead found in M11 was the only example among all the burials in the Qiemu'erqieke Cemeteries which might indicate that burial M11 varies from other burials in the Bronze Group in terms of culture or chronology. In addition, the description of the anthropomorphic stelae from M11 suggests that they are somewhat different from the others (Wang Bo 1995:187). All those aspects suggest that M11 may belong to a different group.

Cemetery IIb comprises M13 to M17, a total of fifteen burials, two of them left unexcavated. This leaves thirteen excavated burials (Fig. 3, Table 2). Those thirteen burials in Cemetery IIb share some common characteristics in having no iron objects or wheel-made pottery. The bodies are mostly positioned face upwards with bent legs. According to the description in the original table, the orientation of the cists falls into two groups: 315° -340° and 15° - 30°. This clustering

may reflect the major differences in cist/pit orientations within the Bronze group in the three Qiemu'erqieke cemeteries. All burials in the Bronze Group of Cemetery I are oriented to 20°, that is within in the range of 15°-30°. The exceptions in Cemetery II are the orientation of 270° at burial M11 and 60° at (robbed) burial M9 in Cemetery IIa. The orientation of 10° for (robbed) burial M10 still falls only slightly outside the range for the group of 15°- 30°. ¹⁰ Artefacts found in Cemetery IIb are similar to those of the Bronze Group of Cemetery I (Fig. 7.4, 5, 14, 15, 18). Stone moulds for the casting of bronze artefacts were interesting additional finds from M17-m1 and m2 (Fig. 7.1, 2).

Cemetery III

Cemetery III consists of the burials close to the villages of Brigade 1 and Brigade 6 on the east bank of Qiemu'erqiek River (Fig. 1, centre). Like Cemetery II, this cemetery forms two areas of burial concentrations: M19 – M26 of Cemetery IIIa and M27 – M32 of Cemetery IIIb, plus one isolated burial, M18 (Figure 5).

Cemetery IIIa comprises the burials on the top of a small hill, assuming that the irregular elliptical shapes shown on the map indicate contour lines. Two different burial rites can be identified in this group, defined by the types of burials and the artefacts contained within them. Iron objects were found in burials M22, M23 and M25. These graves are pit burials rather than the rectangular cist burials associated with bronze artefacts. The orientations of the burials range from 70° to 88° to 115°, all facing roughly to the east. This eastern orientation is quite distinct from the broadly northern orientation of the cist burials which are mainly at 345°. The exceptions to this are M24 at 30° and M21 at 100°. M24 fits with the range 15° - 30° for the Bronze Group burials found at Cemetery I and II. M24 also contained similar artefacts including a ceramic goblet (lamp) (Fig. 7.6), a stone cup and a stone lamp. ¹¹ Burial M21 lacks diagnostic artefacts except one stone anthropomorphic figurine of a type that is usually ascribed to the Bronze Age in

¹⁰Since the excavations were conducted in 1963, the equipment and techniques used for measuring may not have been as accurate as those in use today and a small error of 5° in measurement is plausible.

¹¹The stone lamp found in M24 was not illustrated, but it is presumed that it is similar to the ceramic 'oil burner' found in the same burial.

Xinjiang (Lin Yun 2008), so the unusual orientation of M21 might be one example of variation among the Bronze Group

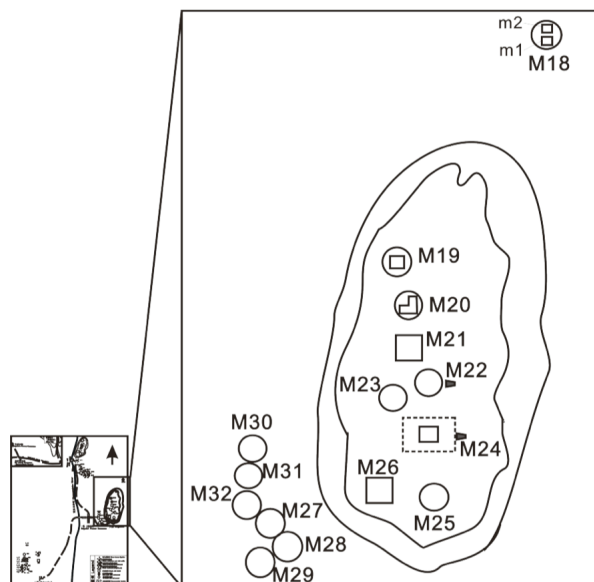


Figure 5: Cemetery III.

in this cemetery due to differences in date or cultural practices. Cemetery IIIb comprises the burials which lay to the south-west of the small hill (Fig. 5). This group contains burials M27 – M32. All are pit burials and can also be categorized as Iron Age. All except M30 contained iron objects. M30 has one diagnostic artefact, a bronze arrowhead with a triangular cross-section. This type of bronze arrowhead did not appear in Xinjiang until around 1000 BC. Two examples of this type of bronze arrowhead dated to *c.*1000 BC were found in burials 216 and 305 at Chawuhu on the northern rim of the Taklamakan desert (Xinjiang Institute of Archaeology 1999:249). Thus it is likely that burial M30 is not earlier than the 10th century BC (Jia Weiming, Betts and Wu Xinhua 2008).

Stylistic variation in the anthropomorphic stone stelae may also provide evidence for potential differences between the two groups. There are no pictures of stelae in the preliminary report that could easily be used for comparison. The best reference for analysis of the stelae is the monograph compiled by Wang Bo (1995). Wang Bo was able to access the stelae found in the Qiemu'erqieke cemeteries. He also studied

a large number of other stone stelae discovered in Xinjiang and adjacent areas. Stelae from two unexcavated cemeteries,

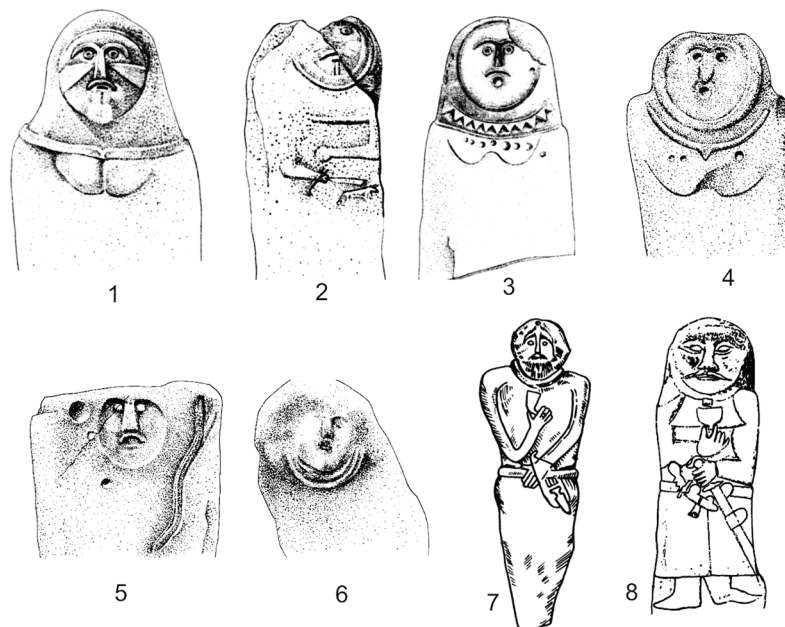


Figure 6: Stone Stelae: 1-4. Qiemu'erqieke Phase I from the Kayinar cemetery (Kovalev 1999); 5, 6. Qiemu'erqieke Phase II from Kalatasi cemetery (Wang 1995:62, 177-Ea-22, 23); 7. Scythian (Telegin and Mallory 1994); 8. Turkic (Wang 1995: 82)

Kalatasi and Hayinar (Wang Bo 1995:62 and plate 156 - Ea-1), have become key typological representations of the two groups from Wang Bo's study. The Kalatasi and Hayinar statues are still *in situ*, lined up along the eastern side of the stone enclosures that surround each cemetery. The cemeteries are located a few kilometres south of the Qiemu'erqieke cemeteries and both are protected by local council and open to tourists.¹² Two small stone slabs carved with human faces standing on the side of the Kalatasi stone enclosure (Figure 6:5-6), are said by Wang Bo (1995:62, 177-Ea-22, 23) to be similar to the stele found at M4 (Phase II) in the Qiemu'erqieke cemeteries. These stand in contrast to the large stone statues erected on the side of the Hayinar stone enclosure (Fig. 6:1-4), which have been categorised by Wang Bo (1995) as typical of the Bronze Age

¹²The authors have visited both cemeteries.

stelae in the Qiemu'erqieke cemeteries (Lin Yun 2008). The design of the stelae changes from an almost life-size statue emphasising the upper half of the body in the Bronze Age to a small stele with a simple representation of the human face on the surface of the stone in the Iron Age (Fig. 6:5-6). There is also a shift from three dimensional sculpting to flat engraving. Overall, the two types of stelae associated with the two groups of burials constitute two cultural traditions and neither the Bronze Age nor the Iron Age stelae in the Qiemu'erqieke cemeteries can be linked in any way to the later and much more common Turkic stone anthropomorphic sculptures dated to the 4th -7th centuries AD (Fig. 6.8).

Summary

In summary, the burials found in the Qiemu'erqieke cemeteries can be classified into at least two groups: one group with bronzes only and one group with iron artefacts. The groups are also distinguished by differences in orientation and style of grave. It seems likely, therefore, that the two groups represent two different periods, the Bronze Age, and the Iron Age and thereafter. The burials identified as Bronze Age include M1, M2, M3, M5m2, M7m1 and M7m2 in Cemetery I, M8, M11, M15, M16m2, M17m1, M17m2 in Cemetery II, and M18m1, M18m2, M19, M20, M21 and M24 in Cemetery III. Following Lin Yun (2008) in assuming the group with bronzes only to represent a new early Bronze Age culture, this is here referred to as Qiemu'erqieke Phase I, in order to distinguish these early burials from those of the second group, Qiemu'erqieke Phase II. Qiemu'erqieke Phase II is characterized by the appearance of iron artefacts and different burial practices. The discussion below will examine Qiemu'erqieke Phase I in greater detail.

Qiemu'erqieke Phase I

Cultural contexts of Phase I

Building on previous studies (*e.g.*, Jia Weiming, Betts and Wu Xinhua 2009, Lin Yun 2008, Shao Huiqui 2007, 2008, Guo Wu 2005, Han Jianye 2005, Mallory and Mair 2000, Chen Kwang-tzuu and Hiebert 199, Shui Tao 1993, Wang Bo 1991), the analysis of the cultural contexts of Phase I below is, however, more detailed because it is based on thorough examination of the evidence for the three individual

cemeteries and also includes comparison with similar burials found at Kuoboer near Ahejiaer village in Woyimokexiang

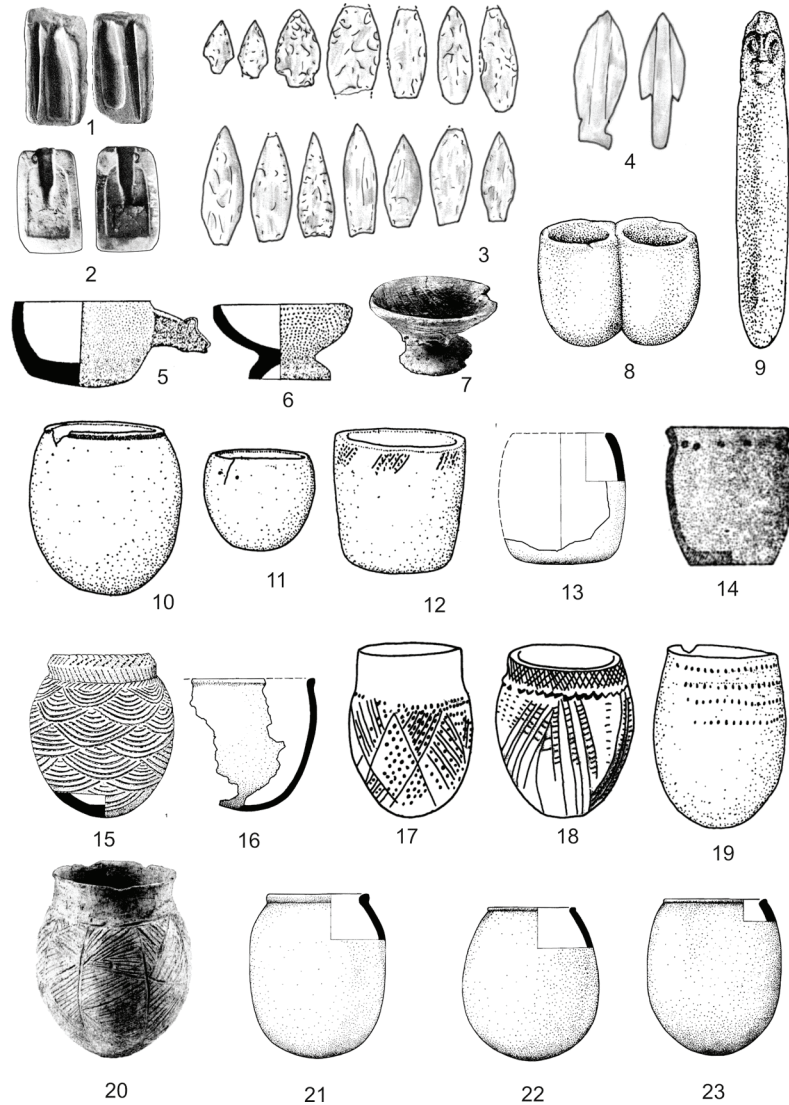


Figure 7: Artefacts from Qiemu'erqieke Phase I: 1-3, 8, 9, 11, 12, 15, 18 (Xinjiang Institute of Archaeology, Academy of Social Science 1985: Plate 66, 68-70, 72-75); 4, 14, 16, 17 (Xinjiang Bureau of Relics *et al.* 1999: 298, 336, 340); 5, 6 (Xinjiang Institute of Archaeology, Academy of Social Science 1981); 7, 20 (Zhang Yuzhong 2007); 10, 13, 19, 21-23 (Kovalev 1999). (The

numbers prefixed by M below refer to grave and artefact number in Xinjiang Institute of Archaeology, Academy of Social Science 1981).

1. Knife mould (Qiemu'erqieke M17:2); 2. Spade mould (Qiemu'erqieke M17:1); 3. Various stone arrowheads (Qiemu'erqieke); 4. Bronze arrowheads (Qiemu'erqieke M17); 5. Stone vessel (Qiemu'erqieke M16:11); 6. Clay lamp (Qiemu'erqieke M24:8); 7. Clay lamp; 8. Stone double vessel (Qiemu'erqieke M3:2); 9. Stone figurine (Qiemu'erqieke M21:1); 10. Stone jar; 11. Stone jar (Qiemu'erqieke M2:9); 12. Ceramic jar (Qiemu'erqieke M7m1:1); 13. Ceramic jar (Qiemu'erqieke M2); 14. Ceramic jar (Qiemu'erqieke M16:4); 15. Ceramic jar (Qiemu'erqieke M16:1); 16. Ceramic jar (Qiemu'erqieke); 17. Ceramic jar (Ka'erzi site, Qitai); 18. Ceramic jar (Qiemu'erqieke M16:3); 19. Ceramic jar (Qiemu'erqieke M16:4); 20. Ceramic Jar (Kuoboer Valley); 21. Stone jars (Qiemu'erqieke M8); 22. Ceramic jar (Qiemu'erqieke M7); 23. Stone jar (Qiemu'erqieke M16).

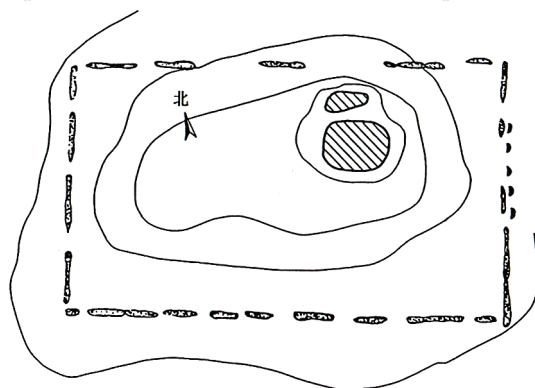


Figure 8: Kayinar Cemetery showing enclosure and stelae (located on right side outside the enclosure) typical of Qiemu'erqieke Phase I (Lin 2008).

(Zhang Yuzhong 2005, 2007), and the Alepabulakao burial (Wang Bo *et al.* 2005) in Buerjin county next to the Aletai region (Fig. 1).

The major characteristics of Qiemu'erqieke Phase I include:

1. Burials with two orientations of approximately 20° or 345° .
2. Rectangular enclosures built using large stone slabs. The size of the enclosure varies from a maximum of 28 x 30 m.¹³ to a minimum of 10.5 x 4.4 m. (Figure 8, Table 2).

¹³The stone enclosure located near Hayinar is the largest one at

3. Almost life-sized anthropomorphic stone stelae erected along one side of the stone enclosures (Lin Yun 2008).
4. Single enclosures tend to contain one or more than one burial, all or some with stone cist coffins.
5. The cist coffin is usually constructed using five large stone slabs, four for the sides and one on top, leaving bare earth at the base (Zhang Yuzhong 2007). Sometimes the insides of the slabs have simple painted designs (Zhang Yuzhong 2005).
6. Primary and secondary burials occur in the same grave.
7. Some decapitated bodies (up to 20) may be associated with the main burial in one cist.
8. Bodies are commonly placed on the back or side with the legs drawn up.
9. Grave goods include stone and bronze arrowheads, handmade gray or brown round-bottomed ovoid jars, and small numbers of flat-bottomed jars (Fig. 7).
10. Clay lamps appear to occur together with round-bottomed jars.
11. Complex incised decoration on ceramics is common but some vessels are undecorated.
12. The stone vessels are distinctive for the high quality of manufacture
13. Stone moulds indicate relatively sophisticated metallurgical expertise.
14. Artefacts made from pure copper occur.
15. Sheep knucklebones (astragali) imply a tradition (as in historical and modern times) of keeping knucklebones for ritual or other purposes. They also indicate the herding of domestic sheep as part of the subsistence economy.

Distribution of sites

The distribution of the Qiemu'erqieke tradition is still not clear, but based on known examples (Table 3), the centre of the distribution tends to be in the Aletai region in Xinjiang and western Mongolia, including the southern slopes of the Aletai mountains, Aletai City as represented by the burials found along the Qiemu'erqieke River (Xinjiang Institute of Archaeology 1981), Buerjin (Zhang Yuzhong 2005, 2007) and Fuhai counties (Lu Enguo *et al.* 2001) but not limited to there (Fig 1).

approximately 30 x 40 m. based on pacing of the site during a visit by the authors in 2008.

Table 3: Archaeological sites of Qiemu'erqieke Phase I

No.	Site name	Local council	Altitude	Latitude	Longitude	Reference
1	Qiemu'erqieke Cemetery I	Qiemu'erqieke County, Altay City	753m	47.810	87.893	XIAASS **1981
2	Qiemu'erqieke Cemetery II	Qiemu'erqieke County, Altay City	753m	47.810	87.893	XIAASS **1981
3	Qiemu'erqieke Cemetery III	Qiemu'erqieke County, Altay City	753m	47.810	87.893	XIAASS **1981
4	Kayinar Cemetery	Qiemu'erqieke County, Altay City	755m	47.826	87.863	Lin 2008
5	Kuoboer burial	Wuoyimoke Xiang, Buerjin County	485m	47.779	86.905	Zhang 2007, 2005
6	Alepabula burial	Buerjin County	485m	47.711	85.903	Wang et al.2005
7	Kuyirkebayidengkuolasi	Fuhai County	598m	47.711	85.903	Lu et al. 2001
8	Kalatasi cemetery	Qiemu'erqieke Xiang, Altay City	691m	47.781	87.860	Wang 1995
9	Changanuoleng Cemetery	Tangpaleyuzi Village, Qinghe County	n/a	46.501	90.307	Wang 1995
10	Basikekren Cemetery	Qinghe County	n/a	46.27	90.156	Wang 1995
11	n/a	Fuyun County	n/a	47.096	89.552	Wang 1996
12	Sasibulake cemetery	Tacheng City	n/a	46.837	83.399	Wang 1997
13	n/a	Jimsar County	n/a	43.949	89.136	Lin 2009
14	Kaerzi site	Qitai County	n/a	43.994	89.594	Xinjiang Bureau of Relics et al. 1999
15	Ulanhussum Cemetery	Bayan Ulgii Province, Mongolia	n/a	48.95	88.63	Kovalev 2008
16	Bulgansum Cemetery	Khovd Province, Mongolia	n/a	48.007	91.1405	Kovalev 2008

** Xinjiang Institute of Archaeology, Academy of Social Science 1981

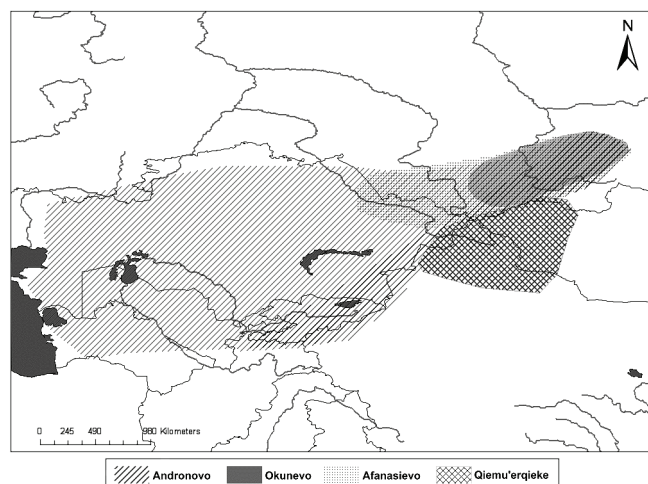


Figure 9: Bronze Age cultures of eastern Eurasia.

Anthropomorphic stone stelae discovered in different locations have provided some signs of the possible extent of Qiemu'erqieke Phase I¹⁴ (Lin Yun 2007, Wang Bo 1995, Chen Kwang-tzue and Hiebert 1995). The stelae that may be identified with Qiemu'erqieke phase I have been discovered in Qinghe County in the east Aletai region, for example at Chaganguoleng Cemetery (Wang Bo 1995:54, 160-Ea-5), Tangbaleyuzi Village (Wang Bo 1995:54, 161-Ea-5), and Basikekeren Cemetery (Wang Bo 1995:55, 162-Ea-7), as well as in Fuyun County, Aletai (Wang Bo 1995:58-63, 169-Ea-14, and 171-Ea-16). They have also been found at the Sasibulake cemetery in Tacheng County south of the Aletai region (Wang Bo 1995:77, 209-Eb-4). These stelae possibly imply the eastward and southward expansion of the Qiemu'erqieke tradition. Similar burials found in western Mongolia have suggested a further eastward expansion (Kovalev 2008). The example found at Jimusar County at the southern edge of the Zhunge'er Basin (Wang Bo 1995:93, 240-Ee-7) seems far away from the centre of Qiemu'erqieke, but a similar round-bottomed jar also found at Qitai County east next to Jimusar is unlikely to be a coincidental discovery. On the contrary, it may in fact suggest a far southeastern distribution (Fig. 1).¹⁵

¹⁴No illustrations of stone stelae on enclosures were published in the 1981 preliminary report.

¹⁵See also Chen Kwang-tzue and Hiebert 1995.

Whether this tradition spreaded across the Tianshan into the southern area of Xinjiang is not clear, but there is no evidence so far from the southern slopes of the Tianshan for Qiemu'erqieke Phase I unless the purported links with Xiaohu basketry are to be believed (Lin Meicun 2002). To the west, it may extend into eastern Kazakstan along the Upper Irtysh River valley and the southern area of the Russian Aletai based on recent discoveries (Kovalev 2008) in those areas (Figure 9).

Chronology of Qiemu'erqieke Phase I

Available evidence suggests that the date range for Qiemu'erqieke Phase I should fall from the later third into the early second millennium BC. There are several reasons to suggest that the time span is around the early second millennium BC. Lin Yun (2008) has specifically discussed this issue. First, he suggests, based on the evidence of copper objects and the number of bronzes found at Qiemu'erqieke, that Phase I cannot date back as early as the start of the Afanasievo (2008:158). He maintains that the bronze artefacts found in Phase I show a greater sophistication in the level of copper alloy technology than that of the pure copper artefacts common to the Afanasievo tradition. On this basis it might be suggested that the Afanasievo could be considered to be Chalcolithic with a time span across much of the third millennium BC (Gorsdorf *et al.* 2004:86, Fig. 1).¹⁶ Qiemu'erqieke Phase I, however, should more properly be considered as Bronze Age. Lin Yun also used the bronze arrowhead from burial M17 to narrow down the date of Qiemu'erqieke Phase I. Two arrowheads were found in this burial, one of them leaf shaped with a single barb on the back (Fig. 7:4). A similar arrowhead, together with its casting mould, has been found at the Huoshaogou site of Siba tradition (Li Shuicheng 2005, Sun Shuyun and Han Rufen 1997), in Gansu province, northwest China, dated around 2000-1800 BC (Li Shuicheng and Shui Tao 2000). This supports a date in the early second millennium BC for the Qiemu'erqieke arrowhead. The painted, round-bottomed jar from the Tianshanbeilu cemetery (Jia Weiming, Betts and Wu Xinhua 2008: Fig. 7,

¹⁶There are different opinions on the chronology of the Afanasievo due to the great deal of variation in available C14 dates (Chernykh 2004), but a recent study by Gorsdorf *et al.* (2004) for the Afanasievo of the Minusinsk Basin should be relevant for this analysis.

bottom left) has been considered as a hybrid between the Upper Yellow River Bronze Age cultures of Siba in northwest China and the steppe tradition of Qiemu'erqieke in west Siberia (Li Shuicheng 1999). If this assumption is correct, the date of Tianshanbeilu, around 2000 BC, can be used as a reference for Qiemu'erqieke Phase I (Jia Weiming, Betts and Wu Xinhua 2008, Lin Yun 2008, Li Shuicheng 1999). Stone arrowheads found in Qiemu'erqieke Phase I also imply that the date is likely to fall within the earlier part of the Bronze Age as no such stone arrowheads have yet been found elsewhere in sites of the Bronze Age in Xinjiang dated after the beginning of the second millennium BC.¹⁷

Table 3. Chronological typology of the Okunev ceramic tradition

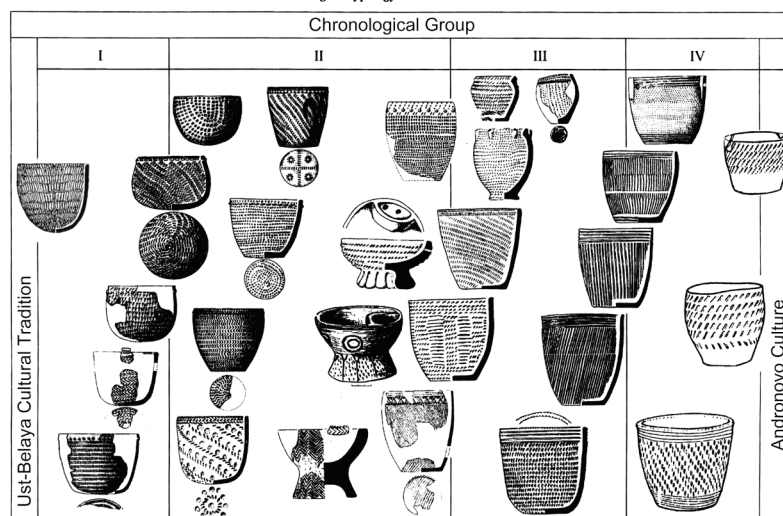


Figure 10: Typological chronology of the Okunevo tradition (Sokolova 2007).

Recent research on the Okunevo culture by Sokolova (2007) can be used as a parallel reference to provide more information for the chronology of Phase I. One of the defining characteristics of Okunevo pottery during the earlier Bronze Age in the steppe is the predominance of flat-based vessels. By analysing typological change through time, Sokolova (2007) has established a chronology for the Okunevo pottery (Figure 10).

¹⁷For example Chawuhu and Xiaohe cemeteries (Xinjiang Institute of Archaeology 1999, 2003).



Figure 11: Comparisons of different ceramic traditions: 1) Yamnaya; 2, 5) Anfnasievo; 4, 7) Okunevo; 3, 6, 8, 9) Qiemuerqieke Phase I.

1. (Shishlina 2008: 46); 2, 5. (Okladnikov 1959:77); 3. (Zhang 2007); 4. (Sokolova 2007); 6. (Xinjiang Institute of Archaeology, Academy of Social Science 1981); 7. (Lazaretov 1997:61, Fig. 20, 2); 8. (Xinjiang Institute of Archaeology, Academy of Social Science 1985: 71); 9. (Kovalev 1999: 161, Fig. 13. 3).

A jar form with a flat base, straight wall and rim and incised pattern of decoration near the rim from Qiemu'erqieke Phase I (Xinjiang Institute of Archaeology, Academy of Social Science 1985: pl. 74; Fig. 7.12) is similar to the jar form found in the later stage of the Okunevo, Sokalova's chronological group IV, dated around 1900 BC, contemporary with the appearance of the Andronovo in eastern Kazakhstan (Fig. 10) (Sokolova 2007: table 3). Chen Kwang-tzuu and Hiebert (1995:271) have made comparisons between a flat-based shouldered jar (Fig. 7.14) and early Andronovo ceramics. This jar form also occurs in related sites in Mongolia (Kovalev 2008: fig. 3, 9-11). It can only be generally compared to Andronovo vessels and the evidence is insufficient to support Chen Kwang-tzuu and Hiebert's (2005) suggestion of a close affinity to the early Andronovo. Pottery

“oil burners” (goblet-like ceramic vessels, possibly lamps) have been found in three traditions: Afanasievo (Gryaznov and Krizhevskaya 1986:21), Okunevo (Fig. 10) and Qiemu’erqieke (Fig. 7.6-7). It is believed that this oil-burner found in Siberia and the Aletai is a heritage from the Yamnaya and Catacomb cultures (Sulimirski 1970: 225, 425; Shishlina 2008:46) in the Caspian steppe further to the west (Figure 11), but does not seem to exist in known Andronovo cultures. The oil-burner tends to disappear after around 2300 BC during the mid-Okunevo period (Fig. 10). It is, however, possible that the tradition continues longer in the Qiemu’erqieke sites.

The construction of the stone enclosures also reveals a close connection between Qiemu’erqieke Phase I and the mid and late Okunevo tradition (Sokolova 2007). Slab built stone enclosures emerged in both the Okunevo and Afanasievo traditions (Gryaznov and Krizhevskaya 1986:15-23, Kovalev 2008, Sokolova 2007, Anthony 2007:310, Koryakova and Epimakhov 2007). In the early Afanasievo the enclosure is circular with no cist coffin (Anthony 2007:310, Gryaznov and Krizhevskaya 1986:20), but in the early stage of the Okunevo square stone enclosures with a single cist burial are dominant. Square or rectangular stone enclosures are a marked feature of Qiemu’erqieke Phase I, suggesting temporal relationships between Qiemu’erqieke Phase I and the Okunevo. In Okunevo chronological group II, possibly with influence from the Afanasievo, circular stone enclosures appeared in combination with rectangular enclosures within individual cemeteries, referred to by Sokolova (2007: table 2) as hybrid examples. By Okunevo chronological group III, rectangular stone slab enclosures with multi-burials emerged again. This is the dominant form in Qiemu’erqieke Phase I. Okunevo burial traditions changed again to single cist burials in the late stage around chronological group V (Sokolova 2007). A specific mortuary rite of decapitated burials exists in both the Qiemu’erqieke and Okunevo traditions (Sokolova 2007, Chen Kwang-tzuu and Hiebert 1995), as does the occasional occurrence of painted designs on the interior of the slabs forming the cists (*e.g.*, Khavrin 1997: 70, fig. 4; 77: tab. IV.5). Based on these comparisons, the date of Qiemu’erqieke Phase I may well parallel that of the Okunevo from at least chronological group II around 2400 BC (Gorsdorf *et al.* 2004: fig. 1).

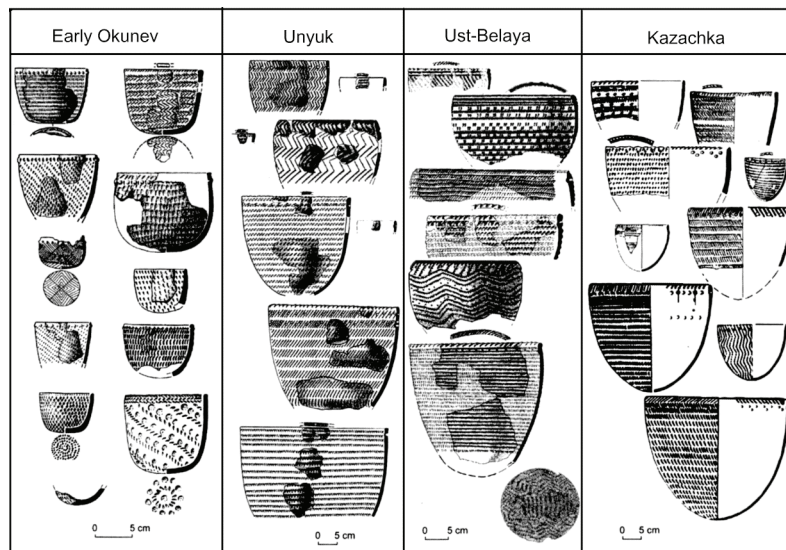


Figure 12: Comparison between the Early Okunevo and contemporary cultures (Sokolova 2007).

With regard to the tradition of rounded bases on the ceramic vessels in Qiemu'erqieke Phase I, this has generally been assumed to relate to the Afanasievo (*e.g.*, Lin Meicun 2002), despite the occasional appearance of flat-based vessels in the Afanasievo assemblage (Lin Yun 2008). Rounded bases, however, are not exclusive to the Afanasievo, but rather they appear in various cultural traditions across a broad area of the Eurasian steppe from the sixth millennium BC Neolithic to the Bronze Age of the second millennium BC (Zakh 2006, Kislenko and Tatarintseva 1999), including the Baotai tradition in western Siberia (Kislenko and Tatarintseva 1999) and the Kitoi, Galzkovo and Serovo assemblages in Baikal and the northern Altai Region (Aseyev 2002, Kungurova 2003). Besides the Afanasievo, during the third to second millennium BC, round-bottomed jars with full body incised decoration also occur in the Minusinsk Basin in, for example, the Ust-Belaya culture (Sokolova 2002), and east of the Aletai region in Mongolia, in the Selenga-Daurian culture (Figure 12) (Cybiktarov 2002). They are also a feature of the early Okunevo (Figs 10, 11). With this wide distribution of a generalised technique of ceramic manufacture it is difficult to be precise about the specific influences on Qiemu'erqieke pottery making.

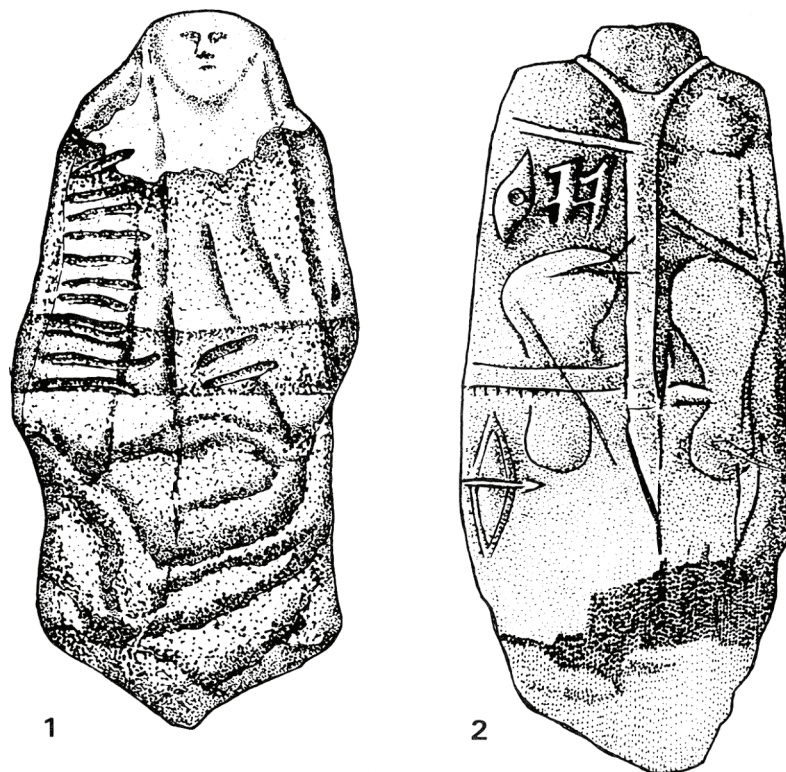


Figure 13: Chalcolithic stelae from the Ukraine (Telegin and Mallory 1994:6).

With the present lack of intensive fieldwork, the possibility cannot be ruled out that some traditions, including the ceramic forms of Qiemu'erqieke Phase I, were acquired at a very early period even before their appearance in the Qiemu'erqieke region, as suggested by Lin Yun (2008). In addition to the pottery making tradition, the anthropomorphic stone stelae may also have earlier antecedents. In the Okunevo assemblage there are anthropomorphic stelae that are longer, thinner and more abstract than those of Qiemu'erqieke. There is no indication of such stelae in the Afanasievo tradition (Gryaznov and Krizhevskaya 1986:15-23). However, further to the west, anthropomorphic stone stelae are associated with the Kemi-Oba and Yamnya cultures around the third millennium BC (Telegin and Mallory 1994; Figure 13). Some major characteristics of these stelae such as the

necklace, hands placed on the abdomen and incised cattle

Atmospheric data from Reimer et al (2004);OxCal v3.10
Bronk Ramsey (2005); cub r:5 sd:12 prob usp[chron]

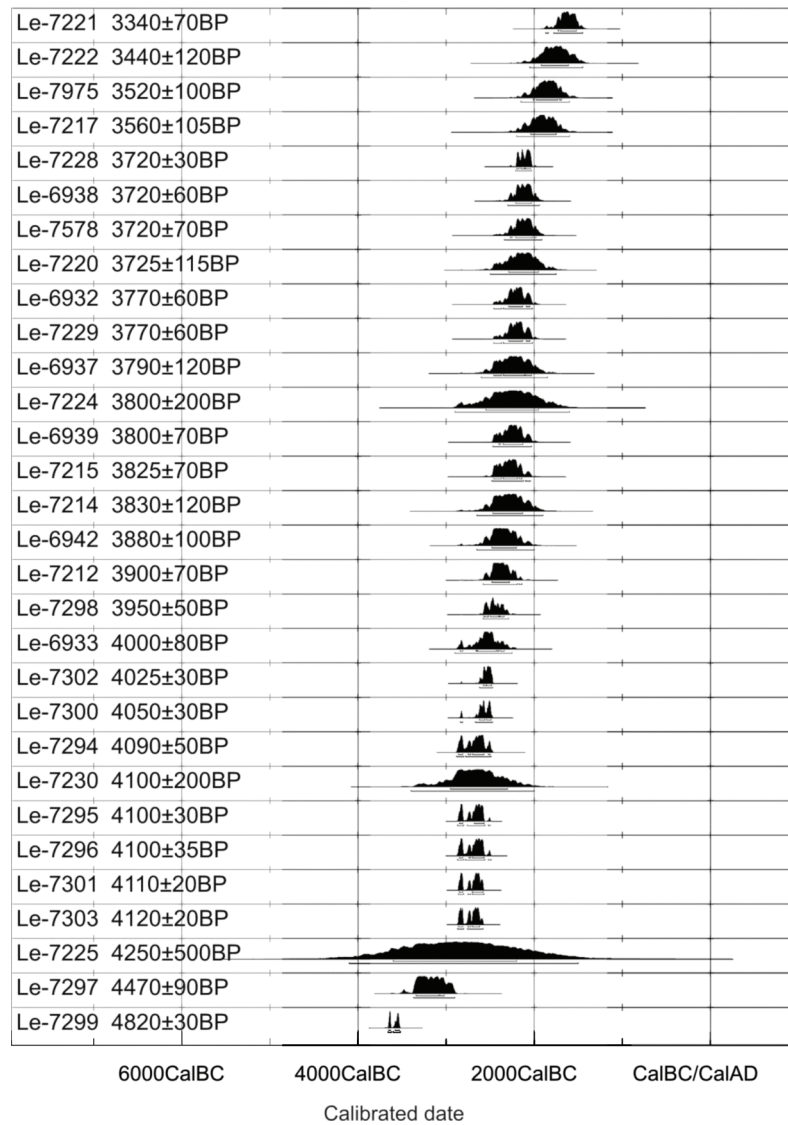


Figure 14: C14 dates from eastern Mongolia (based on Kovalev 2008, recalibrated using Oxcal v.3).

Table 4. C14 dates from east Mongolia (based on Kovalev 2008, recalibrated using Oxcal v.3)

EXCAVATED UNIT	LAB CODE	MATERIAL	UNCAL DATE (BP)	CAL DATE 68.2%(BC)
Buural kharynar, burial chamber	Le-7225	human bone	4250±500	3600-2200
Kara tumsik burial pit	Le-7302	charcoal	4025±30	2757-2485
Kara tumsik burial pit	Le-7303	charcoal	4120±20	2860-2620
Kheviin am 1	Le-7975	human bone	3520±100	2010-1690
Kheviin am 1, burial chamber	Le-7217	human bone	3560±105	2040-1750
Kheviin am 1, burial chamber	Le-7222	human bone	3440±120	1920-1610
Kheviin am 1, burial chamber	Le-7224	human bone	3800±200	2550-1950
Kheviin am 1, burial chamber	Le-7229	charcoal	3770±60	2290-2050
Kheviin am 1, burial chamber	Le-7230	wood	4100±200	2950-2300
Kheviin am 2, burial chamber	Le-7214	human bone	3830±120	2470-2130
Kheviin am 2? burial chamber*	Le-7228	charcoal	3720±30	2200-2040
Kulala ula 1 earliest burial pit	Le-7297	charcoal	4470±90	3340-3020
Kulala ula 1 earliest burial pit	Le-7298	charcoal	3950±50	2570-2340
Kulala ula 1 earliest burial pit	Le-7299	wood	4820±30	3650-3530
Kulala ula 1 secondary burial 1	Le-7220	human bone	3725±115	2290-1950
Kumdi gobi earliest pit	Le-7300	charcoal	4050±30	2620-2490
Kumdi gobi earliest pit	Le-7301	charcoal	4110±20	2850-2580
Kumdi gobi secondary burial 1 (the latest)	Le-7221	human bone	3340±70	1730-1520

EXCAVATED UNIT	LAB CODE	MATERIAL	UNCAL DATE (BP)	CAL DATE 68.2%(BC)
Kumdi gobi secondary burial 2	Le-7212	human bone	3900±70	2480-2280
Kurgak gobi 2 earliest pit	Le-7294	charcoal	4090±50	2860-2500
Kurgak gobi 2 earliest pit	Le-7295	Charcoal	4100±30	2850-2570
Kurgak gobi 2 earliest pit	Le-7296	Charcoal	4100±35	2850-2570
Kurgak gobi 2 secondary burial	Le-7215	human bone	3825±70	2460-2140
Yagshiin khodoo 1	Le-7578	human bone	3720±70	2270-1980
Yagshiin khodoo 1, burial chamber	Le-6937	human bone	3790±120	2460-2030
Yagshiin khodoo 1, burial chamber	Le-6938	human bone	3720±60	2210-2030
Yagshiin khodoo 2, burial chamber	Le-6942	human bone	3880±100	2480-2200
Yagshiin khodoo 3	Le-6939	human bone	3800±70	2400-2130
Yagshiin khodoo 3,	Le-6932	human bone	3770±60	2290-2050
Yagshiin khodoo 3, burial chamber	Le-6933	human bone	4000±80	2840-2340

*Question mark in the original table

icons on the front face of the stelae (Telegin and Mallory 1994:8-9) also appear on stelae found in Qiemu'erqieke Phase I. Recalling the oil burners that may have been inherited from the Yamnya culture and which are found in the Afansievo, Okunevo and Qiemu'erqieke Phase I, it might be possible to speculate that Qiemu'erqieke Phase I has its origins even earlier than the first half of the third millennium BC. This idea has also been suggested by Kovalev (1999).

Despite the affinities with the Okunevo cultural tradition, Qiemu'erqieke Phase I appears to be a discrete regional variant. The ceramic assemblage shows traits unique to this cluster of sites, while the anthropomorphic stelae are also distinctive markers of this tradition. There are no C14 dates available for the Qiemu'erqieke cemeteries, but some recently

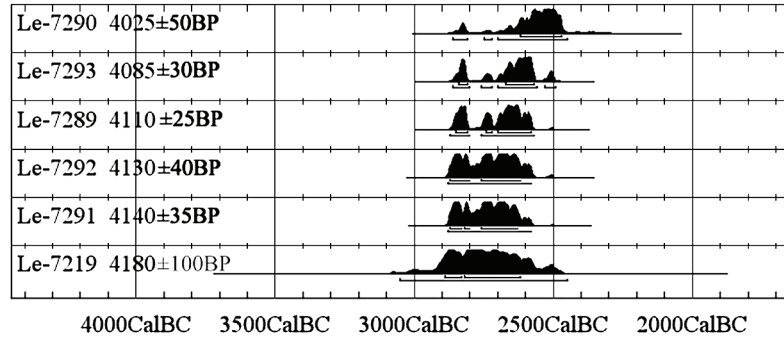


Figure 15: C14 dates for the Afanasievo tradition (based on Kovalev 2008, recalibrated using Oxcal v.3)

discovered cemeteries in western Mongolia, eastern Kazakstan and Russian Aletai have been identified by Kovalev and Erdenebaatar (Kovalev 2008:348; Kovalev *et al.* 2008) as relating to the Qiemu'erqieke (Phase I) tradition. The ceramics include flat-bottomed jars with incised decoration (Kovalev 2008: fig. 2.16; fig. 3, 9-11) which can be paralleled in the flat-bottomed shouldered jar with punctuate decoration from Qiemu'erqieke Phase I (Figure 7.14; 14), while the stone vessels (Kovalev 2008: fig. 2.16; fig. 3. 8) closely parallel those from Qiemu'erqieke. Burials are predominantly in stone cists, some with painted designs on the slabs (Kovalev 2008: fig. 2.16; fig. 3. 2), and anthropomorphic stelae closely resemble those of Qiemu'erqiek Phase I (Kovalev 2008: fig. 2.16; fig. 3.3). There are 30 carbon dates available from eastern Mongolia at sites believed to be affiliated to Qiemu'erqieke Phase I (Kovalev 2008) (Table 4). The date range (Figure 15) stretches across a period that begins from the late fourth millennium BC (around 3200 BC) and ends in the early second millennium BC (around 1700 BC) lasting 1500 years, overlapping with both the Afanasievo (Figure 16) and Okunevo cultures (see Table 1 and Table 5). Overall, if the three early dates are discounted, the range narrows substantially to fall within a mean of the mid third to early second millennium BC. Kovalev places the date range for his 'Chemurchek' sites at 2500-1800 BC (Kovalev 2008: 344). Of the three earliest dates, one, Le-7225, has a very high margin of error, while the other two, Le-7297 (3400-2900 calBC) and Le-7299 (3660-3520 calBC), come from the earliest of two burials at Kulala-Ula. The later burial at Kulala-Ula, Le-7220

(2500-1750 calBC), falls well within the dates given by Kovalev for the 'Chemurchek' sites. The first burial may be a very early precursor to the main cultural tradition.

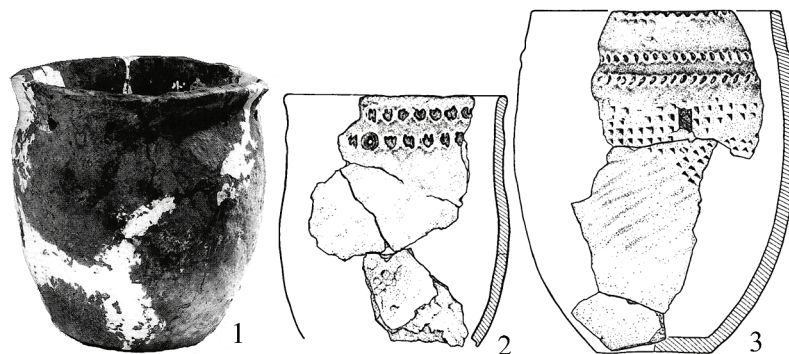


Figure 16: Comparison of ceramic jars from Qiemu'erqieke and Mongolia.

1. Qiemu'erqieke M16:4 (Xinjiang Bureau of Relics *et al.* 1999:339, Plate 0949); 2 and 3 (Kovalev 2008: Fig. 3. 10, 11).

Nevertheless, without further typological and chronological study of both Qiemu'erqieke Phase I and the apparently related sites in eastern Mongolia, it is not clear as to whether the start of Phase I in the Qiemu'erqieke cemeteries can be dated as early as the late 4th millennium BC. A date from the mid-3rd millennium BC or even slightly later may be more accurate. This supports the evidence from the material culture outlined above for broad affinities with the Okunvo. Phase I is likely to have come to an end before the beginning of the Karasuk around 1700 BC. Although it is still premature to draw any conclusions about the cultural roots of Qiemu'erqieke Phase I, there are hints that these lie to the west and may relate to the Yamnaya culture in some ways. It is now apparent that Qiemu'erqieke Phase I represents, as Lin Yun (2008) proposed, a cultural entity clearly distinct from the Afansievo-Okunevo sequence, although it displays affinities with these traditions, in particular the Okunevo. So far it is known only from cemeteries; no occupation sites have been identified. Its cultural heartland is the southern foothills of the Aletai mountains. It is the earliest of the known Bronze Age assemblages in Xinjiang and it may have spread its cultural influence southwards and eastwards through time. However, it is quite distinct from the Xiaohe and Gumuguo traditions. Based on evidence available to date, it is difficult to see

Qiemuerqieke Phase I as in any way directly ancestral to these largely aceramic oasis cultures.

Table 5. C14 dates for the Afanasievo in eastern Mongolia (based on Kovalev 2008, recalibrated using Oxcal v.3)

BURIAL	LAB CODE	MATERIAL	UNCAL DATE (BP)	CAL DATE (BC)
Kurgak gobi 1 burial pit	Le-7290	Charcoal	4025±50	2620-2470
Kurgak gobi 1 burial pit	Le-7293	Wood	4085±30	2840-2570
Kurgak gobi 1 burial pit	Le-7289	Charcoal	4110±25	2850-2580
Kurgak gobi 1 burial pit	Le-7292	Charcoal	4130±40	2870-2620
Kurgak gobi 1 burial pit	Le-7291	Charcoal	4140±35	2870-2630
Kurgak gobi 1 burial pit	Le-7219	human bone	4180±100	2890-2620

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Donn, Amairgen, Íth and the Prehistory of Irish Pseudohistory

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Although it is increasingly generally recognised that much of the narrative of Irish legendary history, including its overall framework, is to be understood in terms of Latin sources and imported concepts, this does not exclude the possibility that some elements within the scheme have an older, indigenous background. This paper focuses on three key figures in the account of the Gaelic settlement of Ireland - the brothers Donn and Amairgen, and their kinsman Íth - arguing that they reflect ancient origin myths with significant analogues elsewhere in Indo-European tradition.

Writing in the year 1919, Kuno Meyer concluded his criticism of various points in Arbois de Jubainville's 1884 book *Le cycle mythologique irlandais* with a sweeping dismissal of that study's approach to the Middle Irish treatise known as *Lebor Gabála*, the work which provided medieval Ireland with the most influential account of its legendary history:¹

...Unfortunately, a great part of Arbois' book is built upon similarly inexact interpretations - above all, however, on the aforementioned mistaken view that the story-telling (*Fabeleien*) of early Irish scholars is always somehow based on native saga and tradition. Thus he already refers in the first chapter, for example, to the content of *Lebor Gabála* as 'the Irish mythological cycle', and without more ado proposes comparisons with Hesiod's *Theogony*. The days when this fabrication (*Machwerk*) was regarded as the prehistory of Ireland are, it is to be hoped, gone for good; but it is also time to stop using it, without qualification, as a mine of Irish mythology and legendary history. It is van Hamel's achievement to have uncovered many of the sources - for

¹For some introductory discussion of *Lebor Gabála*, and of the Irish pseudohistorical tradition as a whole, see Carey 1994 and 2005.

the most part misunderstood passages from classical authors - from which the authors of *Lebor Gabála* derived their wisdom (Meyer 1919: 546).

Ninety-one years on, Meyer's expectations can be seen to have been a good deal too optimistic. Far from its being the case that the notion of using *Lebor Gabála* as evidence for Irish prehistory had been abandoned forever by the second decade of the twentieth century, it could be said that the idea is still ubiquitous in those areas of popular culture and 'fringe' speculation which concern themselves with such questions. That it has not been banished from the universities themselves can be illustrated with three examples, to which others could probably be added without much difficulty. T. F. O'Rahilly, one of the giants of Irish philology in the first half of the last century, is now best known in many quarters on the strength of his *Early Irish History and Mythology*, in which his formidable learning was harnessed to the task of correlating the settlements chronicled in *Lebor Gabála* with a postulated series of Iron Age migrations (O'Rahilly 1946: 75-77, 90, 99-101, 193-208, 263-266, 487, 492-496, 513; cf. O'Rahilly 1936).² Writing more recently, Richard Warner has argued for a historical basis for the story of the conquests of the purportedly second-century ruler Tuathal Techtmar (Warner 1995); and now, at the cutting edge of insular prehistory, the geneticist Stephen Oppenheimer is finding in *Lebor Gabála* - and, indeed, in the seventeenth-century *Annals of the Four Masters* - important confirmation not only for the overall framework of his model of the Irish past, but even for its chronology (Oppenheimer 2006: 87, 100-103, 109-110).

If *Lebor Gabála* and related sources can still be taken seriously as records of fact, it is not at all surprising that they also continue to be regarded as repositories of indigenous belief. Thus the approach of the brothers Alwyn and Brinley Rees in their 1961 book *Celtic Heritage* took it for granted that not only individual figures and episodes in *Lebor Gabála*, but also the structure of the whole, reflect a primordial Celtic vision of reality (Rees and Rees 1961: especially 95-117). Two decades earlier, Marie-Louise Sjoestedt was confident that although it was 'retouched, no doubt, by clerics anxious to fit

²Cf. Scowcroft 1988: 42 n. 118: 'It should by now be evident that O'Rahilly composed an *LG* of his own'.

the local traditions into the framework of Biblical history... its pagan quality has not noticeably been altered' (Sjoestedt 1982: 14). Proinsias Mac Cana was considerably more cautious in this connection, and acknowledged the pervasive importance of monastic learning in Irish legendary history. But even he concluded that 'despite its transparent fabrications, there is much in this account that is evidently traditional' (Mac Cana 1985: 62).

Other scholars, however, have carried forward the line of interpretation pioneered by A. G. van Hamel, and championed as noted above by Meyer. The late 1980s saw the publication of two extended articles on *Lebor Gabála* by Mark Scowcroft, which rank among the most significant and penetrating studies of that text. At the beginning of the second of these, in terms which echo a famous metaphor of Matthew Arnold's, Scowcroft reflected that

Students of early Irish tradition have too often pursued a kind of literary archaeology, excavating (sometimes creating) documentary ruins out of which to reconstruct pagan antiquity. Ignoring or dismissing the churches built of that same ancient stone, they may fail to see that their reconstructions rest on cruciform foundations: that traditions as suggestive to the historian as the Milesian invasion or as meaningful to the mythologist as the two battles of Mag Tuired belong in fact to the architecture of medieval learning rather than to its vernacular building-materials (Scowcroft 1988: 1).

Kim McCone cites Scowcroft's conclusions with approval, observing that 'the complex question of later developments in the extant recensions of the *Lebor Gabála* is best left to specialists, but there can be no doubt about the repeated use of the Bible and other ecclesiastical material in the formation and elaboration of this fundamental historical doctrine from the seventh century onwards'. He goes on to state that 'the broad outlines of pre-Christian Irish history were fashioned with the help of key features culled from the great biblical narrative up to the end of Kings' (McCone 1990: 68, 71).

It will, I think, be obvious to any careful reader of *Lebor Gabála* that this is the correct approach to take to that work as a whole. The Bible provides the overarching context within which Irish events are situated; and the parallelism between

the early Gaels and the Israelites, and between the successive settlements of Ireland and the 'world kingdoms' of Eusebius' *Chronicle*, are not only evident in themselves but are in fact explicitly acknowledged in the text. It could indeed scarcely be otherwise, for the *raison d'être* of *Lebor Gabála* - as of other pseudohistorical works produced in other countries - is to close the embarrassing gap between native tradition on the one hand, and on the other the imposing tableau of universal history which had been elaborated by the scholars of the early Church. Nor was assimilation of this kind an innovation of the Christian Celtic peoples. Thus Ammianus Marcellinus, writing in the fourth century AD and drawing to at least some extent on the writings of Timagenes of Alexandria (first century BC), has this to say about the origin legends of the pagan Gauls:

The druids, indeed, relate that a part of the people is indigenous, but that others besides these have poured in from the outermost islands and the regions beyond the Rhine, driven from their homes by frequent wars and the inundation of the furious sea. Some say that, after the destruction of Troy, a few of those who were dispersed in all directions fleeing the Greeks occupied these lands, which were then empty. But the inhabitants of these regions claim this most of all (which, indeed, we have read carved upon their monuments): that Hercules the son of Amphitryon made haste to destroy the cruel tyrants Geryon and Tauriscus, one of whom troubled Spain, and the other Gaul. After he had conquered them both, he united with women of noble birth and acknowledged many children as his own, giving their names to those regions over which they ruled (*Res gestae* XV.9.4-6).

That the doctrines here ascribed to the druids may go back to Timagenes' account, and may in fact give us a glimpse of the mythohistoric speculations of some of the pre-Christian Celts, are attractive possibilities. It is however significant that these ideas were no longer in the ascendant in Ammianus' day: for the Romanized Gauls of the declining Empire, descent from Hercules clearly meant more. It is also interesting that some of the Gauls adapted to their own uses the fiction of Trojan exile which the Romans had used as a link between their own obscure beginnings and the more prestigious traditions of the Greeks. Trojan origins were to be claimed throughout the

medieval period by several of the peoples of Europe, but it is among the Franks that that claim is first attested: Ammianus' testimony suggests that they may have taken the idea over from the Gallo-Romans among whom they settled.³

But it is time to return to the pseudohistorical lore of Ireland, and to the question of its background. Such sources as the Bible, patristic learning, and Graeco-Roman legend, adjusted and embellished by the imaginations of monastic scholars, can explain a great deal of what we find in a work such as *Lebor Gabála*. But they do not explain everything; and it is some items in this unresolved remainder that I wish to consider in what follows. We may begin with the article of Meyer's which was cited at the beginning of this essay: 'Der irische Totengott und die Toteninsel'. As noted already, Meyer held that 'in *Lebor Gabála* we have for the most part to do not with popular legend and tradition, which can - as unfortunately still often happens - be used without more ado to serve mythological ends, but with the self-conscious and systematic creation of classically educated scholars' (Meyer 1919: 538). He nevertheless believed that, in one case at least, it was possible to glimpse a bit of ancient belief behind the screen of learned fabrication: a figure named Donn, although assigned by *Lebor Gabála* to the patently artificial family of Míl Espáine ('the Soldier from Spain'; already present as the *miles Hispaniae* in the account of Irish origins in the ninth-century Welsh *Historia Brittonum*), still bore traces of a much earlier identity. Thus the poet Mael Muru Othna (died 887) said of Donn that

His cairn was raised, and the stone of his kindred, above
the broad sea: an ancient dwelling, a house of the waves,
which is called the House of Donn.

This was his mighty legacy to his abundant
descendants: 'To me, to my house, let them all come
after their deaths.'⁴

³Intriguingly, the passage quoted here is not mentioned in this connection by Ian Wood in his excellent essay on doctrines of Frankish origins. He does however cite Ammianus for the claim that the Burgundians were akin to the Romans (Wood 2006: 114).

⁴The translation is taken from a critical edition of the poem from all manuscripts, currently in preparation. Meyer's translation is essentially the same (Meyer 1919: 538-539).

After reviewing this and other pieces of evidence, Meyer concluded that

After all this, I believe that it is possible to consider it to have been established that the pagan Gaels believed in their common descent from an ancestor Donn, who was also the death-god, and whose dwelling was situated on an island to which all true Gaels came after their death. As ancestor and death-god, Donn is the exact counterpart of the Gaulish *Dis Pater*, who according to Caesar... likewise combined both functions in himself... This belief in an ancestor and death-god in one person is however not only Common Celtic, but Indo-European, as the Indic Yama testifies (Meyer 1919: 542-543).⁵

The correspondence between the Irish and the Gaulish figures is indeed a striking one, and most scholars have accepted Meyer's interpretation of the evidence. Our understanding of the significance which such a divinity may have held for the early Celtic peoples has been further enriched by a recent suggestion of Kim McCone's. After pointing to indications that, *pace* the objections of the currently vocal 'Celtosceptics', the term 'Celt' was in fact used as a collective designation in most parts of what has been conventionally regarded as the 'Celtic' sphere, McCone goes on to propose an etymology for the term itself: that it is

a *v̥rddhi* derivative meaning 'offspring of **k̥ltos* "the hidden one"... a likely enough title of the god of the dead from whom the ancient Gauls all claimed descent according to Caesar.... The same root is certainly attested among the Celts' Germanic neighbours in the name of the underworld and its presiding goddess (Old Norse *Hel...*) still surviving in English as *Hell*. *V̥rddhi* derivatives have a clearly defined meaning 'belonging to, consisting of, descended from', are invariably thematic (i.e. add an *-e/o-* suffix, if not already present in the base) and are produced by inserting *e* after the first consonant of the base's root.... **Keltos* could have been formed from **k̥ltos* in the manner envisaged no later than the Proto-Celtic period itself, from which the myth of descent from the god of the underworld would then presumably date. It

⁵Caesar's statement appears in his *De bello Gallico* 6.18.1f.: 'Galli se omnes ab Dite patre prognatos praedicant idque ab druidibus proditum dicunt'.

follows that the speakers of Proto-Celtic actually called themselves **Keltoi* ‘Celts’ (McCone 2008: 38-39; cf. McCone 2006: 95).

While any reconstruction of beliefs so deep in the preliterate past must to at least some extent remain speculative - a stricture which will apply in equal measure to the suggestions advanced below - I cannot see anything in the case outlined by McCone which is not well reasoned and eminently plausible. If he is correct, then the medieval Irish sources preserve, amid all their borrowings and concoctions, a recollection of one of the central myths of the Celts, from as far back in time as it is meaningful to speak of Celts at all.⁶

It is an arresting coincidence - if presumably no more than this - that one of the most important deities in the Egyptian pantheon, the divine sovereign *Amun*, also had a name which meant ‘the Hidden One’ (Budge 1920: i.51a; cf. Budge 1960: 194).⁷ As the designation of an underworld and/or death god, ‘Hidden One’ could have been a protective euphemism, a way of avoiding direct reference to a dreaded supernatural power. This may have been the original significance of *Donn* as well: the word’s normal meaning is ‘dark brown’, but there are lingering traces of an earlier sense ‘black, dark, dusky’.⁸ The same range of senses can be found for its closest cognate, the rarer Welsh adjective *dwinn*; and we can further compare words like English ‘dun’ and ‘dusk’, Latin *fuscus* ‘dark’ and Sanskrit *dhūsara-* ‘ash-coloured’ (Pokorny 1959: 270-271; Vendryes *et al.* 1959-: s.v. ‘donn’). As Käte Müller-Lisowski pointed out, the Greek word *θάνατος* ‘death’ may go back to an analogous euphemism: it appears to be related to such words as Sanskrit *dhvāntá-* ‘dark’, Avestan *dvānman-* ‘cloud’ and Latvian *dvans* ‘vapour, smoke’ (Müller-Lisowski 1952: 27; cf. Pokorny 1959: 266).

⁶See now John T. Koch’s argument that the word **keltos* may be attested in Tartessian, which he proposes as the oldest Celtic language (Koch 2009: 87).

⁷The root does not appear to have survived in Coptic, apart from the name *Amente* ‘Hades, underworld’. That *Hades* itself meant ‘invisible’, although the etymology is disputed, was a recurrent idea in antiquity: see e.g. Burkert 1985: 196; Kingsley 1995: 47 n. 43.

⁸Cf. Bruce Lincoln’s suggestion that the name *Donn* may be ‘simply a descriptive term which has replaced an older proper name’ (Lincoln 1981: 230). Lincoln’s insightful article, of which I only became aware when revising this paper for publication, anticipates several of my own conclusions.

Meyer was struck by the difference between what seems to have been Donn's original significance and the rôle accorded him in *Lebor Gabála*. Far from appearing in the latter as the ancestor of all the Gaels, Donn is there said to have drowned in the course of the second landing of the sons of Míl in Ireland, leaving no descendants after him and never himself obtaining possession of any part of the new land - a discrepancy so sharp that Meyer was moved to speak of a 'pseudo-Donn, thrust into the place of the old god' (Meyer 1919: 539). I believe, however, that there is much in the Middle Irish account which may in fact also point back to Donn the primordial ancestor.

Anyone reading *Lebor Gabála's* narrative of the Gaelic conquest can hardly help but notice that it consists of two parallel strands. The first of these concerns the doings of Éremón (ancestor of the Uí Néill, the Connachta, the Laigin, and other population-groups) and of Éber Find (ancestor of the Eóganachta and others), both with names derived from names of Ireland (Irish *Ériu* and Latin *Hibernia* respectively), and associated with such similarly eponymous siblings as Ír (ancestor of the Ulaid),⁹ Érennán and Érech, along with Colptha (eponym of the Boyne estuary). We hear about their landing places, their wives and sons and other descendants, and (briefly) about some of their deaths, but next to nothing apart from this: it is the same sort of genealogical and toponymic lore which makes so much of *Lebor Gabála* such pedestrian reading.

Only two of the sons of Míl Espáine have a further part to play in the story, and it is their deeds which make up its second strand: these are Donn, the eldest of the brothers; and Amairgen, the poet and judge, whose name may mean 'Born from Song'. They appear in conjunction, but also in opposition, representing the rival claims to authority of the warrior and the sage. When the advancing Gaels successively meet, on the tops of three hills, the three goddesses of the land, it is Amairgen who gains their good will by promising them that Ireland will be known by their names; Donn however defies them, saying that 'We will not thank her for it, but our own gods and powers'. Speaking apparently under inspiration, Amairgen declares that the Gaels must put to sea once more, go nine waves from shore, and then land again to

⁹There may however be more to the figure of Ír; see below.

take definitive possession of the island. When they are then brought into difficulties by a magical wind raised by the Tuatha Dé Donann, Donn calls this ‘a shame to the men of art’, only to have Amairgen calm the storm with a chant in which he invokes the land itself in terms of its various features, concluding with a recognition that Ireland is personified in the ‘vast woman Ériu’. Filled with an exultant arrogance, Donn cries ‘Now I will bring under the edge of spear and sword all that there is in Ireland’, whereupon the wind drives his ship upon a sandbank so that he drowns.¹⁰ And so it is Amairgen who, in an anecdote found at the beginning of the saga *Mesca Ulad*, divides Ireland between the Gaels and the old gods (LL 34591-7).

I accordingly propose a division of the sons of Míl into two groups: a collection of two-dimensional ancestor figures, whose artificial-looking names are almost all that we know about them; and Donn and Amairgen, who are the central characters in the actual story. These feature as a pair as far back as the early eighth century: in the cryptic treatise known as ‘The Caldron of Poesy’, the author claims the identity of ‘white-kneed, blue-shanked, grey-bearded Amairgen’, and says that the nature of poetry should be made known ‘in order to compose poetry for Éber Donn (*do Hébiur Dunn*) with many great chantings’ (Breatnach 1981: 62 §1). The text’s editor, Liam Breatnach, has followed one of its Middle Irish glosses in understanding ‘Éber’ and ‘Donn’ as two distinct names here, juxtaposed in asyndeton (Breatnach 1981: 78); but this interpretation seems to me unlikely. For one thing, ‘Éber Donn’ is well attested as a designation for Donn, appearing for instance several times in *Lebor Gabála* (thus LL 1533, 1539, 1600, 1612, 1629). For another, it is hard to understand the rationale behind the pairing postulated by the gloss. Éber (Find), ancestor of the chief dynasties of the south, is normally mentioned in conjunction with Éremón, his counterpart in the north:¹¹ why associate him instead with

¹⁰For a convenient translation, based on my unpublished edition of the first recension of *Lebor Gabála* (Ph.D. diss., Harvard 1983), see Koch and Carey 2003: 266-267; text in Macalister 1956: 34-39, 52-57, 70-81. The encounters with the three goddesses are also mentioned by Mael Muru (thus Best *et al.* 1954-1983 [hereafter LL], lines 16081-2), and by the Middle Irish *Lebor Bretnach* (Hamel 1932: 26).

¹¹Besides instances of their being treated as a pair in *Lebor Gabála* itself, see e.g. ‘Fiacc’s Hymn’, where the pagan Gaels are collectively called ‘the sons of

Donn, a figure with no dynastic significance, and one who - confusingly - could also be called Éber himself? These objections are probably not insuperable; but it seems considerably simpler to bypass them by translating *do Hébiur Dunn* straightforwardly as 'for Éber Donn'.

If we detach Donn and Amairgen from their brothers in this way, and view them as the protagonists of an older settlement legend,¹² a number of intriguing analogies with other Indo-European traditions suggest themselves. Meyer, as we have seen, compared Donn and the Gaulish Dis Pater with Yama in Indian mythology: also both god of the dead and ancestor of mankind. *Yama* is in fact the Sanskrit word for 'twin', from a root **yem-* which also yielded Latin *geminus* and Irish *emon* (Pokorny 1959: 505). In the *R̥g Veda*, the other twin is Yama's sister Yamī, who offers herself to him as a mate only to be rejected on the grounds that such a union would be incestuous: since it is however stated that they were the only human beings in the world, it seems likely that this hymn is an adaptation of an older tradition in which this primordial couple did in fact engender the rest of humanity (*RV* 10.10).¹³ In the later *Purāṇas*, Yama also has a brother Manu: while Yama becomes god of the dead, Manu is celebrated for his wisdom and becomes a lawgiver and, as survivor of the Hindu flood myth, the ancestor of mankind (O'Flaherty 1975: 50, 65-70, 179-84). The parallelism between Yama and Donn on the one hand (primeval ancestors and lords of the dead), and Manu and Amairgen on the other (judges and survivors), is close enough to raise the question whether they may not in fact reflect the same Indo-European myth.¹⁴ The root underlying

Éber, the sons of Éremón' (Stokes and Strachan 1901-1903: ii.316); and the tale *Airne Fíngéin* (Vendryes 1953: 11-12).

¹²Cf. Mac Cana 1985: 62-63: 'Amhairghin, like Donn, is wholly mythological'. Lincoln too notes that it is these two who 'stand foremost' among the sons of Míl (Lincoln 1981: 228).

¹³Translated with discussion in O'Flaherty 1975: 62-65. For indications that both Indian and Iranian traditions have attempted to disguise an earlier account in which the siblings did in fact unite see Schneider 1967: 18-19.

¹⁴Cf. Lincoln's observation that Donn stands to Amairgen 'in the relation of king to priest' (Lincoln 1981: 228). He sees their story as representing the Irish reflex of an Indo-European mythologem, already examined by him in an earlier article, according to which 'the world begins with a pair of twins, *Manu, "Man", and *Yemo, "Twin", *Yemo being characterized as the first king, while *Manu is the first priest, and in the course of the myth, *Manu offers *Yemo as the first sacrificial victim' (Lincoln 1975: 139).

Yama is also found in the name of *Ymir*, the androgynous giant who figures as the first being in the Icelandic *Gylfaginning*;¹⁵ and according to Tacitus the ancient Germans held that *Mannus*, the universal ancestor corresponding to Indian Manu, was the son of a god *Tuisto*, born from the earth, whose name comes from the root meaning 'two' and evidently also had associations with twinhood (*Germania* 2.3; cf. Maier 2003: 63).

A formidable body of scholarship has been devoted to these and cognate traditions (e.g. Ward 1968, O'Brien 1982), and I will make no attempt to do justice to the whole of it here: even a consideration limited to the specifically Celtic evidence would carry us too far afield.¹⁶ For our present purposes it may suffice to allude quickly to the Dioskouroi in Greek mythology, the twins Castor and Polydeuces, who take turns dwelling in the underworld; to the brothers Hengist and Horsa, said to have crossed the sea to Britain where Hengist established the line of the kings of Kent while Horsa died without issue; and, of course, to Romulus and Remus in Roman legend. Of this last pair, again, one twin establishes a royal line while another perishes - that Remus is killed after derisively leaping across a boundary trench may be compared with Donn's death following his hubristic words just before the decisive landing in Ireland (Livy, *Ab urbe condita* 1.7). It would probably be chimerical to attempt to combine all of this evidence in the quest for some single archetypal foundation myth, from which the story of Donn and Amaingen can be derived. Even if we abstain from such an exercise, however, it seems to be sufficiently evident that this body of material exhibits elements which, again and again, find parallels in the story of the Gaelic settlement.

A further Celtic datum has not, it seems to me, so far received the attention which it deserves in this connection. According to Diodorus Siculus,

the Celts dwelling beside the ocean honour the Dioskouroi most of all the gods; for it has been handed down to them from ancient times that these gods came

¹⁵Snorri Sturluson 1988: 10-12; cf. the earlier allusions to this figure in *Völuspá* §3, *Vafþrúðnismál* §§21, 28, *Grímnismál* §40 (Kuhn 1983: 1, 48-49, 65); and discussion in Turville-Petre 1964: 275-278, 319 n. 11.

¹⁶Thus I will not discuss Lincoln's stimulating argument for an association between Donn son of Míl and Donn the bull of Cuailnge (Lincoln 1981: 229; Lincoln 1981a: 87-92).

from the ocean (*Histories* 4.56.4).

Not only do we find particular reverence being accorded to two divine brothers, then, but also a story of how they came from across the sea. It may be that we have here the last faint trace of a Gaulish counterpart to the story of Donn and Amairgen.

There is one respect in which the Irish story differs from most of those with which I have compared it. Amairgen is not a ruler and dynastic founder, like Hengist or Romulus; or one of a pair of heroes, like Castor or Polydeuces; or a universal ancestor, like Manu. He is, rather, a prototypical poet and legal expert, an embodiment of the powers of the word, set over against his elder brother Donn, the leader and man of action. This polarity recalls Caesar's celebrated generalization that the only two classes of any consequence in Gaulish society were the druids and the *equites*, or horse-riding nobility (*De bello Gallico* 6.13); less remotely, we can recall the semi-allegorical Middle Irish anecdote about the birth of Aí, or 'Poetry', the son of Ollam, or 'Master Poet', and Aí's precocious demands upon the generosity of Ollam's brother the king Fiachu of the Tuatha Dé Donann (Thurneysen 1936: 193-194; trans. Koch and Carey 2003: 222).¹⁷ In this story about two brothers, one a poet and the other a ruler, the agenda is to assert the ultimate supremacy of the former over the latter; and it is plain that the story of Amairgen and Donn has a similar message. It may well be that the *filid* or professional poets, in transmitting this ancient tale, gave it a slant calculated to enhance the standing of their own profession.¹⁸

In his own comments on the passage from Diodorus, Bernhard Maier has suggested that 'a connection between this idea of the gods and the system of the double kingship occasionally mentioned by ancient sources in connection with various Gaulish tribes is in any case conceivable' (Maier 2001: 76). This is an intriguing idea. Maier goes on to caution that the examples of this institution which have been proposed

¹⁷That this story may draw upon very old tradition is suggested by its association of Aí's birth with a strong wind: although there is no trace of this in the word's attested usage, *aí* derives from a root **h₂wē* 'to blow'.

¹⁸This contrasts with the situation which according to Donald Ward obtains elsewhere in the Indo-European world, where one of the twins represents Dumézil's 'third function' (Ward 1970); Ward's own position represents a departure from that of Dumézil himself, who had assigned both twins to the third function.

'are not all to be interpreted in this sense: while Ambiorix and Catuvolcus, according to Caesar's account, ruled jointly over the Eburones, the cases of Dumnorix and Diviciacus..., Indutiomarus and Cingetorix..., as well as Cavrinus and Moritasgus manifestly involve competing antagonists' (Maier 2001: 195 n. 171). The analogy of the Irish legends, however, suggests that these alternatives were not always clear-cut: are Donn and Amairgen allies, or rivals, or both?¹⁹ In the case of the Aedui, it is certainly suggestive that of the two brothers contending for primacy, Dumnorix and Diviciacus, the latter is said by Cicero to have been a druid (*De divinatione* 1.41.90); but this statement has been challenged by some scholars and is in any case too isolated to serve as the basis for broader conclusions (thus Maier 2009: 35-36). More promising, perhaps, is Caesar's portrayal of the Aedui as having had leaders of two kinds: chieftains like Diviciacus and Dumnorix, who owed their standing to birth and to the number of their followers, and who acted as leaders in war; and the supreme magistrate known as the *uergobretus*, 'who is appointed for a year and has power of life and death in their affairs' (*De bello Gallico* 1.16). Whatever may be the sense of this word's first element, *-bretus* clearly designates a judge of some kind; that the *uergobretus* was not an institution peculiar to the Aedui is indicated by the term's occurrence, in the form VERCOBRETOS, in an inscription from the adjacent territory of the Bituriges.²⁰

But let us return to Ireland, and to *Lebar Gabála*. There is one other character in the account of the Gaelic settlement whose adventures are related in some detail: this is the tragic figure of Íth son of Bregon, whose wrongful death is the catalyst for the invasion. His story has various aspects. In part, it is as clear an example of the latinate artificiality of the pseudohistorical schema as one could wish for: when Íth ascends the tower built by his father Bregon in Spain, and gazes across the winter sea to Ireland, he is ascending the

¹⁹For the implicit rivalry between the brothers, present also in the case of Romulus and Remus and, in a non-Indo-European tradition, in that of the twins Jacob and Esau, see Emily Lyle's chapter 'The place of the hostile twins in a proposed theogonic structure' (Lyle 1990: 105-118); for this reference I am grateful to Máire Herbert.

²⁰Allain, Fleuriot and Chaix 1981; on p. 21, Léon Fleuriot identifies the first element as 'le mot *uergo* "acte", ou "acteur", auquel correspond le vieux-breton *guero* glosant *efficax* et beaucoup d'autres mots'.

Roman lighthouse (still standing) of *Brigantia*, the modern Coruña in Galicia, and taking advantage of Orosius' statement that the nearer regions of Ireland 'gaze from afar (*spectant*), across an extensive distance, at the city of Brigantia in Galicia' (*Historiae* 1.2.80-81; Zangemeister 1889: 12). By *spectant* Orosius may have meant only that the southern parts of Ireland face toward northwestern Spain: but there was certainly scope for a more literal understanding of his words, and this is how they were in the event understood. Already in *Historia Brittonum*, written early in the ninth century, we find evidence that Irish pseudohistorians combed sedulously through Orosius' account of world geography for material to use in their accounts of the continental wanderings of the Gaels (*Historia Brittonum* §15, Mommsen 1898: 157; cf. Orosius, *Historiae* 1.2.87-94, Zangemeister 1889: 13-14); the same thing has clearly been happening here.

When Íth has crossed the sea and come to Ireland, however, it is analogies of a different kind which suggest themselves. Traversing the island from south to north, he comes to Ailech Néit where the three Tuatha Dé Donann kings, husbands of the land-goddesses Ériu, Fóitla and Banba, are arguing over an inheritance. In the words of *Lebor Gabála*:

Íth surpassed the judges of Ireland in cleverness and pleading, and he righted every complaint and contention that there was among them, and he said: 'Enact rightful law, for you dwell in a good land. Abundant are its mast and honey and wheat and fish. Balanced are its heat and cold.' Then they plotted to kill Íth, and they banished him from Ireland; and he departed from them in Ailech and came into Mag nÍtha. Emissaries followed him, and slew him there in Mag nÍtha, whence it is called 'the Plain of Íth' (trans. Koch and Carey 2003: 263; cf. Macalister 1956: 10-21).

The reason for the treacherous ingratitude of the three kings is not spelled out in the text, but can be inferred in light of the ideology of early Irish kingship. Thus the celebrated wisdom text *Audacht Morainn* enjoins a ruler to judge justly, for his land and people will only flourish if he does so; and also to 'estimate [i.e. assess, evaluate] the creations of the Creator who made them as they were made; anything which he will not judge according to its profits will not give them with full increase' (Kelly 1976: 12-15 §32). Íth shows that his powers of

judgment are greater than those of the judges of the kings, and evidently of the kings as well; and he is able to recognise, and to describe, the virtues of the land. Through speech, in other words, he shows himself to be better fitted to be a king in Ireland than are those who exercise the rule: the latter perceive this as a challenge, and Íth pays with his life. We may also observe that Amairgen is distinguished in just the same way: his are the decisive judgments, to which even the three kings declare themselves ready to submit; and it is his chanted description of Ireland which causes the magical wind to be stilled, allowing the Gaels to take possession of their new realm.

But if this part of the story of Íth carries us back to traditional conceptions of Irish kingship, his name may carry us further still. Formally, it is identical with a noun *íth* which means 'fat, lard, grease'. Such a name might seem less appropriate to an illustrious Gaelic pioneer than to the Middle Irish satirical work *Aisling Meic Con Glinne* (where an Íth does figure in the 'genealogy of food' which links Mainchín of Cork to Adam; Jackson 1990: 13); but it is in fact appropriate, on a deep level. I think that it is John T. Koch who first pointed out in print the significance of the fact that *íth* 'fat' appears to have the same root as that which John Rhŷs proposed for *Ériu* 'Ireland', both deriving from the Indo-European root **peiH-* (Koch 1986: 7-9, 1991: 22-23; cf. Rhŷs 1873-1875: 196). Ireland would accordingly be the 'fat' country in the sense that it is fertile and abundant: a semantic spread also reflected in the pairing of *íth* 'grain', originally 'food' (from the zero-grade **pitu-*) and *iath* 'land, territory' (from the *e*-grade **peitu-*; Pokorny 1959: 794).²¹ The same suffixation with *-er-* has yielded Sanskrit *pīvarī*, the feminine form of *pīvan* 'fat', and Greek *πείρα*, feminine of *πίων* with the same meaning. It is interesting that Ireland's name seems, following this hypothesis, to go back to a specifically feminine Indo-European form; and even more interesting that Greek *πείρα*, although it can be used of feasts, is most frequently applied to fruitful land. The same association evidently lies behind the Thracian placename *Pieria*.

²¹In this connection Máire Herbert has called my attention to a Middle Irish toponymic tale in which, as the girl *Ériu* ('Ireland') is fed, the hill on which she is sitting grows: Stokes 1893: 490 and 1895: 490; Gwynn 1903-35: iv.184-187.

The vocalism of *Ériu* is a problem, and McCone has recently revived Osborn Bergin's suggestion that this name should rather be understood to have been 'taken over from pre-Celtic inhabitants speaking a non-Indo-European language' (McCone 2008: 11; Bergin 1946: 152-153). But this position leaves out of account the existence of a doublet *íriu*, used as a common noun meaning 'land, earth, world' - clearly cognate with Welsh *Iwerddon* 'Ireland', and readily comparable with Sanskrit *pīvarī*, Greek *πίερα* and Thracian *Pieria*. In light of this evidence, Graham Isaac has put forward a more complex analysis in support of Pokorny's alternative derivation of *Ériu* from **h₁epi-uerion-*, which Isaac himself understands to have meant something like 'by the stream (of ocean)'. This protoform, becoming Celtic **ēweriyū*, would have yielded Irish *Ériu* and Welsh *Ywerddon* (the form attested in thirteenth-century manuscripts), while **īweriyū*, from the 'fat' root, lies behind Irish *íriu* and Welsh *Iwerddon* (which latter he takes to have been originally, like *íriu*, a common noun 'land', which fell together with, and ultimately replaced, *Ywerddon* through natural confusion). Greek *Ἰέρπη* is not evidence that the name for 'Ireland' originally began with *i*, but is simply due to borrowing into a language in which no lexemes began with *ie-* (Isaac 2009).

Isaac's argument is impressive; but its acceptance involves positing the coexistence, since the beginnings of insular Celtic, of two forms **ēweriyū* 'Ireland' and **īweriyū* 'land'. A certain degree of overlap, if not coalescence, would have been virtually inevitable from a very early date - as illustrated by the postulated borrowing of *ē(w)er-* into Greek as *ἔρ-*, and the further postulated falling together of *Ywerddon* and *Iwerddon*. So far as I can see, then, this latest ingenious reading of the evidence does not significantly alter the case which Koch has advanced. Íth's name can scarcely have arisen by an obscurely motivated coincidence, and the idea of *Ériu* as the 'fat land' - whether this was indeed the name's original meaning, or arose through an early reinterpretation - provides it with an explanatory context. The first Gael to die in Ireland appears to have a name associated with that of Ireland itself.

How old might such an association be? In the nature of things, we cannot hope for any even approximate chronology here. It is however telling that the Celtic languages, unlike Sanskrit and Greek, have not preserved any associations with

'fat', etc., for derivatives of the root **peiH-* with a suffixed *-r-*: these only have the sense of 'earth' or 'territory'. This indicates that a link between **(p)īwer-* and **(p)īt-* had probably already become opaque at a point very early indeed in the development of Irish as a distinct language.

Irish tradition appears to preserve some further traces of the mythological/semantic nexus reflected in the names of Ériu and Íth. Íth is said in the genealogical literature to have been the ancestor of various peoples of the *Érainn*: an ethnic designation which, although its true significance is largely obscured by medieval propaganda, evidently designates a population-group centred in Munster which enjoyed considerable importance prior to the rise of the Uí Néill and Eóganacht in the fifth and sixth centuries. One tract attributes two sons to Íth: Iar and Lugaid (O'Brien 1976: 372).²² The name Iar appears elsewhere in the genealogies of the *Érainn* as well (O'Brien 1976: 188-189, 329, 378),²³ and the existence of a personal name Mac Iair bears further witness to his early importance as an ancestor figure (O'Brien 1976: 16, 27, 40, 82, 85, 140, 196, 215, 315, 341, 414, 431).²⁴ Genitive *Iair*, *Ieir* rather than *Éir* shows that the name was disyllabic, going back to a protoform **(p)īweros*.²⁵ If from **peiH-*, this formation is evidently secondary, involving as it does a masculine derivative of originally feminine **pīwer-*; but it is still old enough for the connection with such names as *Ériu* and *Érainn* no longer to be readily apparent. Iar may in turn lie behind the figures Ír and Éir, who in different accounts of the Gaelic settlement occupy a role closely similar to that of Íth in that they are severally said to be the first of the sons of Míl to die in, or while approaching, Ireland.²⁶

²²A paragraph concerning the two brothers gives the form as *Hír*; but in the immediately preceding pedigree the name appears in the genitive as *Ier*, *Iair*, *Hiair*. Cf. O'Rahilly 1946: 82 n. 3. In the early stratum of 'O'Mulconry's Glossary' the *Érainn* are called *tuatha Ier* 'peoples of Iar': Stokes 1900: 254 §417.

²³The name's presence in an Osraige pedigree (O'Brien 1976: 16) may reflect the overlordship which the Corcu Loígde are said to have exercised over that kingdom in the sixth and seventh centuries (Radner 1978: 2 and note on p. 185).

²⁴Note further MAQQI-IARI in an *ogam* inscription from Ballintaggart, Co. Kerry: Macalister 1945: 152 §156.

²⁵There is in fact one instance of genitive *Heir* (O'Brien 1976: 329).

²⁶Ír: Koch and Carey 2003: 264; Macalister 1956: 30-31, 72-3. (It may be significant that a curse uttered by Donn, comparing Ír with Íth, appears to be

Lugaid son of Íth appears in the genealogies as ancestor of the Corcu Loígde, the most prominent of the Érainn peoples: over-used by some scholars though such an explanatory strategy may have been, it does not seem far-fetched in the present case to surmise that he is ‘ultimately the same’ as the eponymous Lugaid Loígde, and as the latter’s son Lugaid Mac Con.²⁷ All of these names of course recall that of the god Lug; and both Lug and Lugaid Mac Con were closely, if variously, associated with the kingship of Tara.²⁸ That Lugaid son of Íth was prominent in early versions of the Gaelic origin legend is suggested by *Lebor Gabála*’s description of him as ‘the harsh violent mighty warrior with the strength of a hundred’ (Koch and Carey 2003: 264; Macalister 1956: 28-29): no other member of the conquering expedition is accorded a description of this kind, even though Lugaid is scarcely mentioned in the rest of the account.

After all of this, what are we left with? A handful of names, a few attributes and sketchy narrative associations, and no certain conclusions. We are, and will almost certainly remain, a long way from being able to reconstruct an archetypal myth of Gaelic origins - even assuming, and this seems to me unlikely, that some single definitive doctrine on this subject ever existed. But we have at least found clues, and suggestive ones, which indicate that some elements in the Irish pseudohistorical construct come to us, not from Orosius or Eusebius or Genesis, but from ancient native stories of the first settlement of the land: stories which find echoes in beliefs attributed to the druids, and which may in part go back as far as the naming of Ireland itself.

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²⁷Discussion and references - going however considerably beyond what is suggested here - in O’Rahilly 1946: 77-82.

²⁸Thus Tomás Ó Cathasaigh sees an identification of the two in terms of Corcu Loígde claims to Tara: ‘[Lugaid] is, so to speak, *their* Lug, deposing Art (of the Dál Cuinn) from the kingship’ (Ó Cathasaigh 1977: 38 n. 147). On Lug and Tara, see further Carey 2005a: 41-44.

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Some Greek Evidence for Indo-European Youth Contingents of Shape Shifters

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Artistic evidence, legends and anecdotes in Greece seem to reflect very archaic ways of fighting, remnants of IE youth contingents covered in skins, lightly armed, and inspiring terror. Although these troops had largely disappeared from the written sources in archaic and classical times, there are enough indications to suggest that they operated past Iliadic times, even if they were no longer well understood.

Shape shifting warriors are known from several IE cultures. Scholars assume that there were probably two types of shape shifters. On the one hand there was a “consecrated” warrior,¹ and on the other hand there were contingents of young adolescent men, who fought for a period of time as shape shifters before they were integrated into the productive structure of their societies (Kershaw 2000: 63).

In a seminal work, Kim McCone identified the contingents of young marauding men in several IE cultures. He established a distinction between them and the organized troops of property owning citizens (**teutá*) (McCone 1987: 111). These young men gathered in the so-called IE **koryos*. Members of the **koryos* spent their time in the theft of cattle and women, activities which constitute the themes of most IE epics.

Possible traces of these young men and their paraphernalia are found in the *Iliad* in the “long-haired” Achaeans, the

¹The best examples for the two separate groups of shape-changing warriors come probably from Tacitus’ *Germania*. According to Kershaw (2000: 41), Tacitus describes the young adolescent warrior in *Germania* 43. As for consecrated warriors, Tacitus describes in chapter 31 the tribe of the Chatti. Tacitus writes that as soon as the Chatti became men, they let their hair and beard grow and would not cut it until they had killed an enemy. They always fought in the front ranks and did not temper their aspect even during peace. They did not possess land or houses and lived from what others gave them. Cf. Egil’s Saga 3: Harald had sworn a solemn oath never to cut or comb his hair until he made himself sole ruler of Norway. That was why he was called Harald the Shaggy.

Myrmidones, and of course the Lykians, Wolf-People, possibly representing some IE Männerbünde (McCone 1987: 122). The *Iliad* has also preserved a word for them, *koiranoi*.² The evidence of the *Iliad* seems sufficient to claim some youth contingents for Greek prehistory. When it comes to Greek history, however, the evidence seems much harder to find. In this paper I would like to investigate the survival of IE contingents of young warriors in Greek archaic and classical times. I will examine the evidence in literature, myth, history, and art from a comparative perspective.

Through comparison with other IE cultures it has been established that contingents of young warriors often assumed some kind of shape shift. Here are the most important characteristics of the shape shifters, which, I will argue, are also present among Greek youth contingents not only in Homeric times, but in archaic and even classical times.

1. Most important is the use of animal skins, which allowed the warriors to assume the nature of the animal.³
2. Other sources mention their long hair or strange hairdos, which was supposed to give them a horrifying aspect.
3. They also impersonated ghosts or dead ancestors.
4. As a result of the physical change there was a psychological change that made them fight defiantly, naked or semi naked, armed only with light weapons.
5. Also, the change made them fight in a state of fury that increased their performance above natural expectations, but left them worn out for a long period after their frenzy.⁴
6. Their ecstatic performance was accentuated by dances and perhaps the use of drugs.

²*Il.* 2. 207, 2. 487, 2.760, 4. 250, 5. 824, 7. 234.... According to Kershaw (2000: 18), Greek *koiranos* is the exact parallel of Old Norse epithet of Odin *herjann* – “he who embodies or leads the *herr*.” In Norse mythology the *herr* is the army of the dead that gathers in Valhalla until the end of times in Ragnarokr.

³The most commonly known “style” of shape-changing warriors among the IE peoples are the Norse berserkers, said to wear a bear-skin or to be naked and to scorn the use of armor. Yet, the most common frenzied warriors were the wolf warriors, who also wore around their shoulders the skins of wolves and/or a cap of the same material. They are called *ulfhedinn* – “wolf coat” in the sagas (Speidel 2004: 15).

⁴Egil’s Saga 27: “What people say about shape-shifters or those who go into berserk fits is this: that as long as they are in the frenzy they are so strong that nothing is too much for them, but as soon as they are out of it, they become much weaker than normal. That is how it was with Kveldulf; as soon as the frenzy left him he felt worn out by the battle he had been fighting, and grew so weak as a result of it that he had to take to his bed.”

From the *Iliad*, we know that the ancient Greeks had some kind of appreciation that shape shifters had been part of their past. It is not uncommon to see some of the warriors of the *Iliad* fighting in a state of frenzy.⁵ There are two words to describe this state: *menos* and *lyssa*. Although *menos* is primarily a state of altered mind⁶ (it is etymologically related to the IE root **men-* to think), it is, nevertheless, associated with animalistic traits. For instance, in book five of the *Iliad*, Diomedes is characterized by his receiving the extraordinary force of *menos* that allows him to continue fighting without paying attention to the wounds he has just received. Diomedes' *menos* is so intense that he even attacks gods in his fury, which has a special character, as we read in *Il.* 5. 136: *then a force (menos) three times as great seized him, as if he was a lion.*

In book 10 of the *Iliad*, Diomedes' description as a lion is matched by him actually putting on over his shoulders a large lion skin that reaches to his feet (*Il.* 10. 177). Later on (*Il.* 10. 255-258), he wears a bull helmet. The helmet is described as a *kynēēn taureiēn*, literally a dog-hide cap in shape of a bull.⁷ This is a special type of helmet⁸ for young people –*aizēōn*.⁹ *aizēoi* is a strange term, without known etymology, but that the ancients understood as compound of *aei* + *zēn* – to live eternally. From this short description we can see that Diomedes presents an altered state of mind (*menos*), animal characterization and perhaps invulnerability, all characteristics of shape shifters. In the same book, the Trojan Dolon, opponent of Diomedes, is covered with the skin of a wolf and a dog-hide cap in the shape of a marten (*Il.* 10. 334-335).¹⁰

Homeric warriors were conscious that they could be possessed and transformed by a power similar to that of animals,

⁵Hector (*Il.* 7. 38, 17. 565...), Idomeneus (*Il.* 13. 424) and naturally Achilles (*Il.* 11. 268, 21. 305...)

⁶Dumézil 1983: 185 argues that the warrior inflamed with *menos* does not lose his lucidity, since he can still heed advice. He also argues that dressing up with an animal skin does not turn a warrior into an animal.

⁷However, *Il.* 3. 316 – bronze *kynēē*. Obviously, the noun had become a common way to designate a smaller helmet.

⁸Odysseus boar's tusk helmet is also mounted on a dog-hide (*Il.* 10. 261).

⁹There are 17 instances of this word in the *Iliad*. Most of them refer to young men during the hunt of wild animals with dogs.

¹⁰In the Saga of the Volsungs chapter 8, Sigmund and Sinfjotli put on some wolf skins that transformed them into howling wolves with superhuman powers. Marten warriors are not unknown in other IE groups, see Speidel 2004: 51.

as exemplified by the word *menos*, which primarily describes a mental state, that of madness. Consequently, Diomedes in *Il.* 5. 185 is described as a person who is mad with *menos*: (*mainetai*- he is enraged or possessed).¹¹ *mainetai* is used to describe the possession by an outside force: a god, fury, poetic inspiration ... *mainetai* and its related word *mania* are often associated with frenzy in particular of the Dionysian kind. On the other hand, in the *Iliad* *mainetai* still refers predominantly to war frenzy.¹²

Besides the mental transformation expressed through *menos*, there is yet another word describing animal possession and the subsequent shape shift of the warriors, namely *lyssa*, fury. *lyssa* is a feminine abstract of the word *lykos*- wolf (Lincoln 1991:131). In the *Iliad*, *menos* and *lyssa* do not appear together, but both terms of fury appear often in combination with *mainetai* or etymologically related words. For instance, Hector in book *Il.* 9. 237-239 is said to be enraged (*mainetai*) and not to respect men or gods in his fury, which is termed *lyssa*. Also *Il.* 21. 542- 543, Achilles' heart is possessed by fury (*lyssa*) as he intends (*meneainei*) to win glory. Furthermore, the scholia define *lyssa* as *mania*.¹³

Whereas shape-changing warriors were not unknown to Homer and times before Homer,¹⁴ in classical times, only one warrior is described using a word derivate from *lyssa*.¹⁵ Herodotus tells the story in the *Histories* 9. 71. The Spartan Aristodemus, who had returned from Thermopylai because of an eye infection, wanted to redeem his reputation. Therefore, he fought very courageously (Herodotus uses the very Homeric word *aristeuein*). Nevertheless, the Spartans did not consider that he deserved the

¹¹The assertion about Diomedes' madness is made by Lykaon's son, Pandaros. It is difficult to say whether this Pandaros is the same one of the *Odyssey*, whose daughters, according to the scholiast to *Od.* 20. 66 suffered from a disease called *kyon* – dog – or probably more lycanthropy.

¹²*Iliad* 6. 132 refers to Dionysos and *Iliad* 22. 460 to a maenad.

¹³Dumézil 1983:188-189 argues that *lyssa* precedes the doom of the warrior, because the warrior cannot control it. This brings him to commit an act of *hybris* for which he will be punished.

¹⁴Mycenaean warriors in a fresco in Pylos are depicted with tails, also in the Tiryns crater Athens National Museum 1511. In the mythical realm, Heracles, a pre-Iliadic hero, wears continuously a lion skin and he is often furious with *lyssa*.

¹⁵The word *lyssa* or its derivates does not appear in Plato, Thucydides, Xenophon or the orators. It appears once in Alcaeus, several times in Euripides', *Heracles* and *Orestes* and many more times in the *Anthologia Graeca*, but here referring exclusively to the madness of love.

recognition as best fighter because he had become mad (*lyssônta*) and abandoned the formation. This example illustrates that the Greeks in later times did not think that out-of-control warriors had a place in the phalanx formation. Individual, berserk-acting warriors do not make good soldiers and are, consequently, obscured in the written sources during classical times.

As with the word *lyssa*, the word *menos* is not a favorite of the classical writers. It appears in Herodotus 7. 220.4. Herodotus transcribes an oracle of Delphi for Leonidas (the Son of the Lion!) and the Spartans given before the battle of the Thermopylai. The oracle said that Leonidas would not be seized by the *menos* of bulls or lions but that he has the *menos* of Zeus. Whatever this means, it does not seem to refer to the irrational fury of animals, but to some royal or intellectual quality perhaps, which he already has. As in the previous example, the passage also stresses that shape shifters, as irrational fighters, are no longer well seen. The qualities of the shape shifters are not well-suited for later historical soldiers. After Homeric times, the practices of the frenzied warrior seem to have largely disappeared from the sources.

In order to find the groups of young shape shifters I will continue examining the evidence of the *Iliad* beyond the contributions of McCone. The evidence gravitates around two interconnected points, the terrifying aspect of the warriors and the use of animal skins. These points find parallels among other IE young warriors. For instance, the Germanic Harii, whose name is etymologically built on the IE root **koryos*, were, according to Kershaw, contingents of young men more than a separate tribe.¹⁶ Tacitus mentions in section 43 of the *Germania* that “their shields are black, their bodies dyed. They choose dark nights for battle and strike terror by the horror and gloomy appearance of their death-like army. No enemy can bear their strange and almost infernal aspect.”

As the appearance of the Harii is infernal in the dark night, all in black, so Hector is described in *Il.* 12. 463 as having a face similar to the night. The context here is the attack on the Greek ships. There is a fierce battle to climb and break the wall that the Greeks have built around the ships. Hector who seems unstoppable and whose eyes are burning with fire (*Il.* 12. 466), is described as

¹⁶Kershaw 2000: 42 does not think that the Harii are the same people as the Charini (Pliny *H.N.* 4.14. 98) although they refer to the same troops of young men.

“equivalent in his face to the fast night”.¹⁷ The comparison with the night implies death and fear, as seen at the beginning of the *Iliad* when Apollo sends the plague and he also is described as being similar to the night (*Il.* 1.47) (Moreux 1967: 237-272).

Besides the sinister appearance of Hector, the Lykians, who are the ones bearing the brunt of the attack, are described as using animal skins as shields – (*laisêia Il.* 12. 426). Furthermore, neither the Greeks nor the Lykians seem to be wearing corselets because the weapons can penetrate their skin when they turn their *naked* back (*Il.* 12. 428). Whereas the Greeks are fighting in a desperate manner, the Lykians seems to be some kind of shock troops, lightly armed so they can climb the wall.

One can safely say that there are in the *Iliad* reflections of the young IE contingents characterized by the wearing of skins, nakedness and a sinister appearance. On the other hand, even after the *Iliad*, it can be observed that the *laisêia* are not just another type of shield, but probably just a shaggy hide characteristic of certain troops. A fragment of the Cretan poet Hybrias exemplifies this:

There is for me much wealth, spear and sword and the beautiful hide (*laisêion*), cover of the body. With this one I carry plunder, with that one I reap, with that one I trample the sweet wine from the vine, with that one I am called lord of the serfs. Those who do not dare to have spear and sword and the beautiful hide (*laisêion*), cover of the body, all of them falling to their knees bow to me, calling me lord and great king.

The fragment shows that the *laisêion* can be used as shield and cover for the body. This perhaps implies that the narrator was naked. The hide is part of the attire of the warrior, together with spear and sword. It can be certainly used as shield, but it was probably just a hide of an animal. This can be seen in Herodotus 7. 91, when he writes that the Cilicians used *laisêia* instead of (*anti*) shields.¹⁸

¹⁷Egil's Saga 1 describes the bad temper that overcame Kveldulf (Evening Wolf) every night. Also in chapter 40, the powers of Skallagrim (Kveldulf's son and Egil's father) are normal during the day, but at night he is able to pick up a person and dash him so hard against the ground as to break his bones and kill him.

¹⁸The assumption is strengthened by images showing warriors or hunters using skins instead of shields. Cf. Museo Gregoriano Etrusco Vaticano AST 318 showing a hunter or warrior with goat skin and spear and Tarquinia, Museo Nazionale Tarquinies RC191 showing a soldier with skin and round shield on

Herodotus continues describing Xerxes' contingents present at the battle of Salamis. In 7.92 he describes the Lykians, according to Herodotus, historical descendants of those in the *Iliad*, and who were of Cretan origin and received their name from the Athenian Lykos, son of Pandion. This Lykos is a mythical character considered to be the founder not only of the Lykians of Asia Minor when he was exiled from Athens, but also the founder of the sanctuary of Apollo Lykeios – “Apollo, Lord of the Wolves” in Athens. These Lykians wore corselets, but on top of them they also wore around the shoulders the skin of a goat and the *pilos* hat that characterizes young warriors in Greek artistic depictions.

The connection with the Lykians and the goatskins opens yet another connection to another group of shape shifters. The use of the goatskin cannot only be justified through the fact that goatskins are easier to come across than wolves' or bears'. Buck warriors were known from the Germanic and Celtic world (Speidel 2004: 47-50).¹⁹ Wearing skins of goats was probably another means of inducing the shape shift in the warrior.²⁰ Also among the Greeks, Zeus and Athena wear the *aegis* (*Il.* 5. 733, 738 and 742), which even if described as a portent, is still a goatskin. The *aegis* itself has tassels or fringes, more likely to emphasize its wild, shaggy aspect.

Furthermore, the historical and mythical connections of the goatskin produce perhaps yet another piece of linguistic evidence towards the identification of contingents of young men in the *Iliad*. In book 8, Teucros shoots his arrows behind the protection of his half-brother Ajax. He manages to kill several Trojans, but cannot hit Hector, who is described as a furious dog (*kyna lyssêtêra Il.* 8. 299), obviously the epithet of a shape shifter. The rest of the Trojans are described with the word for young men *aizêôn* (*Il.* 8. 298), which we saw already applied to Diomedes and often describes the young men in a hunt of wild animals.²¹ Perhaps this description of the Trojans alludes to Hector's contingent being formed by a group of young men not only enraged like animals, but also dressed in animal hides, most likely goats.²² Certainly

the same hand and spear in the right.

¹⁹The British Gabratovices are literally the Goat People.

²⁰Herodotus 1. 173 explains how Lykos, founder of the Lykians, was driven out from Athens by his brother Aigeus, “the Goat.” The legend may reflect some prehistoric enmity between the “Wolf People” and the “Goat People”.

²¹*Il.* 3.26, 11.414, 18, 581, 17, 282.

²²cf. Tiryns frescoes depicting armed men fighting against men covered in skins.

they seem to have no armor. So Teucros can claim that his arrows pierce the skin of the young men (*Il.* 8. 298).

The word could be related to the word for goat. Mycenaean *aiza* (PY Un 1318) for goatskin (*aigia*) proves that there is an early affrication of the consonant. Although the etymology needs to be worked out in detail, maybe the *aizēoi* were originally men wearing a goatskin over the shoulders. They were hunters (and there are many depictions of hunters with this attire) and also light armed troops, whose name and function the *Iliad* vaguely remembers. As seen from Zeus and Athena, the goatskin could also have a terrifying aspect, which is also a common trait of youth troops. Furthermore, the terrifying aspect of the young men could also be emphasized by the dark color of their garments. In the *Iliad* 4. 280, the phalanxes of *aizēoi* are described as dark blue (*kuaneai*). This color is also used to describe Thetis' mourning veil in *Il.* 24. 93. Apparently, there was no other veil blacker.

In order to develop the discussion of the sinister aspect of the young troops, we have to refer to Angelo Brelich's contribution (Brelich 1961). He argued for the ritual initiatory character of many conflicts in archaic Greece. The soldiers in these conflicts would be largely young men, who used war as initiation into society. Conflicts, therefore, were fought by a reduced number of combatants and were recurring (Brelich 1961: 48-49). He explains the conflict of the Thessalians and Phocians (Herodotus 8. 27) as ritualistic fights. The seer²³ of the Phocians advises them to cover their bodies in chalk and attack the Thessalians in the dark night. The Thessalians fell into a state of panic thinking that they were some kind of apparition.²⁴ Herodotus mentions that the Phocians took not only the shields, which they customarily dedicated to the gods, but also the bodies. Herodotus, however, does not explain what the Phocians did with the bodies of the four thousand dead Thessalians.

The themes of fighting by night, using deceit and the fight being instigated by a seer are also present in other sources, although it is not clear whether the fighters belong or not to a youth contingent. In Thucydides 3.20.1-3.23.3 we read that the Plataeans escaped the besieging Peloponnesians by climbing

²³The word *mantis* is related to *mainomai* and *menos*.

²⁴Kershaw 2000: 63: The Germanic Harii were also conceived as an army of the dead. The fact that they considered themselves to be dead made them invulnerable, since they could no longer die.

through the wall in the middle of a dark, stormy night. Only about two hundred dared to break the siege, the majority considered it too dangerous. Through the description one can see that deceit was part of the plan from the beginning. In this regard, the Plataeans remaining in the city started an outcry at the wall far away from where the men were crossing and also had beacons ready to counteract the signals of the Peloponnesians. There does not seem to be much point to the whole expedition other than a proof of courage, since the Plataeans that escaped to Athens could not bring back the needed food supplies, which, according to Thucydides, had motivated the escape in the first place. It is interesting to note that the troops were lightly armed, as to be more mobile and make less noise, but also they wore only the left sandal. Thucydides mentions that this was with the intention of not slipping in the mud, but most probably this is as well some kind of reflection of an old contingent.

We can find parallels in Vergil, *Aeneid* 7. 682-691, when the troops of king Caeculus are described as lightly armed, wearing wolf-skins, and having only one sandal (Speidel 2004: 41).

King Caeculus is followed by a wild legion (*legio agrestis*) from afar. ... Not all have armor (*arma*), and the chariot and shield do not sound; but the majority hurls balls of blue lead, some others in one hand bear a brace of javelins; they have hoods of tawny wolf-skin as cover for their head; the left foot leaves a barefoot track behind, a rawhide sandal covers the other one.

Barefooted warriors appear in Trajan's column, as well, as part of the Germanic auxiliaries (Speidel 2004:60). Soldiers wearing only one sandal are an obvious variation of the theme. It does not seem that there is any specific importance attached to which foot is left bare, as long as one of them is, so as to prove the manliness and courage of the soldier. Although Thucydides does not mention that the Plataeans were ephebes, Hornblower (Hornblower 2004: 29) has noticed that the description echoes Pindar's 4th Pythian Ode (75-85), where Jason appears covered in a leopard skin, with long hair,²⁵ and also wearing only one sandal. According to Pindar, Jason is the one who is a stranger and a citizen at the same

²⁵Herodotus 1. 82 mentions how the Argives cut their hair short after losing Thyreae to the Spartans, while these let theirs grow from then on. This is, according to Brelich 1961, another ritualistic battle in which only 300 men in each group fight.

time, which has been explained as a symbol for the ephebe (Vidal Naquet 1986: 108). His description reflects the young marginal warrior, common from other IE peoples.

As the figure of Jason indicates, another reflection of IE contingents of youth can be found more generally in the groups of ephebes. Ephebes were not a regular part of the Greek army, only going into battle under special circumstances. They were also set apart in many other ways. Ephebes lived at the edge of society during the two years of their ephebeia. They were posted in the territorial limits of their cities and they were also marginal in social status, having the duty of guarding the territory, but not yet having full-citizen rights. They wore black tunics and were lightly armed, so they were distinct in their appearance. The fighting of the ephebe was not the hoplite phalanx, but more the skirmish at the border of the territories, similar to what we find in IE warfare, which has been characterized as series of raids.

Vidal-Naquet and Brelich studied the myths involving the ephebes that related them to the festival of the Apaturia – Deceit (?) – that celebrated the fight of Melanthos (the Black One) against Xanthos (the White One). Vidal-Naquet (1986:110-112) explains this as a rite of passage of the ephebes into the community. Besides being a characteristic way of fighting of the young warrior, deceit in this myth is explained in part as the presence of Dionysos Melanaigis or “Dionysos of the Black Goatskin”.²⁶ Dionysos, god of intoxication and ecstatic dances, covers his back with a goatskin, which we have seen as well as a characteristic of certain fighters, including Zeus.²⁷

Ephebes were by and large under the patronage of Apollo, who was an ephebe himself. Just as the IE contingents of young warriors are represented by the image of the wolf, Apollo is also associated in many myths with wolves as the epithet Lykeios expresses. Other wild beasts fall into the realm of his twin sister Artemis. As much as she is the mistress of the wild beasts in general, Apollo is the master of the wolves (Gersheson 1991: 7). The initiation of the ephebes also included rites to Artemis, as in Sparta to Artemis Orthia, who is, of course, patroness of hunting

²⁶There is another association with deceivers and goats. In the *Odyssey*, Dolios, Odysseus' faithful goatherd, is the father of Melanthios (the Black One), who fights on the side of the suitors. The fight against the suitors reenacts then the myth of the ephebes.

²⁷Dionysos Melanaigis is associated with the festivals of the tragedy. Winkler 1990: 35-36 argues that the tragedy is the “ephebes' song.”

as much as protector of animals.

Associated with the ephebes' warfare style and considered essential part of the ephebes' training is the hunt. In case of the Spartan youth, the hunt was even a human hunt, as seen in the famous *krypteia*. The hunt of the ephebe was conducted by night using snares and traps. Deceit, then, was characteristic of their hunt, but also of their way of fighting. There seems not to be a clear distinction between both. In the *Iliad*, we saw how Dolon - Deceiver - tried to steal the horses of Achilles by night.²⁸ He was covered by a skin of a wolf and his hunt turned into a manhunt. Pictorial representations of the scene make the double layer of meaning clear. In the vase in St Petersburg, State Hermitage Museum 653 Odysseus and Diomedes kill Dolon, who is covered with a wolf skin. Odysseus and Diomedes wear the *pilos* and *chlamys* typical of the ephebes, they are also very lightly armed, one with a spear and the other with a sword. Since these are also the garments of the hunters, the wolf hunt becomes a man hunt and vice versa.

Hunters and ephebes are sometimes mixed in the visual representations, especially when they are both wearing skins. Although written sources are silent about this, images consistently show a young warrior, dressed with skins, who is opposed or complementary to a hoplite. Several vases show young men in what has been described as hunter's and shepherd's outfits, but they are put in contrast to the hoplite. The Athenian red-figure crater in Rome Museo Nazionale Etrusco di Villa Giulia 50431 has three men in the front. The one on the right is a bearded man draped, holding a staff. The figure in the centre is a beardless youth, with helmet, spear, shield and a *chlamys*. He does not seem to be wearing a corselet, but looks more like an ephebe than perhaps a hoplite. On the left there is another young man clothed with what seems to be a leopard skin and hat. He has a spear over his shoulder. Although the outfit may correspond to the shepherd, nothing in the vase indicates his activity. His appearance looks foreign and perhaps represents a character in opposition to the hoplite.

Also the Athenian red-figure cup in Havana Museo Nacional de Bellas Artes 212 shows a young man arming. On the left there is a woman holding his spear, while the man closes his corselet.

²⁸Dolon in the *Iliad* was a wolf-warrior whose name means "deceit." This might have been a traditional name for youth warriors in the same manner as Irish and Germanic warriors use the word "thief" in their names.

On the right another of these “shepherd figures” is standing with shield and spear and dressed with skins. The fact that he is holding a shield makes him more a warrior than a hunter and certainly not a shepherd, although the foreign aspect remains. A better example comes perhaps from the red-figure lekythos in Athens National Museum CC1196. Here a young man with a sword and with a skin hat and skin cloak has fallen on the attack of a hoplite and desperately fights for his life. The opposition between the Greek hoplite and the foreign other is demonstrated very clearly.

Who are these young men in skins that the vases show? Are they ephebes or foreign troops? Are they part of a residual system of fighting not well incorporated into the phalanx? They seem to be unimportant enough to have vanished from most sources and only represented in a few vases.²⁹ Written sources are elusive, but perhaps we could find some allusions. Tyrtaios frg. 10, vv. 15-25 mentions the young warrior as opposed to the married soldier. This combination has always been understood as part of the phalanx, but this would imply perhaps a phalanx that was more varied and not as tight as during the Macedonian times (Krentz 2007: 72-73).

If the archaic and classical phalanx offer room for light armed troops to fight within their lines, perhaps these young men of the vases can be seen as contingents within the phalanx. In this case, it might also be significant that they retained the wearing of skins, not just as a way to get cheaper armor, but also as a way to differentiate themselves consciously from the hoplites through an association with animals. On the other hand, the written sources are largely silent about this point and Tyrtaios’ poetry might not be enough evidence.

Nevertheless, the youth in Sparta was associated with animals. The groups of the young men were called *agelai*— herds and were led by a *boagos* — leader of cattle (Chrimes 1999: 84). Twenty year old young men were called *eirenes*— rams. As Chrimes (1999: 119) states, the theory underlying the education of the boys in Sparta was that they were wild or half-wild animals and treated as such until tamed.

²⁹We cannot assume that every hoplite was armed in an equal manner, especially during the archaic period. Some would have worn bronze corselets, some others only leather or skin corselets. Krentz (2007: 80) assumes that during the archaic period light armed troops fought within the phalanx and only during the Persian Wars was there a separation of heavy and light armed troops into different contingents.

The *eirenes* were in charge of a fierce mock battle over the bridge at the Platanistes. As part of preparation for this battle, a puppy was sacrificed at night (Plutarch, Lycurgus 17). Sacrifices of dogs are common among certain IE youth. Dogs and wolves are connected with demons of the underworld, like Hecate, the Erinyes and the Keres (Kershaw 2000: 257, 262). Also, dogs and wolves are considered to have the same function among the warriors. For instance, while Germanic and Greek warriors built their names with the word for wolf, Irish warriors did it with the word for dog, like the famous Cú Chulainn. That the *eirenes* would sacrifice a puppy instead of a more common victim suggests IE warrior practices.

On the other hand, it seems that there was a division according to age in the usual Spartan formation. Age division in military formations occurs in other IE cultures, as already mentioned (McCone 1987:145). Apparently, the young *eirenes* were only exceptionally put in the military formation and they occupied themselves in other important duties, like overseeing the helots, riding the horses provided by richer citizens (Xenophon, *Hellenica* 6. 4.11) and, according to Chrimes, assisting in the *krypteia* (Chrimes 1999: 390), which she considers more than the ritual initiation of the young men and believes it had to do with teaching the young men how to perform scouting missions (Chrimes 1999:375).

Herodotus (9. 85) mentions that different contingents at the battle of Plataea were buried separately. One of the Lacedaemonian contingents was that of the *eirenes*, which implies that they fought separately from the hoplites. Another contingent that might have fought separately from the phalanx is that of the Skirites, which occupied the left wing of the army (Thuc. 5. 67.1), usually the place of most danger. It is still debated whether or not the Skirites were full citizens or *perioikoi* from Skiritis. Chrimes (1999: 378-379) proposes two etymologies for the name – Skirites can be related either to a hard covering as a coat of gypsum or an animal skin used as armor. Either one of these would go with the tradition of IE shape shifters. We saw how the army of the Phocians (Hdt. 8. 27) covered themselves in gypsum to frighten their enemies. Also, we have established that using animal skins as body armor in conjunction with fighting in the most dangerous position and apart from the citizen army were traits of special light-armed contingents. A Spartan *pithos* of the sixth century shows a warrior with helmet and sword and perhaps

a stone in his hand and covered with an animal skin fighting side by side the hoplites. Perhaps this is a representation of a Skirites,³⁰ which could find an echo in Tyrtaios 11, a poem in which he contrasts the heavy armed hoplite with shield and spear (vv. 21-34) with the light armed (*gymnêtes*– naked) throwers of stones and javelins under the protection of those in full armor (vv. 35-38).

To summarize, artistic evidence, legends and anecdotes in several cities of Greece seem to reflect very archaic ways of fighting, perhaps going all the way to a common IE past when youth contingents covered in skins and lightly armed were put in the most dangerous places of the battle, besides the better protected army of citizens. In the Greek world, these troops may have lost some of the element of frenzy that characterizes shape shifters in other cultures, but they still maintained the terror inspiring appearance and the tactics of shock troops. Although these troops had largely disappeared from the written sources in archaic and classical times, there are enough indications to suggest that they operated past Iliadic times, even if they were no longer well understood

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Indo-European “Smith” and his Divine Colleagues

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The purpose of the present study is to map the terms designating the craft of “smith” in Indo-European languages, analyzing their etymologies, classifying them according to semantic typology, and identifying “divine smiths.” The designations of “smith” in various non-Indo-European language families and isolated languages are also analyzed.

Indo-European Traditions: Indo-Aryan: 3 (§§ 1-9). Nuristani: 12 (§10). Iranian: 12 (§§ 11-18). Armenian: 22 (§ 19). Anatolian: 23 (§§ 20-23). Greek: 27 (§§ 24-26). Albanian: 36 (§ 27). Italic: 37 (§§ 28-29). Celtic: 45 (§§ 30-38). Germanic: 57 (§ 39-42). Baltic: 65 (§§ 43-46). Slavic: 73 (§§ 47-50). Non-Indo-European Traditions: Basque: 80 (§§ 51-53). Kartvelian: 81 (§§ 54-55). West Caucasian: 81 (§§ 56-58). East Caucasian: 82 (§§ 59-62). Hattic: 82 (§§ 63-64 (& 20, 22)). Hurrian: 82 (§ 65 (& 19.3)). Elamite: 82 (§ 66). Sumerian: 83 (§ 67 (& 21a-f)). Semitic: 83 (§§ 68-84). Egyptian: 89 (§§ 85-90). Cushitic: 90 (§§ 91-103). Berber: 92 (§§ 104-108). Fenno-Ugric: 93 (§ 109 (& 12)). Samoyedic: 94 (§§ 111-112). Turkic: 95 (§ 113). Mongolic: 95 (§ 114). Tungusic: 95 (§ 115). Korean: 95 (§ 116). Japanese: 96 (§ 117). Dravidian: 96 (§§ 118-119).

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On the Derivational History of Iranian **mairya* and **mariyaka*

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The present study aims at investigating the derivational history of old Iranian **mairya* and **mariyaka* in different Iranian languages through to Modern Persian as well as in Sanskrit. To fulfill the objectives, texts representative of the use of **mariyaka* from five Iranian languages have been extracted and analyzed.

1. Introduction and Background of the Study

The old Iranian words **mariyaka* have exercised over a century of study. The most relevant ones are summarized chronologically as follows:

- a. Bartholome (1904) explored and discussed the term **mariyaka* and its derivations.
- b. Herzfeld (1938) presented some points on the meaning of *marika* as it occurred in Old Persian.
- c. Wikander (1938) investigated the development and changes of meanings of the word **mariyaka* from Vedic to Avestan.
- d. Dumézil (1952) presented some theories on *márya* and its relation to the Indo-Iranian gods.
- e. Kent (1953) dealt with the meaning of the term *marika* in Old Persian.
- f. Mayrhofer (1963) analyzed the etymology of the term *ma'rya*- while Nyberg (1964) discussed the word in Middle Persian regarding its etymology and meaning.
- g. Widengren (1969) investigated the term *mīre/a*, and its relation with social institutions and organizations.
- h. Mackenzie (1971) investigated the term *mērag* in Middle Persian from a semantic point of view while Hinz (1972) discussed the term *marika*- in Old Persian.
- i. Mayrhofer (1986-2002) investigated the term *márya*- across the Indo-European languages.
- j. Schmitt (2000 & 2009) dealt with the term *marika*- in Old Persian.

1.1. The objectives of the Study

The present study aims at investigating the derivational history of Old Iranian **mairya* and **mariyaka* in different Iranian languages such as Avestan, Old, Middle and Early and New Modern Persian as well as in Vedic. **mariyaka* originally appeared in the form of *marian-ni*, an Indo-Aryan loanword describing warriors who served the king of Mitanni in Mesopotamia (Widengren 1969: 12). It also appeared in Akkadian as *maria-nnu* 'charioteer' and in Egyptian *mrin* 'a member of a group of young soldiers'.

For the present study, sample texts are presented from the five Indo-Iranian languages in the belief that they are representative of the use of the word **mariyaka* over its entire derivational history. In the same direction and for the ease of understanding on the part of the readers, their translations have been provided from traditional authoritative sources as indicated after each text.

2. Corpus: The uses of **mairya* and **mariyaka* in the Indo-Iranian languages

In the following sections, we present texts in which examples of the word **mariyaka* is presented.

2.1. Vedic

According to Mayrhofer (1963), the Vedic word *márya* is masculine and means 'young man, lover and free young man'. The Vedic *márya-* derives from Indo-Iranian **meryo-*. Examples of **márya-* in the meaning 'young man' in Vedic are shown in the following texts. .

Text 1: RV 1, 115.2 (Griffith 1896)

*sūryo devīmuṣasaṃ rocamānāṃ mario na yoṣāmahyeti
paścāt |
yatrā naro devayanto yughāni vitanvate prati bhadrāya
bhadram ||*

Like as a **young man** followeth a maiden, so doth the Sun
the Dawn, refulgent Goddess:
Where pious men extend their generations, before the
Auspicious One for happy fortune.

Text 2: RV 3.33.10 (Griffith 1896)

*ā te kāro śṛṇavāmā vacāṃsi yayātha dūrādanasā rathena |
ni te naṃsai pīpyāneva yoṣā **maryāyeva** kanyā śāsvacai te ||*

Yea, we will listen to thy words, O singer. With wain and
car from far away thou comest.

Low, like a nursing mother, will I bend me, and yield me
as a maiden to her **lover**.

Text 3: RV 4.20.5 (Griffith 1896)

*vi yo rarapśa ṛṣibhir navebhir vṛkṣo na pakvaḥ sṛṇyo na jetā |
maryo na yoṣām abhi manyamāno 'chā vivakmi puruhūtam
indram ||*

Him who is sung aloud by recent sages, like a ripe-fruited
tree, a scythe-armed victor;

I, like a **bridegroom** thinking of his consort, call hither
Indra, him invoked of many

2.2. Avestan

Wikander (1938) revealed the trend of semantic changes of this word between Vedic and Avestan. As many Iranian and Indic shared words, it developed a negative connotation due to Zoroastrian changes. In Avestan, *mairya* is among the sinners comparable to Ahrīman, karapans, wolves and magicians (Wikander 1938: 35). Wikander showed that this word originally referred to 'a group of young warriors' who joined war enthusiastically, even taking on the appearance of wolves (*vəhrka* in Avestan) because of their enthusiasm. He is even of the view that the above group of brotherhood society and its earthly counter parts worshipped Freidun (*θraētaona* in Avestan) and Garshaseb (*Kərəsāspa* in Avestan) as their guardians. However, in the society they lived a rude and reckless life which accounts for the semantic shift from 'warriors' to 'villains' in the Zoroastrian religion.

There are three major sections of the surviving Avesta, namely, a. the Yasnas (Ys.); b. the Yashts (Yt) and c. the Vidēvdāt (Woodard 2008: 101-102). The following texts illustrate the use of Av *mairyo* 'villain' as it appears in the Ys. and Yt.

Text 1: Ys. 9.11

yō jānaṭ ažīm sruarəm yim aspo.garəm nərə.garəm yim višavantəm zairitəm, yim upairi viš raodaṭ ārsṭyō barəza zairitəm, yim upairi kərəsāspō ayan ha pitūm pačata ā rapitwinəm zrvānəm. tafsaṭča hō mairyo x^visaṭča; fraš ayan hō frasparaṭ yaēšyantīm āpəm parāṅhat. paraš tarštō apatačāt naire. manā kərəsāspō (Reichelt 1911, p.2).

He smashed the horned dragon, the horse-devouring, man-devouring one, the venomous, yellow one, over whom the yellow venom rose up to the height of a spear; on whom keresaspa cooked his meal in an iron pot at noontime. The **villain** started to get hot and to sweat. He shot out from under the pot and scattered the boiling water. frightened he ran off and a way, Keresaspa, whose thoughts were those of heroes (Skjærvø 2006).

Text 2: Ys. 9.18

nī taṭ yaθa taurvayeni višpanəm tbišvatəm tbaēš daēvanəm mašyānamčā yāθwəm Pairikanəmčā sāθrəm kaoyəm karafnəmčā mairyanəmčā bizanranəm ašəmao anəmčā bizanranəm vəhrkanəmčā čaθwarə. zanranəm haēnyāšča pəθu. ainikayā pataiθyā (Reichelt 1911: 2).

(I call) down (all) that so that I may overcome the hostilities of all hostile ones, old gods and men, sorcerers and witches, false teachers, poetasters, and mumblers, **villains** on two feet, obscurantist's on two feet, wolves on four feet, and (their) army with wide front, deceiving and falling (all over) (Skjærvø 2006)

Text 3: Yt. 8.59 (To Tishtirya)

mā hē mairyo gəurvayōit, mā jāhika, mā ašāvō asravayaṭ. Gāθō ahumərəxš paityārənō iməm dāēnəm yəm āhūirīm zaraθuštrīm (Reichelt 1911: 28).

May not a **villain** seize it nor a witch or . . . (?) who has not performed the Gāthās, who destroys (this) existence, who opposes this daēnā, that of Ahura Mazda, that of Zarathuštra (Skjærvø 2006: 91).

Text 4: Yt. 10.2 (To Mithra)

*mərənčaitē vīspam daiŋ'haom mairyō miθrō.druxš spitama;
yaθa satam kayaḍanəm avavat ašava. ǰaçit miθrəm mā ǰanyā,
spitama mā yim drvatāt pərəsāŋ he mā yim x'ādaēnāt ašaonāt;
wvayā zī asti miθrō drvataēča ašaonāēča (Reichelt 1911: 15).*

The **knave** who is false to the treaty, o spitamid, wrecks the whole country, hitting as he does the truth owners as hard as would a hundred obscurantist's. Never break a contract, o spitamid, whether you conclude it with an owner of falsehood, or a truth owning follower of the good Religion; for the contract applies to both, the owner of falsehood and him who owns Truth (Gershevitch 1967: 74-75).

Text 5: Yt. 19. 56 (To the Earth and Divine Fortune)

*yaṭ isaṭ mairyō fraŋrase zrəyāŋhō vouru. Kašāhe maǰnō
apa.spayat vastrā, taṭ x'arənō isō, yaṭ asti airyanəm dahyunəm
zātanəm azātanəmča yaṭča ašaonō zaraθuštrahe ā tat x'arənō
apa.hiḍat, aḍa hāu apaǰžārō bvaṭ zrəyāŋhō vouru. kašāhe
vairiš, yō haosravā nāma (Reichelt 1911: 17).*

The Turian **villain** Frangrasyān sought it from the vourukasha sea. He threw off (his) clothes and naked sought that fortune which belongs to the Aryan lands, to the born and unborn, and to orderly Zarathushtra. Then that fortune rushed forth, that fortune ran away, that fortune evaded (him). Then that stream came into being, flowing out of the Vourukasha sea, the lake called Haosravah (fameds) (Skjærvø 2006: 116).

Text 6: Yt. 19.77 (To the Earth and Divine Fortune)

*yaṭ paiti kava haosrava ṭam kərəsəm upatəm čarətəm ǰəm
darəǰəm nava. frāθwərəsəm razurəm yaṭ dim mairyō. nurəm
aspaēšu paiti farətata: vīspe bvaṭ aiwi.vanyā ahurō kava
haosrava mairīm tūirīm fraŋrasyānəm bandayaṭ kərəsavazdəm
puθrō kaēna syāvəršānāi zūrō.ǰatahe narahe.aǰraēraθaheča
naravahe . . . ? (Reichelt 1911: 17)*

so that {...} Kavi Haosravah [outmaneuvered] Keresavazdeh / on that (famous) long race course of nine intersections / [around the All-Aryan] Forest / when the **scoundrel** of crooked [mind] / contended with him at horse racing. / Overcame all (of his enemies) / the lord Kavi Haosravah. / [He slew] the Turian **scoundrel** Frangrasyan, / he bound Keresavazdeh, / (doing that) in revenge for Syāuuaršan, being the son of (this) treacherously slain hero, and (in revenge) for heroic Agraēratha (Humbach and Ichaporis 1998: 53)

Text 7: Yt. 19.82 (To the Earth and Divine Fortune)

yey'he taṭ x'arənō isaṭ mairyō tūiryō fraṅrasa višpāiš avi karšvaṅ yāiš hapta. Pairi yāiš hapta karšvaṅ mairyō apatat fraṅrase isō x'arənō zaraṭuštrāi. ā. taṭ x'arənō frazgaḍata avi vayam vitāpəm. inja mā urvisyatəm, aēzo ḡasatəm aēzahe, yaḡa kaḡača tē ās zaošō mana yaṭ ahurahe mazdā daēnayāsca māzdayasnāiš (Reichert 1911: 17).

His was the fortune that the Turian **villain** Frangrasyān sought in all the seven continents in the seven continents in which the **villain** Frangrasyān ran about seeking the fortune of Zarathushtra. Then he rushed upon that fortune pursuing it to the wide water, (calling). Ho, go back here! . . . (Skjærvø 2006: 118).

2.3. Old Persian

The following texts presents the use of Old Persian *marikā* 'young man' < **mariyaka* (Kent 1953: 202) from DNB, Darius's inscription in Naqsh-e Rostam (Kent 1953: 137).

Text 1: DNB. 50-55

marikā, daršam azdā kušuvā, čiyākaram ahi, čiyākaramtai ūnarā, čiyākaramtai pariyanam: mātai ava vahištam ḡadaya, tayatai gaušyā ḡanhyāti; avasči āxšnudi, taya paratar ḡanhyāti (Kent 1953: 138-139)

O **young man**, very much make known of what kind you are, of what kind (are) your skills, of what kind (is) your conduct! Let not that seem the best to you which is

spoken in your ears; listen also to that which is said besides (Schmitt 2000: 39-41).

Text 2: DNb. 55-60

marīkā, mātayāt awa nabam θadaya, taya ... kunavāti: taya skauθiš kunavāti, awašči dīdi; marīkā, ... mā patiyātaya ... māpati šiyātiyā ayāumainiš bavāhi ... mā raxθa(n) tu ... (Kent 1953: 138-139)

O **young man**, let not that seem good to you, which the ... does, what the weak one does-observe that too! O young man, do not set yourself against the ..., moreover do not become (a man) without fervour in counter-attack owing to your blissful happiness! Let not ...! (Schmitt 2000: 39-41)

2.4. Middle Persian

Middle Persian *mērag* derives from **mariyaka* according to the regular change in the diphthong /ai/ to /e/ in Middle Persian and the nominal suffix /-ka/ or /-aka/ which changes to /g/. Examples from Middle Persian are drawn from two sources. The first is the Bundahišn which was published in the late Sassanid period, and is a collection of various materials taken from a variety of sources and consists of 36 chapters (Tafazolli 1998: 141-145). The second is the Kārnāmag-ī ardaxšīr -ī pābagān, the only historical work in the Pahlavi language. It deals with historical issues and is full of fables and myths about Ardashir, the founder of Sassanid kingdom (Tafazolli 1998: 263-265).

The following texts exhibit the use of Middle Persian *mērag* which indicates 'husband' in the Bundahišn. This word is frequently used with *ziyānag* 'wife' (Wikander 1938: 22-41) and 'a couple' (*ziyānag, mērag*) as proper names *asfyān* (*mērag*=man) and *zarišom* (*ziyānag*=woman) who are the founders of the dynasties of the kings. In the Kārnāmag-ī ardaxšīr -ī pābagān, however, this word denotes a 'messenger' though the dictionary meaning is 'a young man' and 'husband'.

Text 1: Bundahišn

az jam ud jamak ī-š x^wah/ būd/ zād juxt-ē(w) mard ud zan āgenēn zan ud šōy būd hēnd. mērag Asfyān ud ziyānag zarišom nām būd hēnd. (Pakzad 2005: 390)

From Jam and Jamak who were brother and sister, a pair of male and female was born. These two persons married to each other. The **husband** was *Asfīyān*, and the wife was Zarishom (Bahar 1990: 149).

Text 2: Kārnāmag-i ardaxšīr -ī pābagān

mard ī ardaxšīr, ka ō pēš ī kēd ī hindugān rasīd, kēd ham – čiyōn/čēon mērag dād, pēš ku mērag saxwan guft, u-š ō mērag guft ku tu xwadāyī pārsīgān pad ēn kār frēstīd ku x^wadāyīh ī ērānšāhr pad ēw-x^wadāyī ō man rasēd? (Faravashi 2003: 137).

When Ardashir's man reached the presence of the Ked of India, the latter observing the **messenger**, spoke to him, before he could express himself, (the purport of his message) to the following effect "Are you sent by the king of the Parsis to put me the question:" will the sovereignty of the kingdom of Iran reach unto me as its emperor? (Faravashi 2003: 149).

2.5. Early Modern Persian

The Early Modern Persian examples below are drawn from two early sources. The Panchatantra is a collection of originally <http://en.wikipedia.org/wiki/India> http://en.wikipedia.org/wiki/Animal_fable animal fables in verse and prose and it is "certainly the most frequently translated literary product of India". A Persian version from the 12th century became known as *Kalila and Dimna* (Monshi 1992: 15-17). The second source, *Abu Moslem Nameh*, is an epic taken from the life and heroic exploits of Abu Moslem Khorasani, the Abbasid leader and his followers (Tarsusi 1989: 5). The Early Modern Persian employs both *mīre* and *mīrak*, both 'young man, husband'.

Text 1: Kalila and Dimna

dorudgarī rā dar sarandīb zanī besyār zibā bud. ān zan va hamsāyeye vey rā olfati padīd āmad va xišāne dorudgar vey rā az mājarā bāxabar sāxtand. dorudgar āzmūn e gofteye ānān rā tadbīri andīšīd va vānamud kard ke azm e safar dārad va zan rā vedā? kard va čun u [šowhar] beraft, zan, mīre rā beyāgāhānīd va mi²ād e āmadan qarār dād va dorudgar bigāhī az rāh-e nabahreḥ tā xāne raft. mīre qowm rā ānjā dīd, sā²ati tavaqqof kard, čandān ke be xābgāh raftand ..." (Monshi 1992: 218)

A carpenter lived in Sarandib with a beautiful wife. Unfortunately the wife started a friendship with a neighbour, and the carpenter's relatives made him aware of this relationship. He tried to discover if the news was correct. He pretended that he was going to go on a trip and decided to say goodbye to his wife. As soon as the husband set out for his trip, the wife informed the **young man** and arranged a meeting with him. But, the husband suddenly returned home from Nabahre. The husband saw the two persons (the young man and his own wife) and waited till they went to the bed-room. . . . (Monshi 1992: 218)

Text 2: Abu Moslem Nameh

az qazā, be dar-e dokkāni gozašt ke ā?inegar būd va yāxči nahādeh bud, dar robud va guft : hey , mīrakam, be gašti miravad (Tarsusi 1989, p.5).

She passed a mirror shop and seized a piece of cloth from that shop and said that her **husband** was going to go on a trip (Tarsusi 1989, vol. 2, 119).

2.6 Modern Iranian usage

The term *mire* in Post-Islamic texts used to refer to a member of an organized group. It appeared in the form of 'mire-ye Neishabouri' which is the name of a member of Malamatiyeh sect, a group of mystics who lived in the 11th to 13th centuries CE who concealed their virtue and good behaviors though they publicized their wrongs (Dekhoda 2007: 2803). In the above use, *mire* was employed as a title rather than a proper name (Ansari-heravi 1983: 540). It is interesting to add that despite the fact that the word *mire* does not now exist in formal standard Persian, it is used in a number of informal spoken dialects. We find, for example, *mīrak* in the Bushehri, Bardestani and Dashti dialects in the south of Iran. It is used as a family name and carries a negative connotation as it is employed to denote occupations such as 'barber' which had been regarded among the low class occupations. This term also used to be employed in front of the name of barbers, e.g., Mir-Mohammad, Mir-Ahmad.

The form *mire* is employed in the Bardestani and Dashti, Anaraki, Behdinan, Shushtari and Lori dialects with meaning

of 'husband'. Afghani *maryai* means 'slave' (Mayrhofer, 1963: 5-6)

The Modern Persian equivalent of *mire* is *mol/mol* which means 'bastard'. Linguistically, the shift of /r/ to /l/ is very common in New Persian. The semantic shift may be explained because Persian words originated from two sources, namely, Old Persian and Avestan. As the words in Avestan have negative meanings, it is possible that the negative connotation ('bastard') goes back to its origin in Avestan ('villain').

3. Conclusions

Regarding the corpus and discussion presented above, the concluding remarks can be classified into phonological and semantic and changes as follows:

3.1 Phonetic change

One of the sub-groups of diphthongs in ancient Iranian was /ai/ which appeared as /ae/ and /ai/ in Avestan and Old Persian. The historical origin of /ai/ as a diphthong in ancient Iranian goes back to the Indo-European diphthongs such as /ai/, /ei/, /oi/ and /ǝi/. The most important change with regard to the diphthong (/ai/) in Middle Persian is that it appeared as /ē/ while in Modern Persian it changes into /ī/. Regarding the consonantal development, the phoneme /k/ in Avestan and Old Persian derives from PIE **k^w*. When this phoneme (/k/) appears in middle and final positions of a word along with vowels and consonants such as /r/ and /n/, it changes to /g/ in Middle Persian and in Modern Persian and modern Iranian dialects it changes into /e/ or /a/. The other word found in Iranian dictionaries is /mul/ meaning 'libertine' whose phonological changes involve the shift from /r/ to /l/, /a/ to /u/ and /i/ to /u/, all of which are very common. Table 1 depicts the phonological changes of **márya-* and **mariyaka*.

Table 1. The Phonological Changes of **márya-* and **mariyaka*

Old Indo-Iranian	Old-Iranian	Vedic	Avestan	Old Persian	Middle Persian	Early Modern Persian	Modern Persian
<i>*meryo-</i>	... <i>*mariyaka</i>	<i>márya-</i> ...	<i>máiryā</i> <i>marika</i>	... <i>mērag</i>	<i>mire</i> <i>mīrak</i>	<i>mire/mol</i> <i>mīrak</i>

3.2 Semantic shift

Regarding the evidence and theories concerning the

semantic shifts, the reader can understand a variety of meanings from the forms *máryaka* > *mairya* > *mērag* > *mīra/e* such as 'young man, youth, young fighter, lover, wanderer, wicked, thief, husband, client, servant, and devotee' (Table 2).

Table 2. The Semantic Changes of **mairya*- and **mariyaka*

Old Indo-Iranian	Old-Iranian	Vedic	Avestan	Old Persian	Middle Persian	Early Modern Persian	Modern Persian
young man	young man/ husband	young man, lover and free young man Mitanni 'member of war band'	one who revenges, sordid, liar	young man	husband / messenger	husband / young man / fornicator / adulterer	husband / low class occupation

The justification of opposing and contradictory meanings which are indicated in the cited texts varies in difficulty. Some of the semantic shifts or, better, extensions, are reasonably transparent. Obviously, the association of 'youth, young man' with a 'warrior' is easily motivated by the martial occupations of young men across the world. For example, Old Irish *óac* (< PIE **h₂yuh_x-ŋ-kós* < **h₂óyus* 'strength', Lat *iuvencus* 'young (cow)', Skt *yuvaśá-* 'young') indicates both 'young man' and 'warrior' as also does the related *oclach* 'young man, young warrior' (DIL-NOP 86, 93). The cognate set under discussion derives from PIE **méryos* 'young man' where we can certainly find semantic extensions into the area of conjugal relations, e.g., Lat *maritus* 'husband, lover' that mirrors some of the changes found in Iranian but the other cognates do not appear to include the martial element. It may, however, be supposed that this semantic shift, matching the development seen in the Old Irish example, had already transpired by the time of Indo-Aryan incursions into the Near East as we can see in its application to warrior bands among the Mitanni. This martial aspect should have functioned beside the meaning 'young man' in early Iranian as this provides the basis for explaining the accumulation of negative meanings found in Avestan ('member of warband' > 'liar, thief, villain').

The series of meanings suggesting lower occupational status seen in Middle Persian ('messenger') and Modern Persian ('servant, barber, etc') may be explained in one of two ways. The first presupposes that the semantic shift associated with Avestan ('villain') was sustained in later Iranian in Post-Islamic times. For example, Av *pairikā* 'demonic courtesan'

continued in Post-Islamic Iranian (*pari*) with the same negative connotations but also emerged in some Iranian dialects and standard Persian with what was probably the earlier (pre-Zarathrustra) positive meaning. This is why many Iranians today still name their daughters *Pari* ‘± angel’.

Alternatively, the semantic basis of both ‘youth’ and ‘young warrior’ may be sufficient to explain the shift to lower status occupations. Returning to the early Irish example above, *oclach* not only meant ‘young man’ and ‘warrior’ but also came to mean ‘an attendant, servant, vassal’, i.e., someone occupying a lower status. This extension of the semantic field might also help to explain the Modern Persian application of *miri* to the followers of the Malamatiyeh sect. Irish *oclach* similarly came to be employed to describe the followers or disciples of saints.

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Iberian *ordumeles*, Paleo-Sardinian *Ortumele*, *Ortarani* and *Araunele*. Cognitive Semantics and Substrata Research

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Research on the earliest substratum of Sardinia, Paleo-Sardinian, had long come to an impasse before structural and typological models replaced the older historical-comparative method. The typological approach has shed light on the agglutinative organization of toponymic compounds, and at the same time highlighted striking resemblances with Paleo-Basque morphological and lexical material and with Iberian records. In this paper we argue that a careful examination of the structural and semantic properties of some Paleo-Sardinian microtoponyms related to the color of mountain streams and terrains enables us to decipher one Iberian compound, and opens up a new fruitful area of cross-linguistic research between Old Iberia and Neolithic Sardinia.

1. Iberian

Remarkable progress has been made in the structural analysis of Iberian since Gómez-Moreno deciphered the semisyllabic writing system,¹ and Jürgen Untermann, Javier de Hoz and other scholars began applying distributional and typological models to the *corpus* of inscriptions, thus managing to infer linguistic typology,² morphosyntactic patterns and almost a hundred lexical roots.² Still, further advances in semantic interpretation remained blocked by the lack of cross-linguistic correlation with other Mediterranean Indoeuropean languages. Particularly distressing, moreover, was the view that Basque was of no help in deciphering the almost 170 Iberian inscriptions, and that the scanty cognates displayed by the two ancient languages of the Iberian Peninsula were due to centuries of enduring contact (*Sprachbund*).³ One of the most persuasive correlations between Iberian and Paleo-

¹Cf. Gómez Moreno (1949).

²Cf. Siles (1985), Untermann (1990, 2004), Velaza Frías (1991, 1996), Silgo Gauche (1994), de Hoz (2001, 2008).

³Cf. Tovar (1997:47-145), Gorrochategui (2002).

Basque is the adjectival morpheme for the color ‘black’, attested as *beleß* or *beles*,⁴ and also in Latin transcriptions as *meles*,⁵ as *bele(x)* in old Aquitanian anthroponyms,⁶ and as *bel(e)* and *beltz* in Basque lexical items and toponymic designations.⁷ In the bronze inscription of Ascoli (Italy), dated 89 BC, this morpheme occurs several times within the first names, typically epithets, used for Iberian soldiers in the Roman army. As far as this inscription is concerned, we will focus our attention on the sequence *ordumeles*, which neatly reflects the compound *ortu(n)* ‘?’ + *beleß* ‘dark’, and will attempt an acceptable interpretation by means of external comparison with some Paleo-Sardinian toponyms, which we presume to be formally and semantically tightly bound.

2. Brief typology of Paleosardinian

Since the first descriptions of the wild inhabitants (*Ilienses*, *Bàlari*, *Sardi Pelliti*, *Mastrucati Latrunculi*) of Sardinia by Greek and Roman geographers, the island has kept the secret of its primitive Indoeuropean language hidden. Italian scholars of the first half of the last century⁸ launched a thorough exploration of possible analogies within the whole Mediterranean area, from Iberia to Palestine, and with their historical-comparative method managed to isolate a reduced set of lexical roots, still assumed to have been part of a primaeval Mediterranean substratum.⁹ The Swiss scholar Johannes Hubschmid and the Bavarian linguist Max Leopold Wagner, the “Meister” of Sardinian linguistics,¹⁰ also adhered to the principles of a method which gave total priority to homonymic forms within the examined languages, but failed to unearth the very functioning of their structures.¹¹ The situation has rapidly changed since two of the most archaic areas of the island were investigated exhaustively, particularly in their

⁴Cf. Untermann (1990:216-217; 1998:76).

⁵Cf. Velaza Frías (1996:34).

⁶Cf. Cf. Gorrochategui (1984:158-159).

⁷Cf. Michelena (1985:128,222).

⁸From Francesco Ribezzo, Alfredo Trombetti, Vittorio Bertoldi, Giovanni Alessio, Giandomenico Serra, Benvenuto Terracini to Carlo Battisti and Giacomo Devoto. For all of these see the critical assessment by Silvestri (1979-82) and Blasco Ferrer (2010: chapter II).

⁹Pellegrini (1994) has listed these (*CARRA ‘stone’, *MARA ‘mare’, *SALA ‘running water’, *TALA ‘mountain stream’, among others).

¹⁰Cf. Hubschmid (1953, 1963), Wagner (1951, 1960-64).

¹¹A substantial evaluation may be read in Craddock (1969).

toponymic configuration.¹² In 1993 we showed that a modern structural analysis of microtoponyms (collected from personal investigation *in loco* or deduced from the place-name inventories of Paulis and Wolf), with a thorough distributional and frequential examination of their segmental morphs, offered a syntactic account of the underlying organization of the Preindoeuropean language of central and eastern Sardinia. Thus, by observing the archaic lexeme *orgosa* and the toponym *Orgosa* (and *Orgòsolo*), ‘well, river’s mouth’, we easily recognize two compounding roots, each generating further derivative forms: *orga* and *orge* [g] ‘well, wet ground’, *Orga*, *Orge*, *Org-ai*, *Org-ei*, *Org-oi*, *Org-osa*, *Org-ose*, *Org-ol-ai*, *Orgose-koro*, *Orgos-ol-ai*, and *Osa* (and *Bosa*, with prosthetic consonant), *Osu*, *Os-oe*, *Flumen-d-osa*, *Os-alla*, *Ós-ana*, *Os-ol-ai*, *Ós-olo*, all terms for ‘wells, rivers, river mouths, wet terrains’. Now, it may easily be assumed that *preie*. *orga* has been “glossed” by the second root, **osa*, this connected with **aus-a*, *ósa*, a productive Preindoeuropean (Giacomo Devoto) or – less persuasively in Sardinia – Paleoeuropean (Hans Krahe) root.¹³ Our tautologic toponym shares its complex structure with a handful of similar compounds, scattered all over Europe: *Vall d’Aran*, *Chateau-dun*, *Lingua-glossa*, *Mon-gibello*. Basing our research on further structural and typological examination of the microtoponyms of the central and eastern counties of Sardinia we have been able to gather in recent years a vast amount of evidence about the real organization and the origin of the Paleo-Sardinian language. For its “agglutinative” typology we may adduce the following selected examples, all microtoponyms:

- (1) *ard-ai*, *ili-ai*, *iri-ai*, *istil-ai*, *gurri-ai*, *nur-ai*, *ol-ai*, *on-ai*, *org-ai*, *ort-ai*, *sun-ai*, *tal-ai*, *turr-ai*, *ur-ai*; *aran-ake*, *ili-ake*, *iri-ake*, *nur-ake*, *tal-ake*, *ur-ake*, *berr-age*; *ili-ana*, *tal-ana*, *ós-ana*, *óv-ana*, *ártz-ana*; *árd-ara*; *ol-eri*, *tal-eri*; *sun-ele*, *turr-ele*.
- (2) *os-ol-ai*, *ort-ol-ai*, *ov-ol-ai*, *artz-ol-ai*; *bid-on-i*, *mand-on-i*, *ol-on-é*, *ós-on-o*; *tale-turre*, *tala-suni-ai*, *berri-tal-ai*, *berru-nur-ai*; *ort-aran-i*; *orgo-r-isti*, *bid-istil-i*.
- (3) *arau-nele* (< *aran*), *bidu-nele* (< *bide*), *istiu-nele* (< *istil*), *turru-nele* (< *turri*); *org-ose-koro* (< *orga*), *turri-kore*, *talae-kore*; *tal-erthe* (< *tala*).

¹²Cf. Blasco Ferrer (1988), Wolf (1998).

¹³For Devoto’s periindoeuropean/mediterranean **AUS-A* and Krahe’s Paleoindeuropean **AU-SA* see the balanced summary in Silvestri (1985-86).

Subset (1) shows a recurrent use of suffixes (here *-ai*, *-ake/age*, *-ana*, *-ele*, *-eri*, but all stressed/unstressed vowels and almost all kinds of diphthongs are documented) bound to a restricted set of roots: *ard-*, *artz-*, *berri*, *bid-*, *ili*, *iri*, *isti(l)*, *gurr-*, *mand-*, *nur*, *ol-*, *on*, *org-*, *ort-*, *os-*, *ov-*, *sun-*. *tal-*, *turr-*, *ur*.

Subset (2) shows the pertinent capacity of an agglutinative language to make compounds by simply adding roots and suffixes (with derivative and flexional marks): *os-* + *ol-*, *ort-* + *ol-*, *ov-* + *ol-*, *artz-* + *ol-*; *bid-* + *on*, *ol-* + *on*, *os-* + *on*, *mand-* + *on*; *berr-* + *tal-*, *berr-* + *nur*; *tal-* + *turr-*, *tal-* + *sun-*.

Subset (3) is the most relevant for us, because it illustrates the ability to derive a suffix from a root, an outstanding feature of agglutinative languages: *-gor/kor* (+ paragogic vowel) is an offshoot of Basque *gorri* 'red', a formal development well attested in Iberia and Aquitaine,¹⁴ *-erthe* reflects neatly the bsq. root *ertze* 'wedge, edge', which also matches the denotatum it specifies (*tala* + *erthe* = 'mountain river bank'), and finally *nele*, which is the adjectival unit for 'dark', on which we shall focus our attention next.

The typological organization of Paleo-Sardinian completely coincides with that of Basque. The majority of the extracted lexical roots have an exact correspondence in present-day Basque, and in some cases in reconstructed Basque and Iberian: *ardi* 'ox' (and cf. *ardiule*, *artule* 'wool' and sd. *Ardule*, *Ardauli* in Sardinia), *hartze* 'rock, stone' (and cf. *Artzu*, cognomen, and *Ártzana*, a mountain town on a rocky field), *aran* 'valley', *berri* 'new', Iberian *ili* and Basque *iri* 'settlement', *istil* 'marsh', *gorr(i)* 'red' (and *gurri*: *Gurriaran*), *mando* 'equus asinus', *ola* 'primitive hut', *obi* (> sd. *ov-*) 'cave', (*i*)*turri* 'spring, well', *ur* 'water'. Some other roots are still opaque, and we cannot dwell on their structures and possible correlations in this paper, but only stress the fact that they cannot be dismissed out of hand as potential Basque or Iberian units (so **tala* 'mountain river', **nur*, well represented in *nurake* 'megalithic monument', for us a plural form of 'stone').

From a morphophonemic point of view it is important to point out that in Paleo-Sardinian compounds the final vowel of the first unit is subject to many qualitative variations (raising of mid-vowels, dissimilation), as in *berri* + *nur-* > *berru-*. In derivatives (1) dropping of the final vowel is the general rule (*tala*

¹⁴Cf. Coromines (1981, I:111): "Que *-corr* soit le Basque *gorri* 'rouge', cela va sans difficulté"; Orpustan (1991:128,160).

+ *-eri* > *taleri*), but the ending *-i* turns out to be more resistant (*ili* + *-ai* > *iliai* compared to *tala* + *-ai* > *tal-ai*).

Before going on to interpret the Iberian compound **ortubeleß* with the support of Paleosd. roots, let us briefly recall that recent archaeological, historical and even genetic contributions¹⁵ all point unequivocally to one or more migrations from neolithic Iberia to the island of Sardinia. We have argued in our recent *History of the Sardinian language*¹⁶ that the earliest inhabitants of neolithic Sardinia, the *Ilienses* and the *Bàlari* (with *bal-* linking Sardinia closely to the Balears), are the result of Iberian and Paleo-Basque colonization in search of obsidian, a 'dark volcanic mineral' which is only to be found along the eastern coast of the south of France and Catalonia.

3 Semantics and reconstruction of **ortubeleß*

Along with structural and typological approaches to isolated languages of the Mediterranean (Etruscan, Iberian, Minoan),¹⁷ cognitive semantics has played an important role in the recent debate about diachronic reconstruction.¹⁸ A vast amount of toponymic material widespread over Europe and Asia has clearly revealed that 'black' and 'red' were (and are!) the usual adjectival qualities used to encode a rich variation of color tones concerning 'earth' and 'water', and that color names commonly drew on metaphoric and metonymic transfers ('blood', 'fire' > 'red color').¹⁹ We will now undertake a thorough discussion of the semantic values of **ORTU* based on this assumption. The following list records all the place-names in central and eastern Sardinia which are allegedly derived or compounded with the root **ORTU*: *ort-ai*, *ort-ana*, *ort-aran-i*, *ort-ei*, *ort-eni*, *ort-iai*, *ort-il-o*, *ort-il-á*, *ort-il-ai*, *ort-ol-ai*, *ort-ol-i*, *ort-ol-o*, *ort-or-ai*, *orto-koro*, *ort-orgo*, *ort-os-ai*, *ort-ui*, *ortu-ene*, *ortu-eri*, *ortu-eria*, *ortu-mele*.

Particularly relevant in this context are the four compounds: *Ort-aran-i*,²⁰ *Ort-orgo*, and above all *Orto-koro* and *Ortu-mele*,

¹⁵Cf. Rowland (2001:13,31), Mastino (2005, chapter I and II), Francalacci (2003 with previous literature).

¹⁶Cf. Blasco Ferrer (2009); further detailed examination in Blasco Ferrer i.p.

¹⁷Cf. Agostiniani (1993, 2000) for Etruscan, Velaza Frías (1996) for Iberian, Facchetti (2002) for Minoan.

¹⁸Cf. Taylor (1995), Geeraerts (1997), Blank/Koch (1999).

¹⁹Cf. Bach (1953, 272-299), Rohlf's (1960), Tischler (1977, 155-161).

²⁰The original correct spelling of this microtoponym (a valley!) was confirmed by Dr. G. Cabras of Urzulei; as a result of assimilation it has been quoted as **ort-orani* in the latest administrative records. Also several denominations of

non official toponyms (*lieux-dits*) of the Nuoro region (Bono) and High Ogliastra (Urzulei). We will begin by examining the recorded compounds with *-koro* and *-nele*, thus attempting to ascertain their semantic fields.

3.1. *-koro*. As said before, ['kore, -o] is an allomorph of the Paleo-Basque root *GOR- (bsq. *gorri* 'red'), used in nominal syntagmas to denote waters (*Iturrigorri* 'red spring') or qualities of ground (*Mendigorri* 'red mountain', *Gurriaran* 'red valley'). Indeed *gorri* is also documented in Sardinia, once in an exactly equivalent hybrid compound, *Funtana gorru* (lat. FONTANA, from FONTS, FONTIS 'spring'), as a derivative in another oronymic designation, *Nodu gurr-ai* (lat. NŌDUM 'knot' and metaphorically 'peak of rocky mountain'), and in other more complex toponyms: *gurri-thókinu*, *gorro-ispá*, *gorr-osp-ai*. A much more productive suffix would seem to be *-gor, kor* + Vowel: *orgose-koro*, *orgose-kore* (with *orgose* 'well, spring'), *ili-kore* (Iberian *ili* 'settlement'), *iste-kor-í* (bsq. *istil* 'marsh, mare'), *turri-kore* (bsq. *iturri* 'spring'), *nur-koro* and *nura-koro* (palsd. *nur* 'stone'), *talae-kore* (palsd. *tala* 'stream, mountain river'), *enale-kore* (lat. VĒNA, -ÁLIS 'spring', already in classical Latin *vena fontis*). There is little we can say about *Funtana bar-kori*, which displays a common root *bar-* (as in *barí*, *bar-úmini* and *baru-nele*) of unknown meaning. A quick examination of our records confirms that except for *Nodu gurriai*, *ilikore* and *nurkoro*, all the remaining compounds have a deep meaning of 'water' ('springs, wells, running water in mountain rivers and valleys'). If we now briefly go through the general Sardinian (i.e. Romance) toponymic compounds which exhibit the term 'red' (RUBEUM > *rúviu*, *ruju*, *orrúbiu*), we notice that except for *Nuraghe rúviu*, related to the reddish color of the typical stones of megalithic monuments, *Monte* (MONS, MONTEM 'mountain') *ruju*, *Terra orrúbia*, and *Perda* (PETRAM 'stone, rock') *rúvia*, the most frequent composition is with lexemes denoting 'water': *Abba rúvia*, *ruja* (AQUA 'spring, well'), *Funtana rúvia*, *Ludu* (LŪTUM 'mud') *ruju*.

Interesting, too, is the metaphoric use of 'blood' for 'red', which we have found in derivatives with the Paleo-Basque root

ortumele are subject to changes in their local forms, due to dissimilation/assimilation (*ortunele*, *ortunuli*) or to the influence of MONS, MONTEM (*mont'ortumele* > *mortumele*, *morturumele*, *murtumele*).

*DOL (DO-DOL > *odol* 'blood').²¹ The survival of the iterative reconstructed protobase in High Ogliastra is striking, as well as the subsequent result: *Do-dol-iai* and *Dol-ai*, for 'red granitic, calcareous rocks and mountains', alongside the more common *Rivu* (RĪVUS, -UM) *dol-ia*, *Bruncu* ('summit, highest rocky point') *dol-au* (and cf. *Bruncu riviū*), the plural *doli-ake*, with reference to 'springs', and the compounds *artz-ana-dolu* 'red rocky settlement' and *isti-dul-é*, *iste-dol-i* (bsq. *istil*).

3.2. *-nele*. One astonishing link with Old Iberia is the survival of the root *ib. beleß, meles*, bsq. *bele, bel-tz* 'dark', applied to geomorphologic denotata all over Sardinia. Again we find two successive stages of the root: *mele* and *nele*. The first is still maintained in a few toponyms, such as: *mara-mele* (mediterranean *MARA 'mare'), *Macu-mele* 'Macomer', a town in west Sardinia built on basaltic, dark terrain, with semitic *maqóm* 'settlement' in its first segment, *Arriu* (RĪVUS, -UM) *meli* 'river with slimy waters', *tavara-mele* (sd. *túvara* 'heather'), *kili-melis* and *keru-mele, keré-mule*, towns and terrains, designations where we recognize the typical Iberian segments *kelti-* (*kelti-beleß* is recorded 7 times; in the Iberian writing system *lt* stands for = [l]) and *kertu-* (*ib. rt* = [r], as in *ilti-* = *ili*). This first root has evolved through dissimilation to the typical Paleo-Sardinian root/suffix *nele*, and some double results confirm this evolution: *keru-mele* and *keru-nele, bidu-mele* and *bidu-nele* (bsq. *bide* 'way'), *mortu-mele* and *mortu-nele*. The following compounds, scattered abundantly in the central and eastern regions of the island, turn out to be most instructive in guiding us to the deep denotation: *turri-nele* and *turru-nele* (bsq. *iturri* 'spring'), *istiu-nele* (bsq. *istil*), *orro-nele* (bsq. *orri* 'leaves'), *aran-nulu, arau-nele* and *g-arau-nele* (bsq. *aran* 'valley'),²² *risu-nele* (RĪVUS, -UM, with normal development of [-v]- in central nuorese), *Funtana thiku-nele* (FĪCUS),²³ *gutturu-nele* (GUTTUR

²¹ Cf. Lakarra (1995, 2004).

²² Prothesis of non-etymological consonants seems to be a very productive rule in toponyms, and is triggered of course by the general tendency of Sardinian to add a consonant at the beginning of initial syllables under certain conditions (i.e. after prepositions: IENUĀRIUM > *ennarzu* > *bennarzu*, and so *Iriai* but in *Biriai*).

²³ In the Barbagia F- deletes, so that FĪCUM > ['i□u] (with *Knacklaut* or glottal stop instead of [k]). Nonetheless, prothesis of interdental [y] = *th* is not unusual, as *orgosa, urgusa* and *thurgusa* show. So, we can safely argue that [yi□u'nele] reflects FĪCUS + *nele*, a compound which is recurrent in Romance composition:

> sd. *gútturu* ‘gorge, deep ravine’). A quick survey of lexical compounds based on solely Latin roots lends further support to our equivalences: *Badde* (VALLIS,-EM), *Sa vena* (VĒNAM), *Littu* (*ELĪCTUM, ‘oak forest’, as SALICTUM), *Rivu* (RĪVUS, -UM), *Ficu* (FĪCUS.-UM), *Funtana* (FONTĀNAM) + *nieddu*, *niedda*, lat. NIGELLUM,-AM ‘dark’. We can also observe the exact correspondences with present-day Basque toponyms, such as *Aran-beltza* < *aran* + *bel(tz)* and *Beltz-iturri* < *bel-tz* + *iturri* ‘black spring’. We can now turn our attention to Iberian **ortubeleß*, completing our analysis with the Sardinian compounds of *ORTU.

3.4. **ortubeleß*, *ordumeles*

In the most archaic region of Sardinia we come across two compounds with *ORTU, which may help to make clear the real meaning of the Basque root *orto-koro* and *ortu-mele*, once with ‘red’ and once with ‘black’. But we have also mentioned *ort-orgo*, with *ORG (sd. *orga,-e*) ‘spring, wet ground’ and *ort-aran-i*, with bsq. *aran* ‘valley’, where both combinations point at precise geomorphologic features with creeks, depressions, and flowing water. The latter compound is, we believe, the solution of this puzzle: like *org-osa*, *ort-aran* conceals a tautologic designation, ‘valley’ in Iberian and ‘valley’ in Paleo-Basque; both roots are widespread in Sardinia, as are *ili* and *iri* and derivatives of *beleß* and *bele*.

4. Conclusions and Desiderata

In this short article we have attempted a new, interdisciplinary examination of Iberian, Basque and Paleo-Sardinian microtoponyms, bolstering the comparative approach with typological and semantic tools. Recent archaeological, historical and genetic evidence suggests that Sardinia received one or more migrations from Old Iberia during the Neolithic Age. Our discovery of the agglutinative typology of Sardinian toponymy, and the structural affinities we have identified between toponymic data and Iberian and Paleo-Basque roots, lead us to argue that behind the mysterious compound **ortubeleß*, *ordumeles* lies an exact correspondence with Paleo-Sardinian *ortu-mele*, with the root *ortu* sharing the common primary denotation of ‘valley’. Further research should aim at collecting new material displaying a cohesive structural and semantic interpretation for common roots

Ficu niedda.

The Journal of Indo-European Studies

in Iberian and Paleo-Sardinian. This lengthy process has just got underway.

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The Cognates of the Gothic *u*-stems

Paul Brosman[†]

The present article examines the cognates of the Gothic *u*-stems in a continuation of an attempt to determine the acceptability of an earlier proposal that the first arbitrary athematic feminines resulted from the conversion to zero-grade *i*- and *u*-stems of the cognates of the Hittite *ai*- and *au*-stems following their transfer to the pre-feminine gender. The evidence appears to support the development proposed. Although it differs in other respects from that found previously concerning the Gothic *i*-stems, an explanation for the differences is available.

Not long ago I examined the cognates of the Latin feminine *u*-stems (Brosman 2004a). The study was intended as one part of a larger process of verification of the proposal that inheritance of the cognates of the Hittite *ai*- and *au*-stems provides the basis for explaining the origin of the arbitrary athematic feminines as well as of the Latin *i*-stems with nom. sg. *-és*, the derived nouns of the Latin fifth declension, the Baltic *é*-stems and the Greek nouns in *-euw*, *-vw* and *-v* (Brosman 1984). A related subsequent proposal held that the *i*-, *ú*- and *i/yá*-stems were of the same origin (Brosman 1994). According to these views Anatolian and Indo-European inherited eight types of diphthongal noun consisting of *éi*-, *éu*-, *ói*- and *óu*-stems of each original gender.

The existence of forms of this sort should not be considered remarkable, for the ‘diphthongal nouns’ would merely have been *i*- and *u*-stems with lengthened grade in the nominative singular, parallel to the similar forms long familiar among the other resonant stems, the *r*- and *n*-stems. They were thus presumably what many now would call hysterokinetic *i*- and *u*-stems. Since the *r*- and *n*-stems each included forms containing both *é* and *ó*, it is plausible that the same was true of the lengthened-grade *i*- and *u*-stems. That the Hittite *ai*-stems indeed stemmed from forms in *-éi*- as well as *-ói*- is implied by *hastai* ‘bone’, one of two

[†]It is with profound sadness that we must inform our readership of the passing of Paul Brosman on October 23, 2010. We extend our sympathy to his family, his colleauges, and his many friends.

Hittite diphthongal nouns with known Indo-European correspondences, since its cognate occurs thematicized in Gk. ὕσθον 'bone' (Puhvel 1984–: 3. 237), which contained a stem in original **-ei-*. Although one might suspect that the inherited *éi*-stems would have largely been eliminated for phonological reasons in Hittite, verification of this suspicion is prevented by a lack of additional pertinent correspondences. In any event, details concerning the further evolution of the *ai*- and *au*-stems within Hittite are irrelevant with respect to developments in Indo-European. Whatever their fate in Hittite, it will shortly be seen that Indo-European evidence indicates that the number of inherited *éi*-stems was significant.

In Hittite phonological mergers within the long diphthongs reduced the number of diphthongal types to four, the common and neuter *ai*- and *au*-stems. In Indo-European the loss of the second element of long diphthongs produced identical forms of the nominative singular which caused confusion between the *i*- and *u*-stems, while identical forms among the oblique cases led to confusion between the *é*- and *ó*- types as well as between the diphthongal nouns and the zero-grade *i*- and *u*-stems. Presumably because of the long vowel of their nominative singular the diphthongal nouns were transferred to the newly arisen third gender which later became the feminine, a development which required the inherited neuters to adopt animate forms of the accusative singular and the nominative and accusative plural. However, since the original pre-feminines, the *á*-stems, possessed an asigmatic nominative, the addition of nom. sg. *-s* was not obligatory. As a result, the number of diphthongal types in Indo-European was also reduced to four, as the inherited distinction in gender was replaced by an 'optional *s*' in the nominative of feminine *é(i)*-, *ó(i)*-, *é(u)*-, and *ó(u)*-stems.

Because of their heteroclitic appearance and the welter of confusion to which they were subject, the diphthongal nouns were eventually lost throughout most of Indo-European. As consequences of the cross-currents of conflicting analogical influences among them, four principal methods are proposed for their elimination. Three which were suggested originally are conversion to zero-grade *i*- and *u*-stems, transfer to the distinctively feminine *á*-stems and the spread of the long vowel within the paradigm. At the same time it was noted that, like any other athematic type, the diphthongal forms could be thematicized. The fourth, which is regarded as the first step in

the development of the *i*-, *i/yá*- and *ú*-stems, is the replacement of the nominatives in *-é(s)* and *-ó(s)*, each of which occurred among both the *i*- and *u*-stems, by distinctive forms in *-í(s)* among the *i*-stems and *-ú(s)* among the *u*-stems.

In addition to extending to the zero-grade forms the confusion which had arisen between the diphthongal *i*- and *u*-stems, the conversions to zero grade were held to have produced the first arbitrary athematic feminines. It was recognized that prior to the existence of athematic feminines referring to females the conversion of pre-feminine or feminine diphthongal nouns would have resulted in animate (pre-masculine) or masculine zero-grade forms, since at that stage gender remained linked to form as it was in Hittite and the *i*- and *u*-stems of animate form belonged to the pre-masculine or masculine gender. However, after the origin of athematic feminines with female referents had severed the link with form, the converted diphthongal nouns presumably would have remained feminine and thus, in the case of those with inanimate referents, would have become the first feminines which were formally indistinguishable from masculines but did not refer to females.

Thematicization following the transfer to the pre-feminine would presumably have resulted in *yá*- or *wá*-stems. In Latin confusion between inherited *é(i)*-stems and *yá*-stem variants which they had acquired in this manner in Italic was considered to have produced the fifth-declension nouns with nom. sg. *-iés*, fifty-eight of which possessed twenty-three *yá*-stem variants but no *yá*-stem cognates. The fifth-declension forms of the type of *fidés* and the Baltic *é*-stems were held to have resulted from the spread of *é* from the nominative. The occurrence in pre-Greek of spreads of a different sort in which the vowel disseminated was combined with the resonant rather than substituted for it was regarded as explaining the Greek nouns in *-euw*, *-vw* and *-v*. The Latin *i*-stems in nom. sg. *-és* were alone in preserving a diphthongal type virtually unaltered. That forty such forms (exclusive of duplication in compounds) were found to be attested indicates that, whatever one thinks of the explanations for the fifth-declension nouns, the number of inherited *éi*-stems was indeed appreciable.

When the first set of proposals summarized above was made, two procedures were suggested for their verification. One was taking each of the nominal classes held to have preserved traces of an inherited diphthongal type and examining the cognates of

every member throughout Indo-European. The other was similar studies of the *i*- and *u*-stems in the older Indo-European dialects. In the cases of the Greek and Latin types of proposed diphthongal origin the former has been completed (Brosman 2004b and references). The results appeared to confirm that each of the types examined was indeed diphthongal in origin, that the proposed change of gender took place and that confusion of every possible sort occurred among the diphthongal nouns. The evidence of the cognates also indicated that the most important means by which the diphthongal nouns had been eliminated was conversion to zero-grade forms, though support was found for the other proposed methods of elimination as well. Although it thus appeared that the circumstances under which the arbitrarily feminine *i*- and *u*-stems were held to have arisen existed, little bearing more directly upon this question was revealed.

The examination of the cognates of the Latin feminine *u*-stems was one step in an attempt to determine whether the second suggested method of verification would provide confirmation of the proposal concerning the arbitrary athematic feminines. If the proposal is correct, all inherited feminine *u*- (and *i*-) stems with inanimate referents would have been original diphthongal nouns. The inherited masculines would include all of the original animate zero-grade *u*-stems but need not have been confined to such forms. As has been noted, original diphthongal nouns would have produced masculines through the conversion of feminine diphthongal forms to zero grade prior to the origin of athematic feminines referring to females. Indeed more recent studies of the cognates of the Gothic *i*-stems, masculine as well as feminine, while consistent with the diphthongal origin of the latter, indicated that masculines were approximately as numerous as feminines among originally diphthongal forms (Brosman 2007: 229–230). The Gothic *i*-stem evidence thus implied that an interval of substantial length intervened between the loss of the second element of long diphthongs and the origin of athematic feminines referring to females, since the diphthongal forms converted to zero grade before and after the athematic feminines arose apparently were roughly equal in number.

The evidence concerning the Latin feminine *u*-stems apparently supported the proposed origin of the arbitrary athematic feminines. The number of pertinent *u*-stems was six to eight, depending on the inclusion of *vitus* ‘wheel rim’, which may well have been borrowed from Greek, and **noctus* ‘night’, which

could not be established as feminine, since it was confined to the adverbial expression *noctú* ‘at night’, in which it occurred unmodified. Within the maximum number of eight, six forms, *acus* ‘needle’, *quercus* ‘oak’, *manus* ‘hand’ and *domus* ‘house’, as well as the uncertain *vitus* and **noctus*, apparently possessed cognates of types appropriate for a former diphthongal noun. The two exceptions were *metus* ‘fear’, which had no attested cognates at all, and *tribus* ‘tribe’, which possessed a single *u*-stem cognate elsewhere in Italic (Brosman 2004a: 263). There thus appeared to be no reason to question the view that the Latin feminine *u*-stems were of diphthongal origin.

The present article extends the examination of the feminine *u*-stems to include the forms attested in Gothic. In addition it will consider the cognates of the Gothic masculine *u*-stems as well in order to compare the evidence concerning them to that seen in the case of the feminines, since it was found possible in this instance to deal with the forms of both genders within a single article of moderate length.

Exclusive of duplication in compounds the Gothic dictionaries of Feist (1939) and Lehmann (1986) each include seventy-three forms attested as animate *u*-stems, thirty-one of which contained a productive suffix. As was done in the case of the Gothic *i*-stems, the forms of the latter sort, which consisted of fourteen containing **-tu-*, twelve *-assu-* and five *-ópu-*, *-ódu-*, were excluded here.¹ In this connection it should be noted that with one exception the identification of these forms, all of which were of masculine or undetermined gender, was based on the views of Feist and Lehmann. The lone exception, *luftus* ‘air’, is described by Feist and Lehmann as of uncertain etymology. However, as was stated in greater detail when the *tu-* forms were discussed previously, the view here is that its apparent *ti-* variants and cognates in Germanic point with sufficient clarity to the inheritance of a related pair of original *tu-* and *ti-* abstracts that, even though one cannot confidently identify the Indo-European root from which it was derived, it probably should be considered to belong among the *tu-* forms (Brosman 1997: 31–32).

Since the forty-two remaining forms included eight obvious foreignisms or loanwords of biblical origin and an equal number of other words, *asilus* ‘donkey’, *assarjus* ‘a coin’, *kintus* ‘penny’,

¹ The originally abstract Gothic *tu-* forms have already been examined separately and found apparently to have been primarily *u*-stem variants of abstracts in **-ti-* (Brosman 1997).

kubitus ‘company of people at a meal’, *marikreitus* ‘pearl’, *nardus* ‘oil of nard’, *sakkus* ‘sack’ and *ulbandus* ‘camel’, held by both Feist and Lehmann to have been borrowed from other sources, the number of *u*-stems to be treated here is reduced further to twenty-six, twenty-two of which were masculine and four presumably feminine (Feist 1939: 59-60, 60, 312, 315, 348, 370, 407, 515; Lehmann 1986: 45, 219, 221, 245, 263, 293, 375). However, five of the masculines, *airus* ‘messenger’, *aúhsus* ‘ox’, *magus* ‘boy’, *sunus* ‘son’ and *wiprus* ‘calf’, possessed referents of male or, perhaps in one case, unspecified sex (Feist 1939: 26, 66, 339, 460, 571; Lehmann 1986: 19, 164, 240, 330, 408). For this reason they were also set aside, leaving seventeen masculine and four feminine potentially inherited distinct *u*-stems with inanimate referents.

The evidence of the cognates of the feminines was similar to that seen in the case of the Latin *u*-stems. The four forms were *-qairnus* ‘mill’ in *asilu-qairnus* ‘donkey-mill’, *-waddjus* ‘wall’, which is found in three compounds, *baúrgs-waddjus* ‘city-wall’, *grundu-waddjus* ‘foundation’ and *mid-gardi-waddjus* ‘dividing wall’, *kinnus* ‘cheek’ and *handus* ‘hand’. It should be noted in the case of *-waddjus* that although *baúrgs-waddjus* is attested as feminine, *grundu-waddjus* was masculine and *mid-gardi-waddjus* of unidentifiable gender. However, it was included here among the feminines, since it is improbable that an inherited masculine would have been transferred to the extremely rare Gothic feminines.

Two of the feminines, *-qairnus* and *-waddjus*, possessed cognates indicative of diphthongal origin, while it is possible but uncertain that a third, *kinnus*, had a single such correspondence. In the case of *-qairnus*, the feminine *u*-stem OE *esol-cweorn* ‘donkey-mill’ corresponded precisely to *asilu-qairnus*. Otherwise, however, *ó*-stem cognates occurred throughout Germanic in OE *cweorn* and in ON *kvern*, OS *quern* and OHG *chwirna*, *churn* ‘mill’. Although the frequent transfer of feminine *u*-stems to the *ó*-stems in Old English could account for the occurrence of an *ó*-stem there, a similar explanation would not apply elsewhere in Germanic. Moreover, the existence of an inherited *á*-stem is apparently confirmed by Lith. *girnà* ‘millstone’. It thus appears appropriate that Feist and Lehmann give the etymon as *gw@nu-*, *gwerná-*. In addition, a *ú*-stem cognate is attested in OCS *zriny* ‘mill’ (Feist 1939: 59; Lehmann 1986: 44; Pokorny 1959: 477).

The only Germanic cognate of *-waddjus* was the *i*-stem ON *veggr* ‘wall’. That *veggr* was masculine does not prevent it from

serving as an example of an *i*-stem variant of a *u*-stem. The variation between *i*- and *u*-stems arose among the diphthongal nouns. Later both of the diphthongal variants were often converted to zero grade, but not necessarily at the same time. It thus is possible that in this case the *i*-stem was converted before the origin of athematic feminines with female referents and the *u*-stem after that development had occurred. Outside Germanic *á*-stems are found in Skt. *vayá*, OCS *v±ja* ‘branch, bough’ and OIr. *fé* ‘twig’, which were at one time near synonyms of *-waddjus*, the original meaning of which was ‘wattle, woven wall’ (Feist 1939: 538; Lehmann 1986: 386; Pokorny 1959: 1120-1121).

Among the apparent Germanic cognates of *kinnus* there occurred a variety of forms: OS *kinni* (f.), OHG *chinni* (n.), the *ó*-stem OE *cinn* ‘jaw, chin’ and a feminine consonant stem in ON *kinn* ‘cheek’. However, the variation is of little import from the present point of view. The *-nn-* of *kinnus* is explained as resulting from the cluster *-nw-* which occurred among its oblique cases. Since each of the other Germanic forms also contained *-nn-*, it seems safe to say that in every case the *u*-stem corresponding to *kinnus* had been inherited and that the different forms occurring in the different dialects were not the reflexes of variants occurring in the parent speech but stemmed from it through developments which took place separately among the separate Germanic dialects. Elsewhere the feminine *u*-stem was inherited also in Greek, Indo-Iranian, Celtic and Tocharian. That Latin should also be included is indicated by the derivative *genuinus* ‘related to the cheek or jaw’. However, the *á*-stem *gena* ‘cheek’ also occurred in Latin. It has been explained through analogy with *mála* ‘cheek-bone, jawbone’ (Brugmann 1906–11: 1.179). Although this proposal is described as disputed by Feist (1939: 312), it is accepted by Lehmann (1986: 218), Walde-Hofmann (1938–54: 1.590) and Pokorny (1959: 381). The analogy would indeed appear plausible even if *genuinus* were not attested. Therefore, although the inheritance of *gena* cannot be wholly ruled out, particularly in view of what was seen concerning *-qairnus* and *-waddjus*, it seems too uncertain to be cited as evidence of the diphthongal origin of *kinnus*.

Concerning the fourth feminine, *handus* ‘hand’, there is little that can be said. Of uncertain etymology, it possessed cognates confined to Germanic (Feist 1939: 244; Lehmann 1986: 176). Since these forms included *u*-stems in OE *hond*, OS *hand* and OHG *hant* ‘hand’, it seems safe to say that, as held by Kluge-

Seebold (2002: 388), it stemmed immediately from a Germanic *u*-stem. Although ON *hœnd* was a consonant stem, it presumably resulted from a development occurring in Germanic, for it has been seen that ON *kinn*, which seemed clearly to have been an earlier *u*-stem, was a consonant stem also. It thus appears that in Old Norse, where feminine *u*-stems no longer existed, there had been to at least some extent a tendency to transfer the few of them that had been inherited to the consonant stems.

It should also be noted that Lehmann holds **handu-* to have been a Germanic innovation. If this were true, it would be of some consequence, for it would reduce the number of inherited feminine *u*-stems to three. In that case, the occurrence among them of at least two forms of apparent diphthongal origin would be precisely proportional to the evidence found in Latin, where four of six forms of seemingly certain inheritance possessed cognates indicative of such origin. However, the view here is that this proposal is incorrect, since not only were the *u*-stems as a whole unproductive in Germanic or anywhere else (Brugmann 1906–11: 1.180), but it is especially improbable that a form derived in Germanic would have been assigned to the rare feminine *u*-stems. It thus appears that Gmc. **handu-*, like Lat. *metus* (Brosman 2004a: 257), should be regarded as an inherited feminine *u*-stem of unknown etymology. The Gothic evidence nevertheless seems sufficiently consistent with that of Latin to be held to provide further support for the diphthongal origin of the arbitrarily feminine *u*-stems.

As has already been noted, the evidence of the Gothic *i*-stems indicated that masculines of diphthongal origin were approximately as numerous as feminines. It will be seen here later that in some respects the history of the diphthongal *i*-stems deviated greatly from that of the *u*-stems. However, since in each case the relative frequency of the genders among the former diphthongal nouns, regardless of their absolute numbers, was presumably determined by whether the diphthongal *i*- or *u*-stems had been converted to zero grade before or after the origin of athematic feminines referring to females, there is apparently no reason not to expect it to have been more or less the same for both types. It thus is appropriate that among the seventeen masculines the number of forms for which there was evidence of diphthongal origin was again two.

One of the two masculines, *hallus* ‘cliff, rock’ (< **kolnus*), apparently had at least one *i*-stem variant in Lat. *collis* ‘hill’ (<

**kolnis* or **kōnis*). Although Feist (1939: 241) and Lehmann (1986: 174) for some reason are somewhat uncertain about connecting the two forms, Walde-Hofmann (1938–54: 1.245) and Pokorny (1959: 544) do not hesitate to do so and would add the Germanic form OE *hyll* ‘hill’, attributed by them to **kōnis*, as a second such variant. In the latter case the Germanic etymon is given by Feist and Lehmann as **hulnja-*, which could have resulted from thematization of the cognate of *collis*. The *a*-stem ON *hallr* ‘stone’ also occurred in Germanic. Although it could have been a transfer from the *u*-stems, it seems more likely to have been inherited, for Lith. *kálnus* ‘mountain’ and Mir. *coll* ‘chief, leader’ indicate that an *o*-stem existed in the parent speech, where it probably was produced by independent derivation as part of the general proliferation of the *o*-stems.

The second form was *walus* ‘staff’. In addition to a precise cognate in ON *voolr* ‘round staff’, it had beside it in Germanic an *ó*-stem in OE *walu* ‘wale, ridge or stripe from a blow’. Also occurring were *n*-stems of differing genders in the compounds OE *wyrt-wala* and OHG *wurzala* (< **wurt-waló*) ‘root’. That the Old High German form was feminine is perhaps explained through confusion between the *ó*- and feminine *n*-stems. Elsewhere there occurred an *i*-stem in Opr. *walis* ‘shaft of a wagon’ and the Baltic *é*-stems Lith. *vol̃* ‘wooden mallet, beater, drum-stick’ and Latv. *vāle* ‘washing beater’ (Feist 1939: 549; Lehmann 1986: 393; Pokorny 1959: 1142–1143). Pokorny also cites a second *á*-stem in Lat. *vola* ‘cavity of the hand, arch of the foot’, which he considers to correspond to OE *walu*. Walde-Hofmann (1938–54: 2.825) agrees that *vola* contains the root held by the other scholars to be found in the forms above but does not mention *walu* or any of the other forms cited here. Although Pokorny’s comparison is possible, for semantic reasons it seems somewhat too uncertain for unreserved acceptance. Although it would raise the question, rejection of the comparison would not require rejection of the diphthongal origin of either of the *á*-stems. Since the evidence above indicates that an original diphthongal noun developed *u*- and *i*-stem variants, another possibility is that both *á*-stems were of diphthongal origin but instead of being immediate cognates stemmed from different diphthongal variants in the parent speech and began evolving along different semantic paths at that stage.

None of the remaining fifteen masculines possessed a cognate which could be taken to suggest diphthongal origin,

though it is not unlikely, since the total number of feminines was four, that one or two of them were former diphthongal nouns for which no evidence of their origin happened to survive. One form, *drunjus* ‘sound’, had no cognate anywhere, while the apparent cognates of eight others, like those of the feminine *handus*, were confined to Germanic. The latter forms were *grédus* ‘hunger’, *hairus* ‘sword’, *húhrus* ‘hunger’, *sidus* ‘custom’, *wandus* ‘rod’, *fair^us* ‘world’, *stobjus* ‘dust’ and *wintrus* ‘winter’ (Feist 1939: 127, 220, 235, 273, 418, 550, 139, 457, 566; Lehmann 1986: 96, 160, 171, 193, 301, 393, 105, 328, 404; Pokorny 1959: 255, 441, 940, 565, 883, 1148, 822, 79). In the cases of the first five, the forms occurring beside them were consistent with the inheritance of masculine *u*-stems, for when not themselves *u*-stems, they were masculine *a*- or *wa*-stems. The cognates of one of the remaining forms, *wintrus*, deviated from this pattern by including a masculine consonant stem in ON *vetr*, while those of another, *fair^us*, consisted of the *a*-stems ON *ffœr* ‘life’, OE *fearh* ‘life’ and OHG, OS *ferah* ‘soul, life’, which were neuter more often than masculine. In the case of the third, *stobjus*, the only potential cognate was the neuter *ja*-stem OHG *stuppi* ‘dust’. Although the precise histories of these forms are somewhat less certain, there is no reason to think that a diphthongal noun was involved.

Concerning four of the six remaining forms extra-Germanic evidence was hardly more abundant than before but not entirely without significance. The Germanic forms corresponding to *paurnus* ‘thorn’, ON, OE *þorn*, OS *thorn*, OHG *dorn*, presumably were secondary *a*-stems, since the transfer of masculine *u*-stems to the *a*-stems, not unknown in Old High German and Old Saxon, was frequent in Old English and Old Norse and a *u*-stem is attested elsewhere in OCS *trûnû* ‘thorn’ (as is Skt. *trnam* ‘grass, straw’) (Feist 1939: 492; Lehmann 1986: 357; Pokorny 1959: 1031; Kluge-Seebold 2002: 211). Among the Germanic forms beside *lipus* ‘limb, member’, were a *u*-stem in ON *lidr* and an *a*-stem in OE *lip*, the secondary nature of which is in this instance indicated by its occurrence as a *u*-stem as the first element in compounds. Although *i*-stems occurred in OHG *lid* and OS *lid*, it seems safe to say that they resulted from the usual transfer of masculine *u*-stems to *i*-stems in Old High German and Old Saxon, rather than a development involving diphthongal *i*- and *u*-stems, for in Old Saxon *u*-stem forms are again found as the first element in compounds. That OHG *lid* was feminine in a few instances

presumably is an example of the occasional variation in gender among the Old High German masculine and feminine *i*-stems. Neuter *a*-stems also occurred in OHG *lid* and OE *lip*, which, like all of the other forms mentioned here, meant ‘limb, member’. The only related form found outside Germanic was Lat. *lituus* ‘curved staff borne by an augur’. Since the words for ‘limb’ are attributed to the root **ley-* ‘move, bend’, it presumably resulted from thematization of the *u*-stem inherited by Germanic (Feist 1939: 332; Lehmann 1986: 234; Pokorny 1959: 309; Kluge-Seebold 2002: 361; Walde-Hofmann 1938–54: 1.815).

In the case of *skadus* ‘shadow’, one finds masculine *wa*-stems in OHG *skato* and OS *skado*. Contrary to Feist (1939: 427) and Lehmann (1986: 308), who identify it as masculine, OE *sceadu* was a *wó*-stem (Pokorny 1959: 957; Kluge-Seebold 2002: 795). Why *sceadu* was feminine is not clear. However, there is little reason to believe that it stemmed from an inherited feminine *u*-stem which had been partially transferred to masculine, since the tendency in Old High German and Old Saxon was to eliminate the masculine *u*-stems as well as the feminine. The neuter *a*-stem OE *scead* ‘shadow’ and masculine Gk. *skōtow* ‘darkness’ also occurred. Although their precise relationship to the other forms is also uncertain, it is of little import, since neither raises a question of diphthongal origin. The fourth form, *wripus* or **wrépus* ‘herd’, occurred beside the masculine *a*-stem OE *wráep*, *wráed* ‘herd’ and Skt. *vrātas* ‘troop, crowd’ (Feist 1939: 574; Lehmann 1986: 411; Pokorny 1959: 1151). Whether *wráep* corresponded precisely to *vrātas* or resulted from the Old English transfer of a masculine *u*-stem to the *a*-stems is again of little moment.

The final two masculines, *fótus* ‘foot’ and *tunpus* ‘tooth’ are of disputed inheritance. The extra-Germanic evidence, which is ample for each, points almost exclusively to a pair of inherited root nouns. In the case of *fótus* such forms occur in Skt., Av. *pad-*, Gk. *p̄w* and Lat. *pés*. In that of *tunpus* original root nouns are found in Skt. *dán*, Gk. *Û-d̄n*, Lat. *déns* and OIr. *dét*. Although Lith. *dantis* was an *i*-stem, gen. pl. *dantÛ* indicates an earlier Baltic consonant stem. Outside Gothic the evidence in Germanic is consistent with the inheritance of root nouns corresponding to those above, for ON *fótr*, *tōnn* and OE *fót*, *tóp* were consonant stems. Since *man* ‘human’ was the only monosyllabic masculine consonant stem to survive in Old High German and Old Saxon, OHG *fuoz*, *zan(t)* and OS *fót*, *tand* are

presumed to have been inherited consonant stems, though transfer instead from the *u*-stems cannot unequivocally be ruled out. It should be noted that ON *tǫnn* was feminine. Its gender has been explained by A. Sturtevant (1932: 248) through analogy with the feminine consonant stems *hǫnd*, *mǫrk*, *rǫng*, *tǫng*, *rǫnd*, and *strǫnd*, which contained the same radical vowel. Although the view here is that a semantic association with *kinn* seems somewhat more likely to have been responsible, the two proposed analogies are not mutually exclusive.

In view of the evidence above it has been proposed (Brugmann 1906–11: 2.276) that *fótus* and *tunpus* were formed in pre-Gothic on the basis of inherited consonant-stem accusatives containing Gmc. *u*. However, the *u*-stem *pádús* ‘foot, stride’, which would correspond precisely to *fótus*, is attested once in Vedic. Citing it, Feist (1939: 159, 483) holds both *fótus* and *tunpus* to have been inherited from Proto-Indo-European. Lehmann (1986: 121, 349) appears more favorably inclined toward Brugmann’s view but does not explicitly endorse it. Pokorny (1959: 790, 289) accepts the analogical explanation of *tunpus* but does not mention it in connection with *fótus*. Walde-Hofmann (1938–54: 1.340; 2.294) also agrees with Brugmann concerning *tunpus* but, while mentioning his proposal, takes no position concerning it in the case of *fótus*. On the other hand, Kluge-Seebold (2002: 324, 1003) agrees with Feist that both consonant and *u*-stems meaning ‘foot’ were inherited by Germanic but does not mention the question when discussing the words for ‘tooth’. The view here is that, though the position of Feist seems most likely to be correct, the inheritance of *fótus* and *tunpus* should be considered too uncertain for acceptance. However, from the present point of view their elimination is of little consequence, for its only effect is to reduce the number of inherited masculines with inanimate referents from seventeen to fifteen. It thus remains true that among the pertinent forms masculines were approximately four times as numerous as feminines.

In addition to the cognates of the individual forms, the relative rarity of the feminines supports the view that the feminine *u*-stems were of diphthongal origin. Since the proposal concerning the origin of the arbitrary athematic feminines holds the masculines to have included all of the original zero-grade forms plus a portion of the former diphthongal nouns and the feminines to have been confined to the remaining portion of the

original diphthongal forms, the masculine *u*-stems should be expected to have been distinctly more numerous than the feminines if the proposal is correct. Since it has been found more recently in the course of the proposal's verification that the evidence concerning the Gothic *i*- and *u*-stems apparently agrees in indicating separately that the number of masculines of diphthongal origin was approximately equal to that of feminines, it appears that the disparity in the frequency of the genders should be especially pronounced. For example, even if the original zero-grade and diphthongal *u*-stems had been equal in number, the masculines should apparently be expected to have been roughly three times as numerous as the feminines. That the figures for the Gothic *u*-stems exceed that ratio thus indicates that the zero-grade forms had originally been appreciably more common than the diphthongal nouns. In this respect the Gothic evidence was again similar to that of Latin as far as can be told, for the number of feminine *u*-stems found in the more abundantly attested Latin was only slightly larger than that of those occurring in Gothic and, although the Latin masculine *u*-stems have not yet been examined, it is well known that they far outnumbered the feminines.

However, it should be noted that among the Gothic forms the evidence seen here concerning the *u*-stems differs greatly from that found in the case of the *i*-stems. Although it appeared from the Gothic evidence that among both *i*- and *u*-stems of diphthongal origin masculines and feminines were approximately equal in number, the parallelism extended no further. Whereas among the *u*-stems regardless of origin masculines were four times as frequent as feminines, among the more numerous *i*-stems masculines outnumbered feminines by only twenty-one to thirteen. Since the thirteen feminines were matched by an equal number of masculines identifiable as diphthongal in origin, there were left only eight potentially original zero-grade *i*-stems. Thus in contrast to the indication of the *u*-stem evidence that at the time of the loss of the second element of long diphthongs zero-grade forms were more numerous than diphthongal nouns, that of the *i*-stems indicated that quite the reverse was true (Brosman 2007: 226, 229).

The disparity in the evidence concerning the *i*- and *u*-stems may be explained by productivity on the part of the diphthongal *i*-stems held to have occurred in Proto-Indo-European prior to the loss of the second element of long diphthongs. In addition to

the proposals summarized here at the outset, which dealt with developments among the diphthongal nouns as a whole following the loss of the second element of long diphthongs, others have been made more recently concerning the history of the diphthongal *i*-stems prior to the phonological change (Brosman 2005). It was noted that since the Hittite common *ai*-stems, the Latin *i*-stems with nom. sg. *-és* and the Greek nouns in *-v* were all closely associated with verbal abstracts (E. Sturtevant 1937: 61–62; 1951: 69; Friedrich 1960: 39; Kronasser 1966: 204–205; Ernout 1965: 23–24; Kurylowicz 1966: 19–20; Schwyzer 1939: 478; Brosman 2005: 191–194), it seems clear that the animate *éi*- and *ói*-stems were productive in the derivation of such forms prior to the separation of Anatolian. Evidence that the productivity continued beyond the separation was found in Hittite, where the occurrence beside the secondary verbs *maniyahh-* ‘govern’ and *istarnink-* ‘sicken’ of the further derivatives *maniyahhai-* ‘government’ and *istarningai-* ‘sickness’ indicated that the *ai*-stems were still productive in the derivation of action nouns in pre-Hittite (Friedrich 1952: 135, 92; Puhvel 1984–: 6.49–51; 2.476). It thus is plausible that in Proto-Indo-European the animate diphthongal *i*-stem abstracts remained productive until their paradigms were disrupted by the loss of the second element of long diphthongs. That such was in fact the case is indicated by the semantics of the zero-grade *i*-stems. In Hittite the *ai*-stems were associated with action nouns but the *i*-stems were not. However, in Indo-European *i*-stem forms of both types displayed a close connection with verbal abstracts. Because conversion to zero grade was the most important means employed in the virtual elimination of the diphthongal nouns as a whole, an obvious explanation for the zero-grade abstracts of Indo-European is the conversion of diphthongal abstracts to zero-grade forms. Since Brugmann (1906–11: 1.167–170) has indicated that a large majority of the Indo-European *i*-stems consisted of action nouns, it was concluded that the diphthongal abstracts had indeed remained productive until the loss of the second element of long diphthongs and that as a result by the time of the phonological change the diphthongal *i*-stems had become perhaps more numerous than the corresponding zero-grade forms. The reason given for describing the relative frequency of the two types of *i*-stem in such vague terms was the possibility that the zero-grade abstracts became productive to some extent in their own right after they had arisen. Otherwise

the diphthongal forms would have been held to have been considerably more numerous. The same evidence also implies that the diphthongal *i*-stems of other meanings were not appreciably productive during that time, though they presumably were not yet subject to elimination to a significant degree (Brosman 2005: 196, 200–201).

The examination of the Gothic *i*-stems was intended to assist in the verification of the proposals concerning the earlier history of the diphthongal *i*-stems as well as of those regarding the origin of the arbitrary athematic feminines. As can be seen from the evidence cited here, which indicated that among the *i*-stems those of diphthongal origin were considerably more numerous than original zero-grade forms, it appeared to confirm the previous proposals concerning the productivity of the diphthongal *i*-stem abstracts and to render more precise that which described the relative frequency of the originally different *i*-stem types. Moreover, the implication of the numerical superiority of the former diphthongal nouns that the zero-grade *i*-stem abstracts did not become significantly productive after their origin could apparently be corroborated by other evidence. Since no Gothic form identified as an abstract which had an extra-Germanic *i*-stem cognate did not also possess other correspondences indicative of diphthongal origin, there was no evidence of a Proto-Indo-European *i*-stem abstract which could have resulted from such productivity (Brosman 2007: 228).

Since there was no reason to think that the diphthongal *u*-stems had been appreciably productive, the differing evidence concerning the Gothic *u*- and *i*-stems was not unexpected. That the contrast was so striking apparently confirms that the diphthongal *u*-stems were not productive and provides further support for the proposed productivity of the diphthongal *i*-stems. It also supports the explanation proposed for the arbitrary athematic feminines, for the Gothic *i*-stems, which contained a much greater proportion of apparent former diphthongal nouns, also included a much larger number, in both absolute and relative terms, of feminine forms. That the *i*-stems were more numerous than the *u*-stems overall was apparently also due to the productivity of the diphthongal *i*-stems. It may be added that evidence of this sort appears not to be confined to Gothic. Although the incomplete Latin evidence provides only partial corroboration, Whitney (1889: 116) has reported that in Sanskrit *i*-stems were more numerous than *u*-stems and that the difference

was especially great among feminines.

The principal conclusion drawn from the evidence concerning the Gothic *u*-stems was that it appeared to provide additional support for the proposed origin of the arbitrary athematic feminines. One source of this support was the cognates of the feminine forms, which closely resembled those of the Latin feminine *u*-stems in their apparent consistency with the diphthongal origin of such forms. In the case of Gothic this evidence was reinforced by its contrast with that concerning the masculines, at least a solid majority of which were presumably original zero-grade forms. It also seemed significant that, as far as could be told, the *u*-stem evidence agreed with that of the Gothic *i*-stems in indicating that among original diphthongal nouns masculines and feminines were approximately equal in number, though the small number of relevant *u*-stems made this evidence possibly less reliable concerning this question. That the *u*-stems contained a much smaller number of forms apparently of diphthongal origin than did the *i*-stems was consistent with another previous proposal, which held that prior to the loss of the second element of long diphthongs the diphthongal *i*-stems had been productive in the derivation of verbal abstracts. Presumably it was for this reason that the evidence of the two Gothic types differed concerning the relative frequency of zero-grade and diphthongal forms at the time of the phonological change. Whereas in the case of the *i*-stems the diphthongal forms had been found to have been more numerous, the evidence seen here that the masculine *u*-stems were four times as common as feminines indicated that among the *u*-stems the zero-grade forms had been more frequent.

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Proto-Germanic **krēþja-* and Proto-Slavic **krěpbъ* ‘strong’

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In the present article, I argue that the received etymological connection of Proto-Germanic **kraftu-* ‘power’ with ON *kræfr* ‘strong’ is misleading, because the adjective 1) does not mean ‘strong’ and 2) must be analyzed as a direct derivation from ON *krēfja* ‘to demand’. Alternatively, we may connect **kraftu-* with **krēþja-*, a formation that I reconstruct on the basis of the Visperterminen Swiss form *xrēþfe* ‘strong’. This adjective was possibly borrowed into Slavic, so as to emerge as Old Church Slavonic *krěpbъ* ‘strong’.

1. The reconstruction of the Proto-Germanic form of ON *krōftr* m., OE *cræft* m., OHG *chraft*, gen. *chrefti* f. ‘force, strength’ poses little problems. The attestations point to two slightly different Germanic proto-forms, i.e. **kraftu-* and **krafti-*, which are further mirrored by the appurtenant adjectives ON *krōftugr*, OE *cræftig*, OHG *chreftig* etc. Since there is an established tendency for *u*-stems to shift to the *i*-stems in West-Germanic, cf. OHG *skilt*, pl. *-i* ‘shield’ vs. ON *skjoldr* ‘id.’, OHG *heit*, pl. *-i* ‘nature, manner’ ~ Go. *haidus* ‘id.’, I assume that the *tu*-stem is original, and that it was remodeled into a *ti*-stem in West Germanic. This leaves us with a root **kraf-*, **krap-* or **krab-* as the derivational base.

Two suggestions have been made concerning the etymology of **kraftu-*. It is traditionally connected with ON *krēfja* ‘to demand’ (< **krabōjan*) and OE *cræfian* ‘to crave’ < **krabjan*.¹ It must be said, however, that this linkage is not necessarily convincing on the semantic side. The alternative connection with ON *kræfr* ‘strong, brave’ < **krēfi-*, as given by

¹The Old Norse preterite *krāfða* points to a weak conjugation, but in some Norwegian dialects the verb is strong, i.e. *krēvja*, pret. *krōv*. Although the transition from strong to weak verbs is more common, this can be an innovation.

Fick/Falk/Torp (p. 52), is semantically more attractive, not in the least in view of the dialectal Norwegian form *kræv* ‘able, tough’ (Aasen 1850: 241). It turns out, however, that *kræfr* originally did not mean ‘strong’, so that the direct linkage of this adjective to **kraftu-* must be reconsidered.

2. It is, in fact, not an easy task to establish the exact meaning of Old Norse *kræfr*, the word being a hapax. Fritzner (1886-1896), Heggstad (1930) and Zoëga (1910) do not mention it at all. De Vries (1962: 329) does refer to *kræfr* under *krefja*, but an actual entry is lacking. The only dictionary that has included the word and specifies where it is attested is the 1860 edition of the *Lexicon Poeticum* (p. 477).² According to this dictionary, *kræfr* only occurs once in *Skáld-Helga rímur* 6, 7: *váþna þorr í vígi kræfr* ‘the weapon tree [= Helgi] in battle ...’.³ Evidently, the word can mean many things in this context, including ‘strong’. Cleasby and Vigfússon in their *Icelandic-English dictionary* gloss *kræfr* as ‘daring’ (p. 357), which in the textual context seems no better or worse than ‘strong’. But this meaning seems to be based on modern usage, as it is labeled “conversational”, i.e. colloquial by Cleasby and Vigfússon.

The modern Icelandic meaning is actually much broader, however. Árni Böðvarsson’s *Íslensk orðabók fyrir skóla og skrifstofur* provides two different translations for *kræfur*, that is 1) *seigur* ‘tough’, *sêður* ‘thrifty’, *sleipur* ‘sleek’ and 2) *sem unnt er að kreffast* ‘what is to be claimed (of debt)’. Clearly, the meaning ‘strong’, which is given by the etymological dictionaries, does not cover the full semantic load of the word. One may therefore wonder whether this gloss was not simply inspired by the proposed connection with **kraftu-*.

To my mind, the original meaning and derivation of *kræf(u)r* is betrayed by Böðvarsson’s latter definition, i.e. ‘what is to be claimed, claimable’. In view of these unambiguously gerundival semantics, *kræfur* must be compared to *vꝛddhi-* gerundives such as ON *auð-skæðr* ‘easily damaged’ < **skōþi-* to *skaða*, *dræpr* ‘allowed to be killed’ < **drēþi-* to *drepa*, *ætr* ‘edible’

²The entry is lacking in the later editions.

³Normalization mine. Note that, since *æ* and *ǣ* (ǿ) had already merged into *æ* in the 16th century (and later) manuscripts of the *Skáld-Helga rímur* (publ. Rafn 1838; Jónsson 1905-1912 I: 105-165), the original Old Norse vocalism can only be reconstructed on the basis of Nw. dial. *kræv*.

< **ēti-* to *eta*, *sær* ‘visible’ < **sēhwi-* to *sjá* etc. It seems therefore logical to assume that *kræfr* was at some stage derived from *krefja* < **krabjan-* (cf. Blöndal 1989: 512), or, given its *ē*-vocalism, from a related but non-attested strong verb **kref/ban-*.⁴

The other meanings given by Bōðvarsson, i.e. ‘thrifty’, ‘tough’ and ‘sleek’ can easily have developed out of the old gerundive as well, even though they rather seem to point to a gerund-like aspect, e.g. “claiming” or “urging” (as in Faroese *krevja* poet. ‘to urge’). Such a diathetical shift of roles is far from unique. The element *tækr*, for instance, means “takable” in e.g. *laus-tækr* ‘easily taken’, but “able to take” in compounds such as *fá-tækr* ‘poor’ and *djarf-tækr* ‘bold in taking’. The simplex *færr*, an old gerundive to *fara*, means both ‘able to go’ and ‘passable, safe’. Similarly, *auð-sær* means ‘easily seen’, i.e. “what is easily to be seen”, while *djúp-sær* translates as ‘smart’, i.e. “able to see deeply”. The semantic differentiation between *kræfur* ‘claimable’ and *kræfur* ‘thrifty, tough’ should therefore be explained as resulting from the difference between passive and active use of the gerundive to *krefja*. There is consequently no compelling reason to suppose two separate words, i.e. *kræfur* 1 and 2 (pace Blöndal loc. cit.)

3. Since *kræfr* must be a derivation from *krefja*, the direct connection with **kraftu-* is to be reconsidered. The link can only be maintained through **krabjan-*, assumed that this verb is indeed related to **kraftu-*. For the latter, there is an alternative etymology for **kraftu-*, however. Potentially relevant material can be drawn from the Swiss dialects, such as the archaic vernacular of Visperterminen in Wallis/Valais, which was amply documented by Elisa Wipf as early as in 1910. In a highly valuable lexicon of this dialect, which was compiled by Fides Zimmermann-Heinzmann over more than twenty years, and finally published in 2000, we encounter the adjective *xreepfe* (m.), *xreepfi* (f.), *xreepfs* (n.) ‘strong’.⁵ This adjective, which has correspondences in Graubünden and

⁴Cf. Schwenck (1834: 353): “Der Stamm muß [Gotisch] *kriban* geheißten haben.” Note that **kriban* with its ending *-an* could theoretically also have been a third class weak verb. It is clear, though, that Schwenck did not have this in mind.

⁵Zimmermann-Heinzmann elucidates the word with the phrase *æs išt as xreepfs Mæmmi* ‘it is a strong baby’ (orthographical rendering mine).

Sankt Gallen,⁶ seems to be of considerable antiquity. Visp. *ee* is the regular reflex of fronted OHG *ā* < PGm. **ē*.⁷ Thus, *xreepfe* looks like the regular reflex of an Old High German form **chrāpfī*. This **chrāpfī*, in turn, directly leads back to PWGm. **krāþþja-* through the High German sound shift, and further to PGm. **krēþja-* with West Germanic *j*-gemination⁸. So, in spite of the fact that the adjective is attested two millennia after the final stage of the Germanic parent language, it can be reconstructed with great accuracy. We may consequently consider to connect it with OHG *chraft* and related forms (cf. Pokorny 1959-1969: 385-390 on *chrāpf*).

4. Interestingly, the reconstruction of **krēþja-* is not only relevant for Proto-Germanic. It also has a bearing on the Slavic languages, which have a close parallel, i.e. PSlav. **krěþь* ‘strong’, cf. OCS *krěþь* ‘id.’. Usually, the etymological dictionaries connect this *krěþь* with ON *hræfa* ‘to tolerate’ (Pokorny 1959-1969: 620; Vasmer 1967: 2, 372; Derksen 2008: 246), which is quite remote semantically. It is conceivable, however, that the word was borrowed from Germanic **krēþja-* at the Proto-Slavic stage, the formal and semantic match being more complete. It would then have to be added to the large corpus of nouns, adjectives and verbs that were adopted by the Slavs from Germanic in the prehistoric period, cf. PSI. **bugь* ‘bracelet’ << PGm. **bauga-*, **nutь* ‘cattle’ << **nauta-*; **gotovь* ‘ready’ << **gatawa-*, **xōdogь* ‘dexterous’ << **handaga-*; **gonesti* ‘to heal’ << **ganesan-*, **goněti* ‘to suffice’ << **ganah-* ‘suffices’ (cf. Lith. *ganā* ‘enough’), etc. (cf. Bräuer 1961: §14).

Beside **krěþь*, the Slavic languages also bear evidence of a form extended with a *kr-*suffix, e.g. Ru. *krěpok*, SCr. *krěpak*, Sln. *krěpek* < **krěþьkb*. This formation is formally comparable to adjectives such as OCS *lgbьkb* ‘light’, *tnьkb* ‘thin’ and *ozьkb* ‘narrow’, which are all old Proto-Indo-European *u*-stems, cf. Skt. *laghú-* ‘light’, *tanú(ka)-* ‘thin’, *amhú-* ‘narrow’. Consequently, there is a distinct possibility that **krěþь* originally was a *u*-stem as well. It can therefore be surmised that OHG **chrāpfī*, the

⁶Schweizerisches Idiotikon, s.v. *chrāpfē*.

⁷Cf. Visp. *weexer* ~ *weher* ‘better’ (= Cimbrian *begor* ‘id.’) < **wēhizan-*, comp. of OHG *wāhi* ‘fine, beautiful’.

⁸Like Standard High German, the Visperterminen dialect obviously preserves WGm. geminates after long vowels, e.g. *ant-slaafu* ‘to fall asleep’ < PGm. **slēpan-* vs. *ant-sleepfu* ‘to sedate’ < PGm. **slēþjan-*.

precursor of Visp. *xreepfe*, replaced older **krēpu-*. In fact, such a scenario dovetails with the general replacement of *u*-stems by *ja*-stems in continental West Germanic, cf. OHG *engi* ‘narrow’ < **angja-* vs. Go. *aggwus*, OHG *herti* ‘hard’ < **hardja-* vs. Go. *hardus* etc.

A possible objection against the hypothesis that Slavic **krěpъ* was borrowed from Germanic would be its accentuation. The word has been reconstructed with accent paradigm a (Derksen 2008: 246),⁹ which is characterized by columnar stress on the first syllable. It is relatively frequent in loanwords from Germanic, e.g. PSl. **bukъ* ‘beech’ < **bōk-*, **plugъ* ‘plow’ < **plōga-*, **xlěbъ* ‘bread’ < **hlaiba-*, which must be a reflection of the word-initial stress in Germanic (Meillet 1909: 69). The accentuation of SCr. *krījep* and perhaps also Ru. *krépiti*, on the other hand, rather points to the mobile accent paradigm c (Dybo 1968: 155-158), which is not common in Germanic loanwords. Since, however, instances such as PSl. **klějъ* ‘resin, glue’ < PGm. **klaija-*, **lstsъ* ‘deceit’ < **listi-* and **tjudjъ* ‘foreign’ < **peudja-* in spite of their Germanic origins have the same accentuation, this objection does not seem to be decisive. Moreover, in the case that **krěpъ* did have accent paradigm c, it may well be that this is due to analogy with other mobile *u*-stems such as **tъnъkъ* ‘thin’ (Derksen 2008: 505).

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⁹Derksen reconstructs the root as PIE **kreh₁p-*, because accent paradigm a is associated with laryngeal roots and roots ending in **d*, **ĝ* and **g^w*. The laryngeal was already suspected earlier by Kortlandt (1975: 61).

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***R̥gveda* 7.95.2 and Karen Thomson**

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1. In her paper ‘A still undeciphered text’ (2009) Karen Thomson (KT hereafter) deals with several rigvedic issues, continuing with her idea that the *RV* needs a new approach. I agree with much of what she writes but find some errors of methodology and of fact. She sent me a copy of her paper in 2009 (together with the three Comments). I made some notes then but had to put the matter aside due to pressure from many sides. I am surprised the *JIES* referees did not spot the errors. Just as surprisingly, the three critical Comments on her paper also did not spot them. I shall confine my observations to these errors – without intending to demean the rest of her good work.

Before I deal with the passage 7.95.2 and *samudrá*, a few words on *árma(-ká)*, *vailastha-* and ruins (in 1.133.3), a subject that immediately precedes KT’s treatment of 7.95.2. I agree with her dismissal of Witzel’s (and others’) view that *armaká* means ‘ruin’. How scholars (e.g. Witzel 1995: 3-4; Rao 1991:32; Burrow 1963 *passim*) came to this strange conclusion is not difficult to understand since it appears in that sense in some post rigvedic texts and as final in compound names of old villages like *guptárma* ‘hidden, preserved’; also, initially, under the Invasion Theory, scholars thought the fiends and goblins mentioned in this stanza were the native enemies whom Indra had to destroy. However, hymn *RV* 1.133 has nothing that remotely suggests ruins. Moreover, stanza 6f states explicitly that invincible Indra ‘does-not-kill-men’ *ápūruṣaghna-* ! Certainly, there is a ghostly scene of frightful desolation with unfriendly she-fiends, goblins and demons (*yātumāti*, *piśāci* and *rākṣas*) but not a single mention of bricks, the chief building material of Harappan constructions, stone-slabs, fallen walls, beams or rafters and the like. In sharp contrast, the Old English poem *The Ruin* contains abundant persuasive details of the ancient remains (from Roman times?) so that some scholars think it refers to the town of Bath (Mitchell and

Robinson 1996:252-5).

KT rightly cites Mayrhofer (EWA, 2, p120) who gives for *árma-* the meaning *wohl Brunnen* ‘perhaps *spring*’ and connects the word with Tocharian *álme* ‘spring’ and names of European rivers like ‘Almus’ etc. This certainly seems to be so. The element of flow and moisture appears in the eye-disease called *arma* (in *Suśruta*). The word occurring in stanza 3 is very probably a (*guṇa*) development from \sqrt{r} (>*ár-ti*, *íy-ar-ti*, *r-ṇó-ti*, *r-cchá-ti*: 2nd, 3rd 5th and 6th class) with the sense ‘move’ (in the *Dhātupāṭha*, the native list of roots, *r=gatau* ‘movement’). Similar formations are *kr* > *kár-man*, or *dhr* > *dhár-man* etc. The word would therefore entail movement in its denotation – like ‘spring’, ‘flow’, ‘up or down’ and the like, not a static state of ruins. And, of course, in the *RV*, *armaká* is *hapax legomenon*; but consider similar formations *anta-ka* ‘ending’, *karma-ka* ‘action’, *reca-ka* ‘out-breath’, etc. I will come back to this.

In the same stanza 3, we find *vailastha* - and cognates which are also *hapax legomena*: *vailasthāna(-ká)* and *mahā-vailastha*. These are left as “uncertain” by KT (p 28), following Mayrhofer. But surely this need not remain so! The stem *vaila-* is clearly a (*vṛddhi*) development from a primary stem *vila-* (> *vet-* then > *vail-*) and a possible dhātu *vil*. Now, the native *Dhātupāṭha* has *vil* twice and for both the meaning *bhedane* ‘breaking, cleaving’. It is surely paradoxical that we have primary *bila* and secondary *vaila-* but not primary **vila* and secondary **baila-*. This suggests that the two stems are connected. Indeed, the Dictionaries have *vil* or *vila* but direct you to *bil* or *bila* because, as we know, the consonants *b* and *v* are often interchangeable (e.g. *varh* and *barh*, *vala* and *bala* etc). *bil* is not in the *Dhātupāṭha*, but is in the Monier-Williams Dictionary with the sense ‘breaking, cleaving’. The word *bila* means ‘cave, cleft, hole, opening, pit’ (*RV* 1.11.5 and 1.32.11 etc). So *vaila-* has to do with a cave or pit. Surely now, since we meet demons, fiends and sorcerers in this ghostly scene we can connect it with the great pit or chasm (*vavrá* in 7.104.3 and *pársāna* in 7.104.5) wherein are cast and destroyed evil-doers and fiends, as 1.133.5 says *sárvaṃ rákṣo ní barhaya* ‘[O Indra,] hurl down/in every demon’. So stanza 3 of hymn 1.133 prays to Indra (*maghavan*) to dash down (*áva-jahi*) this band of she-fiends/sorceresses in the ‘downrush/sweep/vortex (?=*armaké*) that-is-within-the-pit (*vailasthānaké*: in the place/room of the pit), in the downrush (etc) within-the

great-pit (*mahāvailasthe*)¹. The scene now is the pit of hell wherein are cast and dissolved fiends, sorcerers, witches and other evil-doers. (I am happy to consider any other reasonable suggestion – but not ‘ruins!’)

If this delusion about ruins falls in ruination, then the *RV* is seen not to know either Harappan towns or their collapsed remains. Is it not then legitimate to assume that it is pre-Harappan? I should think so – especially if this aspect is taken in conjunction with others. Our investigation leads us to this issue unavoidably.

2. While KT consulted Mayrhofer for *árma(-ká)*, she did not do so for *samudrá*. This is strange, surely. Had she done so she would have found that he gives for *samudrá* ‘confluence (*Vereinigung*) of two or more rivers’ and also (and for the *RV*) *Flut* ‘flood’ and *Meer* ‘sea’. She prefers her own ‘together-water’ which is etymologically correct but is not very helpful since it can denote anything from water in a cup, a puddle, a lake, to a cataract, rain, river, ocean. Here again she is right in exposing Witzel’s mistranslation and misrepresentation of “basic literary facts” regarding *samudrá* (Witzel 2001 §25, fn 204).

However, she abandons the awkward ‘together-water’ and translates 7.95.2 about the river Sarasvatī *súcir yatī girībhya ā samudrāt* as ‘pure, travelling down from the mountains, from the gathering-place of the waters’. And immediately one wants to ask how she knows that there was a “gathering-place of waters” up in the Himalayas. Why so?...A long line of vedicists, both Indians and Westerners, have invariably translated ‘pure, flowing from the mountains (*girībhyaḥ* ablative plural) to (*ā*) the *samudrá* (abl singular: confluence, ocean, sea and Witzel’s ‘terminal lake’). I am certain she knows that the ablative does not require the *ā* either as preposition or postposition to express “movement from” as Pāṇini makes it abundantly clear in the fourth chapter of his Book 1 of *Aṣṭādhyāyī*: e.g. *RV* 4.51.8c – *ṛtāsya devīḥ sádaso budhānāḥ* ‘the goddesses [are] waking **from-the-seat** (*sádaso* < *sádasah* ablative) of-Natural-Order (*ṛtāsya*)’.

KT opts to differ because she thinks that the two ablatives are parallel and the preposition *ā* (which she calls postposition and adposition) governs the first one, i.e. *girībhyaḥ* with the

¹Here I acknowledge my debt to Sethna’s comments in his 1992 publication, pp130-135.

sense ‘from’, and by extension the second one also with the same sense. She adduces the views of some comparativists in the Indo-European field (instead of Paṇini) and avers: “Indeed, some linguists have argued that adpositions were *invariably* placed after the word they govern” (p 32; my emphasis). Now why does she do this when she knows perfectly well that such a view is utterly untrue? Why refer at all to Indoeuropeanists and not to an acknowledged authority on Vedic?... All we need here is not pointless pedantry but a quick look at MacDonell’s *Vedic Grammar* which gives us the bare facts². Writing of prepositions *āti*, *ādhi*, *ānu*, *ā*, *ūpa* etc, this indisputable authority says: “As a rule these prepositions follow, **but also often precede** their case” (1916: 208; §176, 1: my emphasis) Then he adds: “*ā* with the abl., if following, means *from (on)*; **if preceding, up to**” (p209, §176, 2b: my emphasis). But he points out also in a footnote here that *ā* *sometimes* precedes with the sense ‘from’ (cf my example c, below). Indeed, the *RV* corroborates this with many such instances. Obviously one must use one’s reason and discrimination in every situation.

KT does give a similar example where the *ā* seems to follow its case and then govern with the same meaning a subsequent noun. This is it, with four more nouns in the ablative which are omitted for brevity’s sake:

ā yatu índro divá ā prithivyā, maksú samudrāt...
 ‘Come hither Indra from the sky or from the earth,
 Swiftly from the samudrá...’ (4.21.3)

Yes, but one could argue with much reason that the first *ā* goes with the verb *yatu* as is very common, the ablative *diváh* without the *ā* denotes the place from which emerges movement (and so do *samudrāt* and the subsequent ablatives) while the second *ā* governs *prithivyāḥ*: the meaning would now be ‘Let Indra come from the sky to earth [where I am], swiftly from the *samudrá...*’ (and so on with the other ablatives). This seems to me far more reasonable.

Here are some examples with the preposition preceding

²She refers to MacDonell’s *A Vedic Reader for Students* (1917 OUP), which cannot be understood without following up the teeming references to his own *Grammar*! She could also have consulted Wackernagel’s *Grammatik* which again she cites elsewhere! Thus she uses her sources and authorities selectively, as we shall see below, to suit her own notions.

its case:

- a) 1.30.2 : *vayám hí te ámanmahí á ántād á parākāt* ‘we thought of you both nearby and at a distance’.
- b) 1.151.5 with similar construction : *á nimrúca uśasaḥ* ‘until evening and until dawn’ (or ‘at evening...’ etc).
- c) 7.6.7, which is early : Agni Vaiśvānara received treasures *á samudrád ávarād á párasmād...* ‘from the lower samudrá and the upper [one].’
- d) 3.53.11, also early : *svastí á gr̥hébhya ... á vimócanāt* ‘wellbeing up to the houses ... until release/unyoking’.

In all these the *á* precedes and in every situation one has to use one’s common sense and textual content. And with the aid of Lubotsky’s *Concordance* one could cite dozens of other examples with *adhi*, *úpa*, *pári* etc. Take some examples: ‘[The Maruts] like birds sat upon [their] beloved barhis-grass’ *váyo na sídann ádhi barhíṣi priyé* (1.85.7d); ‘Eloquent you repeat upon the waters’ *suváco vādathana-ádhy apśú* (7.103.5d). ‘The eagle shook out from the mountain/rock the other’ *ámathnād anyám pári śyenó ádreḥ* (1.93.6) These examples should suffice. If the preposition invariably followed the inflected word it would be very difficult to have such compounds as *ádhy-akṣa* ‘eye-witness’, *anu-kāmá-m* ‘according to desire’, *pári-vatsará* ‘complete year’ etc, etc, etc.

To (wrongly) postulate invariable syntactic patterns (of this sort) for the *RV* is to show ignorance of Vedic and of poetry generally. (Consider the line from Eliot’s *Four Quartets* “In my end is my beginning” or the abrupt, startling reversal in John Donne’s “Thy beams so reverend and strong/Why shouldst thou think [O Sun]?” from *The Sun Rising*.)

Since the position of *á* in 7.95.2 is not anomalous, it is not surprising that generations of scholars have translated monotonously ‘from the mountains to the *samudrá*’. Moreover, KT’s rendering ‘gathering-place of waters’ has a serious difficulty with reality and common sense. There is no ‘gathering-place’ of ‘together-water’ on the Himalayan slopes. There are masses of snow, ice and glaciers and as soon as these melt they flow down as rivers. And the Vedics surely knew of the conditions on the Himalayas as they knew of the vast ocean far down south.

3. Many scholars doubt that the Vedics knew the ocean.

If KT had consulted the *Vedic Index* she would have seen that all the difficulties raised by modern indologists (e.g. Elizarenkova 1996-7; Klaus 1985) were dealt with adequately therein – except Witzel’s ‘terminal lake’ which is really a non-starter. The conclusion was that the Vedics did know the ocean. Using common sense again, also in the Invasion scenario, we can see that even if the Indoaryans had entered Saptasindhu, the land of the seven rivers, in N-W India from a landlocked region, they would have found out about the ocean from the natives, who engaged in trans-oceanic commerce with Mesopotamia. Since, always in the AIT scenario, they had so intrepidly trekked thousands of miles, they would have travelled down south either on boats on the Sindhu or with carts; there they would have seen the ocean. Surely, there is nothing extraordinary in this. So why all this fuss and denial of the Vedics’ knowledge of the ocean?... Reasoning must always be paramount.³

4. Could *samudrá* denote the ocean? Of course, frequently.

To speak of ‘together-water’ or ‘gathering-place of waters’ is unhelpful to say the least and, again, ignores common sense. A gathering place of waters in mass is a confluence or a river or a lake or the ocean. Now we know that there were several confluences, rivers and small lakes in Saptasindhu, but *samudrá* is usually in the singular, as all my citations herein show. Thus even if we accept “the gathering-place of waters”, we would expect a plural for the Himalayas, the valleys and the plains simply because there were many gathering-places of waters. And when Agni received treasures from the lower and the upper *samudrá* (*ā samudrād āvarād ā párasmād...*), we understand only one lower on earth and one upper in the sky. If it is one terrestrial *samudrá* it can only be the ocean since, otherwise, there are plenty of confluences, rivers, lakes and general gathering-places.

³I don’t really understand scholars who invoke scientism in the humanities (“scientific approach, method” etc). In Physics, Chemistry etc, scientists check their results against the realities of the material world, often with maths and always with reasoning. We can’t ignore the realities in our field (i.e. facts archaeological, grammatical and literary) and, most important, common sense.

However, I offer yet another passage – 7.49.2 :

*yā ápo divyā utá vā srāvanti, khañítrimā utá á vāyāḥ
svayamjāḥ; samudrā rthā yā ḥ śúcayaḥ pāvakā [h]* etc:
'The Waters that are heavenly, or flow in channels, or
arise spontaneously, [and] are clean and purifying, have
as their goal the *samudrā* etc.

The heavenly waters are the river or watermass in the sky and, of course, the rains; the waters flowing in channels are natural rivers or man-made ditches; these that arise spontaneously are springs, lakes and wells. Ditches, lakes and wells do not in ordinary terms aim for a larger gathering place: these we can ignore. Obviously the one gathering place of waters which rains, rivers and springs have as their ultimate aim is the ocean. Confluences themselves move on as larger rivers to the ocean (or a large lake); lakes have themselves arisen spontaneously (if not created and fed by rivers) and there are many of them. So the only watermass (in the singular) left is the ocean.

Let us take a final example. 1.116.4 says that the Aśvins saved Bhujyu from drowning and carried him for three nights and three days (*tisráḥ kṣápas trír áha-*) to the 'distant dry-shore of the watery ocean' (*samudrásya dhánvan ádrasya pāre*). Yes, we have a hyperbole which is common in the *RV*. But the poet has a specific intention here. Now, what "together-water" or "gathering-place of waters" is so large that the two gods, who fly on a car drawn usually by birds, would need three days and nights to traverse to reach its distant shore?...⁴ Only the ocean.

5 KT refers also (p33) to G. Possehl's 1998 paper and cites the passage: "it seems unlikely that the ancient Sarasvati flowed to the sea during those times. The absence of a river scar suggests that the same is true for later periods." (1998: 350) This is absolutely true. However, for reasons known best to herself, she does not divulge that "those times" are the centuries 1500-1000 BCE! She also does not tell us that with the very next sentence Possehl suggests "that the river once did flow to the sea, in very ancient times... (3800-3200 BC), but even this is not certain". She refrained perhaps because she wished to spare us Possehl's uncertainty. Nevertheless, the

⁴I discuss this issue very fully and give ten more examples in my 2009 publication, ch 5.

archaeologist thinks the river might have flowed to the sea but KT turns the archaeologist's hesitancy into certainty that the river did not reach the sea! A more thorough investigation would have revealed to her that Bridget Allchin, a most reputable British archaeologist, expert on Indian protohistory and the Indus Valley Culture, expressed no doubt about the river reaching the sea taking the Nara Nadi river beyond the Derawar Fort as the natural continuation of the ancient Sarasvatī (1999). In the same year L. Flam, another expert published independently an identical certainty about Nara being the continuation of Sarasvatī (1999). French archaeologist P-H Francfort had reached the same conclusion with certainty back in 1992 giving dates 3800-3600 and before. The doyen of Indian archaeologists, B.B. Lal, also expresses no doubt in his 2002 publication that the ancient Sarasvati flowed into the ocean through the Nara. And more recently, a team of reputable Indian scientists traced by satellite the course of the river (Possehl's "river scar") from the mountains to the ocean flowing into the Rann of Kachch or Kutch. (Sharma et al 2006).⁵

So the Sarasvatī, this *nadītamā* 'best of rivers' did flow into the ocean before 3500 (to give an average date), i.e. before, first, its tributary Sutlej was captured by the Sindhu and, later, the Yamuna was captured by the Ganges. This is the picture presented by the *RV* even in the hymns of the tenth and latest Book. So, again, when were these hymns composed?

6. Closely connected with the Sarasvatī is the word *ándhas* which occurs in *RV* 7.96.2 and also engaged KT's attention (p24). The word is in the dual: *ubhé yát te mahinā śubhre ándhasī, adhikṣiyānti pūrāvaḥ* (her translation: 'Since through your might, O bright one, The Pūrus inhabit both *ándhasī*'). Dismissing Griffith's translation "grassy banks" for *ándhasī* and Geldner's *Getränken* "drinks" (echoed also by Renou in French and Elizarenkova in Russian), KT goes off to examine the controversy between indigenists and invasionists and refers to the Bryant and Laurie book, *The Indoaryan Controversy* (2005) but not to the debate in the *JIES* (2002-3), which is in fact later, i.e. more recent, since the papers in Bryant and Laurie

⁵Most of this information except for the 2006 publication was presented in my 'Final Reply' in *JIES* 31, Spring (pp 228-9) but very few scholars bother to read nowadays, widely, attentively and impartially.

were written before 2002! Thus she offers no translation of her own and so, as with the *vaila*-group and *armaká*, the text continues to remain undeciphered. Let us see if we can sort this out.

Of course *ándhas* is well attested in the sense of ‘darkness (and blindness)’ ; also ‘bush, grass, plant, stalk’ (esp 1.28.7) and ‘(soma-)juice’. Mayrhofer, as many another before him, links *ándhas* with Greek *ἄνθος* ‘flower’ and with Friesian *ändul* ‘fine, tender grass’. (This is acknowledged by one Comment and KT in her second paper.)

The ‘bright one’ *śubhrā* addressed in the verse is Sarasvati. So we must ask what two things a river has: it has a beginning and an end; a surface and a bottom, the riverbed; and two banks. Common sense and the diction with the dual concord *ubhé...ándhasī* compels us to dismiss all notions except the “two banks”. The two banks are the only places where the tribe of the Pūrus could dwell (*adhikṣiyānti*). Obviously the “two drinks” of Geldner, Renou and Elizarenkova, despite the massive notes that accompany this rendition, make no sense at all (not even metaphorically), since there are many, not two, drinks from the river-water and since the fluid denoted by *andhas* is the specific soma-juice.

True, *ándhas* as ‘bank’ is not attested anywhere. But why should this matter? Are we to abandon the reality of the material world and our common sense for the sake of philological pedantry? After all, *RV* 8.22.17-8 says that king Citra and lesser-kings *rājaká*- dwell along the Sarasvati (– along its banks, obviously). The enclitic *te* in 7.96.2 ‘thy’ covers both *mahinā* ‘greatness, might’ and *ándhas*.

Moreover, there is the figure of speech called ‘synecdoche’ which uses the part in order to indicate a larger whole, as in “I counted ten heads” instead of “ten persons” or “ten sheep”. Similarly here, we have “both bushes/grasses” to denote the “grassy banks” of the river. There is nothing extraordinary about it. Griffith, not for the first or only time, proves more faithful to the spirit of the poet.⁶ And let us

⁶Consider 6.75.8 *tātrā rātham úpa śagnám sadema*. Geldner translated “...auf den wollen wir der Wagon setzen” and Witzel gave it in English “on this (*rathavāhana*) we wish to put the useful/strong ratha”; O’Flaherty too gives (1981:237) “on it let us place the working chariot”. But the verb *úpa-sad-* never means ‘place, put’; it means ‘sit by/near/on, revere, approach respectfully’ and the like. Only the causative form *úpa-sād-aya-* does mean

acknowledge the simple fact, that much of the *RV* is not just poetry, but great poetry!

7. Several more points could be made about KT's article but I decided to keep this paper short. As I wrote earlier, I agree with most of what she says questioning various established but probably wrong meanings. One general point of caution. India has excellent archaeologists (Lal taught in California; Chakrabarti D. teaches at Cambridge, U.K.) and, of course, unrivalled sanskritists. Some Western indologists deride them with terms like "quacks, Hindutva hacks" and the like; almost all tend superciliously to ignore Indian scholars thinking that only Western indologists (mainly comparativists) transmit the truth. This is a sad mistake.

By way of conclusion I present seven items, apart from ruins, that are not found in the *RV* but are common in the Harappan culture and are found in post-rigvedic texts, especially the Brāhmaṇas and Sūtras.

- i) bricks – *iṣṭakā*;
- ii) cotton – *karṣāsa*;
- iii) fixed altars or hearths;
- iv) iconography – relief or statuary;
- v) urbanization on a significant scale;
- vi) writing – *lipi* or *lekha(na)*;
- vii) dried up Sarasvatī. (There are more.)

Moreover, there is no evidence whatsoever to suggest that the *RV* was composed c1200-1000 BCE - none other than fanciful theory and mechanical repetition. All the tremendous arsenal used once upon a time to support the Invasion scenario has now been reduced to horses and chariots: chariots for war and races did not exist before 2000 and horses appear in Saptasindhu only after c1500 – according to Prof. Witzel who has become the main spokesman for this theory.

The meagre evidence for domesticated horse at the Harappan sites often adduced in discussions, is a red herring. There is no significant increase of horse-remains after the period 1500 BCE. If there was an entry of Indoeuropeans

'make sit on, place upon'! Here again Griffith got it right - "let us here...honour the helpful Car". Of course, Griffith does make many mistakes but this is understandable when we consider that Vedic studies had only just begun.

bringing horses and chariots at c 1500 BCE, there should be masses of such remains. There is no such evidence until the centuries of the Common Era. KT, as others before, rightly points out (p36) that horses are not quite so common in the *RV*, as many scholars claim (see also Kazanas 2002: §VII,1 with many more references). She also shows that the much mistranslated and thus maligned *rātha* is not a “war-chariot”. In fact, in his translations in his *Vedic Reader*, MacDonell never gives the word ‘chariot’ but always ‘car’. The “chariot” is a legacy of classicism (Greece and Rome). Moreover, rigvedic cars are made from native timber (*RV* 3.53.19; 10.85.20). They have space or seating for three *trivandhurā* (*RV* 3.6.9; 6.47.9 etc) and one is a minibus *rātha* having space for eight *aṣṭāvandhurā* (late 10.53.7): they are drawn by oxen, donkeys, antelopes and rarely by horses! There is not a single mention of one- or two-spaced *rāthas*. Here again, actual remains of cars are found only after c 300 BCE. All this was discussed extensively in Kazanas 2002, §VII, 2-3. So the “war-chariot” is another red-herring.

I leave it to the readers to draw their own conclusion about the approximate date of the composition of (the bulk of) the *Ṛgveda*.

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The Plight of the *Rigveda* in the Twenty-First Century Response to N. A. Kazanas

Karen Thomson

In 2007, a collection of rare manuscripts of the text of the *Rigveda* was added to UNESCO's 'Memory of the World' register. This register is designed to honor and protect precious landmarks in the intellectual history of the world.

Two years before, in 2005, Penguin had issued a second edition of its selection of translations from the poems by an American Sanskritist and professor of religion. For the past thirty years this slim volume has represented the *Rigveda* for the English reader. At the end of the book the editor supplies fragments of the text, albeit in an unaccented form that came centuries later, in an index of first lines. According to this index (Doniger 2005: 321), the opening word of the first poem is the mystical particle *ōm*. This particle, conveying religious awe, has been silently added by Professor Doniger from later Indian religious writings. It is not present in the first line, and is entirely unknown to the Rigvedic poems.

These two very different ways of showing respect to the earliest Indo-European poetry pull in irreconcilably different directions.

Why the *Rigveda* is not being studied

For most western readers – particularly if they have ever dipped into the Penguin selection – these poems are an impenetrable enigma. Simon Jenkins, one of our finest journalists and a writer of considerable scholarship, recently coined an elegant metaphor to describe the incomprehensibility of the medieval English church: “a church is a song without words, an architectural Sanskrit” (Jenkins 2000: ix). For the educated modern reader, Sanskrit is the one subject that he knows he doesn't know, and that he is not in the slightest bit embarrassed not to know. Its earliest poems embody this acknowledged, acceptable ignorance. Two of the

The Journal of Indo-European Studies

three responses to my paper “A still undeciphered text” published last year in *JIES* were a reflection of this. Peter-Arnold Mumm is a professor of Indo-European linguistics, a discipline that owes its existence, and indeed its name, to the discovery of the relationship between Sanskrit and the classical languages of Europe. But he is not a Sanskrit scholar, and while undertaking to publish a reply made no apology for not having read my word studies, which contain the linguistic evidence for my argument. Stefan Zimmer, similarly, chair of Indo-European Studies at Bonn, points out that he is not in a position to judge “the breadth and completeness of my critical survey”. No stigma, clearly, attaches to professors of Indo-European for unfamiliarity with the language of the earliest poems.

The *Rigveda* is a minority interest in Sanskrit circles. “Some chocolates can only be sold if they are wrapped up in gold-speckled papers. Books about the Ṛgveda will only be read through the medium of some fashionable theory.” (Staal 1982: 278) The ‘hermeneutic’ approach of the Vedic scholars whose province these poems are considered to be was the focus of my paper last year. And as far as traditional scholars in India are concerned, there is no debate: these poems should simply not be an object of study. What the *Rigveda* ‘means’ is of no concern, it is the tradition deriving from them that matters. As an Indian correspondent politely told me: “Scholars of traditional stamp would prefer that the text remains inscrutable. Western scholarship burrowing into the text is, all said and done, an annoyance.” Meanwhile it continues to be considered irrelevant to the work of most classicists. Caught like a fly in amber in the east, in the west this ancient poetry, unknown until the nineteenth century, has suffered the fate of a child joining school a year after everyone else, too late to find a niche. It is hard to imagine, therefore, who is going to find my linguistic arguments to be within his remit.

Despite not having read the published evidence, Professors Mumm and Zimmer clearly regard as absurd my conclusion that these poems make as much sense as any other ancient literary composition. Professor Mumm is content to meet it with some derision: “Good luck for the project of explaining the whole RV literally!”. Professor Zimmer, similarly, while admitting his inability to assess my argument, is confident in disparaging it. Had I written “A still undeciphered

text” a decade ago, impelled simply by the overwhelming sense that this is great poetry, and that the incoherence must lie in the translations rather than the original, it would be reasonable for scholars to be skeptical. But over the past ten years I have published a number of word studies whose retranlations transform the poems, discovering sense where before there was nonsense.

These studies continue to be largely unread. The only scholars I can be relatively sure have read them are the anonymous referees who approved them for publication. Their response has always been: of course, yes, her argument about this particular word is correct, but she cannot draw the general conclusion that she does from just one study. But there is not just one study. What, of itself, would be no evidence becomes *by its corroborative position*, proof most sure. This sentence is not mine: it is taken from Edgar Allan Poe, describing, in 1843, the investigative method of Augustin Dupin in his short story *The Mystery of Marie Rogêt*. Dupin is identifying the body of a girl:

“Each successive proof is multiple evidence – proof not *added* to proof, but *multiplied* by hundreds or thousands... it is not that the corpse was found to have the garters of the missing girl, or found to have her shoes, or her bonnet, or the flowers of her bonnet, or her general size and appearance – it is that the corpse had each, and all *collectively*.” (Poe 2006: 59)

In terms of my overreaching argument – that the text responds to the scientific approach, and that it is possible to make sense of it – continuing to publish such proofs is not necessary. The point has been established.

The *Rigveda* as political football

The possibility of attracting others to the academic study of this ancient poetry is however not increasing, it has seriously diminished in recent times. The circumstances that surround it, and contribute to maintaining its neglect, have become much worse. A vitriolic political debate has arisen between a voluble and largely unchallenged western post-colonial coterie, and a beleaguered native party with the instinctive feeling that they are being bullied, but who are unsure how to defend themselves. Any trace of impartial

scholarship, particularly in India, is effectively held to ransom. Scholars are compelled, as my correspondent put it, “to remain silent unless they can support currently popular notions.” And not just in India. I, too, it seems, should have steered clear of such controversial matters as rivers flowing to the sea, and people living on river banks.

Nicholas Kazanas is a sincere man, driven by commitment to what he, like me, observes to be an important and highly poetic text, and a desire to come to the aid of a browbeaten underdog. But he helps his case not at all by insisting upon traditional translations that are built on sand. It is important to be clear about what these cornerstone translations for him and his party are:

1. Kazanas insists that 7.95.2 provides clear and crucial evidence that the river Sarasvati flowed to the sea, despite the fact that the word for sea in the line is ablative. Although he does not dispute that *samudrāt*, in all its other occurrences in the text, means *from*, not *to* the sea, he refuses to countenance any doubt being cast on this traditional translation;
2. He argues that *ándhas* in 7.96.2 must mean ‘grassy bank of a river’, although he agrees that it cannot possibly mean that in any other of its 100 or so occurrences.

Along the way he devotes at least half of his paper to red herrings, which cast considerable dust in the eye of the reader. I did not write that prepositions like *á* are invariably postpositional in Ancient Sanskrit, that was Delbrück. I said that they usually are: “adpositions’ in Ancient Sanskrit usually follow the word they govern” (Thomson 2009: 32). This is not in question: Kazanas quotes Macdonell saying the same. His impassioned digression on the poetic flexibility of prepositions has no bearing whatever on the argument: whether *á* belongs with following *samudrāt* or with preceding *giribhyas*, *samudrāt* remains ablative. And, as I wrote at the time, I quoted the title of Gregory Possehl’s 1998 article “Did the Sarasvati ever flow to the sea?” (2009: 30), because it provided a convenient heading for that section. I refer throughout to the implications of the lack of textual scholarship for scholars in other fields. The archaeological arguments however I leave to the archaeologists; mine relate only to the text of the *Rigveda*.

One of Kazanas's misunderstandings warrants fuller attention, because it leads to further evidence, if such evidence were needed, for the interpretation of 7.95.2, which is his primary concern. He is wrong to think that I avoid translating the word *samudrá* in this paper because I am of the camp that believes 'sea' is an inappropriate translation. By leaving it untranslated, I was hoping not to have my grammatical argument obscured by the political imbroglio. However, if pointing out that *samudrát* is ablative is insufficient to throw doubt on the traditional translation 'to the sea' in 7.95.2, there is a wealth of complementary evidence: rivers flow regularly to the *samudrá* in the poems, and, as any linguist might anticipate, their destination is in the accusative, *samudrám*. The context is usually Indra's paramount achievement in releasing earth's streams from the primordial dragon:

1.32.2a,d

áhann áhim párvate śiśriyāṇám...
He-slew the -dragon on-the-mountain lying

áñjah samudrám áva jagmur ápah
*áñjas*¹ to-the-samudra down went the-waters

Streams are similarly dispatched to an accusative *samudrá* in verses 3.36.6 and 6.17,12, and, with the preposition *áchā* 'towards' (which takes the accusative) at 1.130.5, 2.19.3, 3.33.2, and 6.30.4. The image is also used in similes: worship goes to the gods like streams to the *samudrá* in 1.71.7, 1.190.7, and 3.46, 4. The word *samudrá* is in the accusative in all these passages. Once, exceptionally, the context of the simile compels the dative, *samudráya*:

8.44.25:

ágne dhṛtávratāya te
Agni, whose-laws-are-firm, to-you

samudráyeva (samudráya-iva) síndhavaḥ
Like -to-the-sea rivers

gíro vāsrása irate
The-songs roaring go

¹The word *áñjas* is left untranslated for reasons that will be explained below.

None of this of course provides the kind of information that historians and archaeologists are looking for, in this case whether the poets did or didn't know the ocean. Only deciphering the text will be able to do this. The riches its decipherment will yield up to the important minority of classicists with an interest in comparative Indo-European poetics, notably Martin West at Oxford, are also unknown.

Without a proper scholarly edition and translation of the text, arguments such as those Nicholas Kazanas adduces continue to be bandied back and forth, their validity generally unverifiable, interested scholars groping for handholds in a quagmire. To give a brief example, Kazanas cites 7.49.2 as a passage that speaks of rivers and springs going to "one gathering place of waters". His point is the supposed singularity of the word *samudrá* in this passage: "the only watermass (in the singular) is the ocean". But *samudrá* is not in the singular, it is the first element of the compound *samudrártha* (*samudrá-artha* 'sea-purposed') and is in stem form. Whether you interpret it as singular, plural, or dual is up to you – and, in the current political context, the argument you are trying to urge.²

The 'German School'

At the beginning of "A still undeciphered text" I quoted the nineteenth-century linguist William Dwight Whitney's remark, in an essay on the interpretation of the *Rigveda*, that the content of the poems "seems almost more Indo-European than Indian" (1873: 101). Whitney is comparing the approach of the ancient commentators, who regularly ascribe a diversity of meanings to the same word or phrase, with the scientific methods of what he calls "the German school". He dismisses the work of the commentators so roundly that one would have thought it dismissed forever. On the other hand, he tells us

²He refers, similarly, in defending his 'river-banks' argument, to another passage about Sarasvati for its supposed historical evidence: "after all, RV 8.22.17-18 says that King Citra and lesser-kings dwell along the Sarasvati (– along its banks, obviously)". But this information is not in the text either, there is no verb in the lines he is citing. Not only are the river banks supplied ("obviously", according to Kazanas), but also the verb "dwell", and the justification for supplying it is questionable. Kazanas should have checked the text before drawing his parallel (and if he had done so he could also have corrected his reference; the passage in question is the last two verses of 8.21, not 8.22).

that the linguistic approach of the German school had by 1873 already given the world “an essential part of its knowledge and conception of ancient times” (1873: 132). Professor Whitney would have approved of UNESCO’s recent tribute to the *Rigveda*.

The approach of the German school to the text has changed since Whitney was writing. At the Fifth International Conference on Historical Lexicography and Lexicology (ICHLL5), held in Oxford in June 2010 to celebrate the tenth anniversary of the online *Oxford English Dictionary*, a presentation on a projected new *Rigveda-Lexikon*, under the general editorship of Thomas Krisch, was an unexpected contribution. “Rivelex: The structure of a dictionary to an ancient corpus (Rigveda): morphological, syntactic, and semantic information”.

One of the examples of a lexical entry given on their handout was the feminine form of the word *gáw*, for which the Rivelex team give the following meanings: “cow; milk, butter; earth, dawn, cloud, bodies of water, sacrificial spoon, poetry, voice, bow-string”. They add the note: “These meanings are derived from the context.” But this ‘derivation from context’ is of an entirely different order from the *OED*’s continuing focus on defining meaning through usage. These multiple ‘meanings’ do not represent any kind of scholarly consensus: no two translators, for example, are in agreement in understanding the word in any context to mean ‘sacrificial spoon’ (the suggestion comes from Geldner’s footnotes to 3.7.2 and 10.65.6; others, for these two contexts, offer ‘sacrificial cow’, ‘sacred sound’, ‘speech’, ‘la Vache’, and ‘thunder’). Such lists of meanings derive from the ancient reverential tradition of ‘glossing’ the text rather than attempting to decipher it. Sanskrit dictionaries are full of entries of this kind, which are hold-alls into which anything can be made to fit, like Harry Potter’s friend Hermione’s handbag. It is not the case that the Rigvedic poets could not distinguish a cow from a cloud or a spoon or a bow-string. There is an underlying problem with the traditional interpretation of this word that needs to be sorted out.

What John Chadwick wrote in the 1960s in response to a suggested decipherment of the Indus Script by Asko Parpola and his team also holds good for the *Rigveda*. “To preserve an open mind is incredibly difficult, because we are either

mesmerised into swallowing camels or so prejudiced we cannot manage the odd gnat... What we shall need is not more possible or even plausible interpretations... but the clearest possible demonstration that these meanings, and only these meanings, are correct” (Clauson and Chadwick 1969: 207).

The first example given on the Rivelex handout at the conference was the compound *añjas-pā*, and comes from the part of their dictionary that has already been published (Krisch 2006). The Rivelex editors translate *añjas-pā*, “drinking an ointment; protecting an ointment; moving directly etc.”. Setting aside the solecism of giving three quite different meanings for a word that occurs only twice (at 10.92.2 and 10.94.13), continuing to put forward such bizarre interpretations stands firmly in the way of making sense of the text. The first element of this compound, *añjas*, contrary to what Rivelex tells us and its translation assumes, never means ‘ointment’ in the *Rigveda* (see its occurrence in 1.32.2 quoted above, where it is usually taken as adverbial).

añjas is a nominal derivative of the verb $\sqrt{añj}$, which was included in my list of undeciphered words in “A still undeciphered text” (2009: 80). The verb continues to be misunderstood as meaning ‘smear with ointment, anoint’, in the *Rigveda*, although there are numerous occasions where this makes no sense. At the end of Lesson 10 of *Ancient Sanskrit Online* its Rigvedic usage was briefly discussed and illustrated (Thomson and Slocum 2006). The very different meaning that emerges for this important verb – not ‘smear with ointment’, but something like ‘cause to appear, make manifest’ – exemplifies how ritual interpretations have for thousands of years obscured the *Rigveda*’s conceptual sophistication, and how they continue to obscure it in the twenty-first century.

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JIES Reviews

Archaeology

Christopher I. Beckwith, *Empires of the Silk Road: A History of Central Eurasia from the Bronze Age to the Present*. Princeton/Oxford: Princeton University Press, 2009. ISBN 978-0-691-13589-2. 472 + xxv pages. \$35.

In his Introduction, Beckwith tells us that this book is “about the continent-wide struggle between Central Eurasians and the peripheral peoples...[and that the] [r]ecognition of the struggles of the Central Eurasian peoples against the more than two-millennia-long mistreatment by their peripheral neighbors is long overdue. The warriors of Central Eurasia were not *barbarians*. They were heroes, and the epics of their peoples sing their undying fame” (xxiv-xxv). In the book that follows he sets out to prove that the modern world cultures do not derive from the valleys of the Tigris, Euphrates, Indus, or Yellow rivers but from “the challenging marginal lands of Central Eurasia” (319). He begins with the Bronze Age and takes us up to modern times. Because of the nature of this journal, this review will focus on the earlier Indo-European aspects of the book, leaving the later chapters of the book to reviewers with greater interest in more modern times.

In the Prologue, “The Hero and His Friends,” Beckwith provides origin stories which might be thought of as myths. These stories have common cultural elements shared by the peoples of Central Eurasia that go back to the Proto-Indo-Europeans and which he calls the Central Eurasian Culture Complex. He then turns to the “Comitatus” whose members not only support and defend “the sociopolitical-religious ideal of the heroic lord” in life but also swear to follow him in death. Beckwith concludes his Prologue with what seems to be obvious: “the Silk Road was not an isolated, intrusive element in Central Eurasian culture, it was a fundamental, constituent element of the economy” (28; see also Kuzmina 2008). But he then adds “Its origins, and the formation of the Central Eurasian Culture Complex, go back to the Indo-European migrations four millennia ago” (28) and that these people, the Proto-Indo-Europeans, “are known only from historical linguistics.” His view of what is Central Eurasian is a somewhat modified version of that held by Johanna Nichols (1997) and

according to his map encompasses the entire Eurasian landmass.

Beckwith dates the beginning of the PIE migration out of their homeland (“mixed steppe-forest zone between the southern Ural Mountains, the North Caucasus, and the Black Sea”) at about 4,000 years ago. Most Indo-Europeanists would consider this about 2,000 years too late considering that we have Hittite evidence from Assyria at about 2100 BC (4,100 years ago) in the form of identifiable Hittite names. A more accepted date for the breakup of PIE is ca. 4000-4500 BC (6,000-6,500 year ago). He has three migration “waves,” the first occurring at the very end of the 3rd millennium, the second, which he considers the most important, around the 17th century BC, and the third in the late 2nd or early 1st millennium BC (29-30). His three waves produced three groups of languages: A—Hittite and Tocharian; B—Indic, Greek, Italic, Germanic, and Armenian; and C—Celtic, Albanian, Slavic, Baltic, and Iranian. At no time does he mention the archaeological work of Marija Gimbutas and her three waves.

He claims there is no “linguistically acceptable reason” to date the breakup of PIE any earlier and does not mention the enormous problem of explaining the deep divergence among Mycenaean Greek, Hittite, Luvian, and the Indo-Aryan language of the Mitanni at 1500 BC, a mere 500 years after he would have PIE breakup—an amount of time less than from Chaucer’s death to now. Perhaps, more surprisingly, he claims that the “traditional view” that the dialects of PIE are not in fact real languages at all but creoles and that “it is uncertain if Avestan really is an Iranian language to begin with” (Appendix A, p. 367, fn.12). For his claim of creolization, he depends heavily on the work of Andrew Garrett (1999; 2006) and his own earlier work. Later in the book he seems to say that IE “produced a creole not only with the pre-Chinese...but also with at least some of the pre-Tibeto-Burmans” (48).

Endnote 40, pp. 399-400 says the source of the Chinese language is “undoubtedly a result, at least in part, of the Indo-European intrusion into the area [the area covered by the Shang Dynasty]...[but] it is still uncertain whether Chinese is ultimately a minimally maintained Indo-European language or a local language influenced by Indo-European.” He calls this a “largely neglected problem” and references three of his own articles. This is quite a surprising statement, and I can think of

no reputable Indo-European linguist or Sinologist who would call Chinese a product of IE, but most would agree with Pulleyblank that there are loan words. To bolster his argument, Beckwith submits the Anyang chariot burials of ca. 1200 BC, which do have similarities to western chariot burials, but he believes it was not enough just to capture the chariots and horses. He believes that carpenters and trainers were at least initially necessary. Although they may have been helpful in the beginning, I am not convinced they were necessary. By 1200 BC, Chinese would have been a well established language and in vocabulary may well have been “influenced” by IE speakers, but it seems unlikely to have changed its basic structure.

Beckwith identifies the Tarim Basin mummies, based on “historical and linguistic evidence...as Proto-Tokarians” (35-36), unlike Mallory and Mair (2000) who take a more cautious view.

The chapter on “The Chariot Warriors” has a number of problems, but I will concentrate on one. He claims the earliest “archaeologically discovered chariot remains” were found at the site of Sintashta but fails to mention that the same type of vehicle has been found at several other sites that are related to the Sintashta-Petrovka culture; Kuzmina (2001:12) lists 20 vehicles from at least nine cemeteries. In his endnote 49 (pp. 403-404), Beckwith rejects the view that these vehicles were either prestige or ritual objects but adds that “[t]his problem should, however, be addressed by archaeologists.” It has been addressed on several occasions which he appears not to have read (see Anthony and Vinogradov 1995; Littauer and Crowell 1996; Jones-Bley 2000; Kuzmina 2001; Vinogradov 2003).

This omission brings up another problem with the book that appears several times. Despite the many footnotes and endnotes, Beckwith has not cited some of the most basic archaeological literature. Just as in the case of chariots, there is no mention of either V.V. Gening (the excavator of Sintashta) (1977), E. E. Kuzmina (2001), who has dedicated decades to the study of the Indo-Iranians, or even David Anthony whose views on war chariots closely match those adopted by Beckwith. In the Acknowledgements (xvi, n.1) he regrets the fact that Anthony’s book (2009) was not available during the writing of his book, but there is no mention of Anthony’s 1995 article in *Antiquity* which is essentially a

preview of his book. Just as shocking is the absence of any reference to the work of Marija Gimbutas whose many publications are fundamental to the discipline of Indo-European archaeology (see Gimbutas 1996).

In the Epilogue, entitled “The Barbarians,” Beckwith defends the actions of the “barbarian” peoples against the more “civilized” nations and the traditional views held by modern scholars. He rightly stresses that “*All nomadic pastoralist-dominated states that we know anything about, from the Scythians to the Junghars, were complex.*” [Beckwith’s emphasis]. While I applaud him for taking this rarely held stance, the obvious point he fails to make is that every nation or even tribe works in their own self-interest, which is often itself complex. The use of violence in gaining that self-interest is only one aspect of a people be they the Scythians of ancient times or a modern 21st century nation.

Despite an extensive bibliography, many of Beckwith’s references are quite general. For example he has numerous references to the Cambridge History series and leans heavily on Mallory and Adams (1997) for most things Indo-European. Beckwith does not place much emphasis on archaeology and thus his arguments are mainly historical or linguistic. His main archaeological references are to Robert Drews (1988, 1993, and 2004) and Renata Rolle’s *The World of the Scythians* (1989). Drews’ work is somewhat controversial, but Rolle is excellent. Nevertheless, there are much more recent works on the Scythians written by Russian scholars who still work in the area and which provide more detailed studies of individual problems, e.g. Davis-Kimball, Bashilov, and Yablonsky (1995).

The most cumbersome problem with this book is that if the reader is not to lose his or her place or train of thought, several bookmarks are needed to read this book! The text has footnotes on virtually every page that often refer to the Prologue, Epilogue, the two appendices (which themselves may also have footnotes) and/or to the 111 endnotes (41 pages) which can be as long as an entire page. Beckwith is clearly aware of the note problem and admits that he “*like[s]*” [his emphasis] notes that go into detail on interesting topics.” I confess that I, too, like notes, but here they are excessive and can be disconcerting. Many of these notes could have been included in the text or left out completely.

The book has no illustrations and only two maps—on the

inside front and back covers. While these are useful in seeing ancient and modern Eurasia, small more detailed maps would have been helpful when he discusses the many groups in the book. It is also unfortunate that the center of Central Eurasia is found in the crease of the binding.

It is difficult to determine who Beckwith's readership is meant to be. Although it is called "a history," and I believe that it is ultimately meant as a history, it takes in so much territory and dips into so many disciplines, that it is anything but straightforward. There is, however, an enormous amount of information in this book, but at times one needs to shake one's head due to the format of moving from one group to the other (this is the easy part) and because of the of the number of footnotes and endnotes that refer to other notes and appendices.

Despite these complaints, Beckwith provides a good idea of the complexity of the history of Central Eurasia—a complexity that is essential to know if one even attempts to understand the current events of the area.

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Postscript

There were several of Beckwith's Indo-European linguistic points with which I felt needed clarification by a linguist. I have asked Martin Huld to comment and as a postscript to this review, he offers up the following notes:

1. Beckwith is fundamentally mistaken about the nature of the Indo-European stops. While Gamkrelidze and Ivanov do indeed argue that [d] and [d^h] are allophones of a single phoneme (1995: 16), that phoneme is the one Grassmann and other recognized as *d^h; Grassmann's *d would be the equivalent of Gamkrelidze and Ivanov's +l', so it is never an allophone of *d^h. Gamkrelidze and Ivanov's claim that [t] and [t^h] are allophones of their +t (traditional *t) and [d] and [d^h] are allophones of their +d (traditional *d^h) was not intended as and cannot be misrepresented as a claim that traditional *d and *d^h (their +l' and +d) did not contrast. In both the glottalic and traditional reconstructions all three entities are distinctive and contrastive and constitute separate phonemes in the phonemic system as the following minimal triplets show.

<i>ter-</i> 'speak' LIV 630	<i>uet-</i> 'entrust' LIV 694
<i>der-</i> 'tear, rip' LIV 119	<i>ued-</i> 'be or make wet' LIV 658
<i>d^her-</i> 'fasten, fix' LIV 145	<i>ued^h-</i> 'lead' LIV 659

Because these roots have sonorant codae and onsets, the constraints noted by Meillet's Stricture do not operate, and they demonstrate that all three sounds are contrastive at the surface level in Italic, Germanic, Greek, Armenian, and Indic. This discontinuous geographical and temporal distribution indicates that the contrast was also present in the ancestral language and not a later innovation. These and similar phonemic contrasts were worked out in the nineteenth century, but they were also reworked by trained Indo-Europeanists such as Leonard Bloomfield and Roman Jakobson, who, in fact, were also among the leaders in

promulgating the structuralist view of the phoneme. To imagine that the founders of the structural phonemic principle did not understand how to apply their own principles to Proto-Indo-European sounds is little short of arrogance. Indeed, Jakobson's point in his 1957 address was not that PIE **d* and **d^h* could not contrast without a [t^h] (that problem could have easily been addressed by simply positing that PIE **t* was in fact phonetically [t^h]), his point was that because PIE **d* and **d^h* contrasted, if that contrast were actually one of aspiration, there had to be a comparable phonemic contrast between a PIE [t] and [t^h], which cannot be demonstrated.

The Indo-European situation is not unlike that of Middle Chinese; the vast majority of modern Chinese languages (Mandarin and Cantonese or Guāngzhōu) distinguish only a voiceless stop [t] (written *d* in the Pinyin orthography) from a voiceless aspirate [t^h] (written *t* in Pinyin). Xiāmèn (Min) makes no distinctions, realizing all as [t]. But Sūzhōu (Wú) has a three-way contrast of [t], [t^h], and [d] which reflects the Middle Chinese contrast of initials identified as *duān*, *tòu*, and *dìng* in the rhyme tables. It is worth noting that Karlgren reconstructed these contrasts as a lop-sided **d*, **th*, and **dh*. Yuen Ren Chao, Pulleyblank, and Baxter have instead proposed MC **t*, **th*, and **d*, a common pattern shared by classical Greek and Yerevan Armenian as well as most Wú dialects.

In his 1986 article, Huld pointed out that neuter nouns like PIE **kērd* 'heart' and pronouns **k^wod* 'what' end in voiced stops, never in voiced aspirates and that the voiceless final stop of the third singular secondary endings, **-t* and the voiced stop of the thematic ablative singular **-ōd* are neutralized as an underlying [d] in both Italic and Indic. He took this evidence to indicate that the voiced stops were the unmarked members of the correlation bundle and that **t* was marked by a feature of voicelessness and that **d^h* was marked by some other feature, obviously not voice. From this observation, Huld was able to explain Meillet's constraint patterns in biobstruent roots. *A biobstruent root must contain one and only one marked feature.* Four of the permitted roots, **TeD*, **DeT*, **D_heD*, and **DeD_h*, have a featureless "voiced stop" and only one feature in either a "voiceless" or "aspirated" stop; two of the permitted roots, **TeT* and **D_heD_h*, have only one feature, either the "voiceless" or "aspirated" stop, redundantly realized. Of the

forbidden patterns, *TeD_h and *D_heT have two different features of marking and *DeD lacks any feature of marking. Huld thus concluded that the traditional reconstruction is phonemically correct, that is, that it distinguished the proper contrasts among the members and explained features of neutralization and constraint in a simple, straightforward manner although he conceded that the traditional reconstruction was probably phonetically imprecise in that it failed to correctly identify the precise nature of the marked features of the Indo-European stop phonemes.

2. Beckwith's claim that Avestan was regarded as an Indic language is simply false. I know of no early specialist who makes such a claim. Perhaps he is confused by the ambiguous use of Aryan which is sometimes used as a synonym for Indic or Indo-Aryan, sometimes for Indo-Iranian, and most *incorrectly* for Indo-European. When earlier investigators included Avestan with Aryan, they meant that it was part of Indo-Iranian, which is a classification that is accepted today. If Jackson could state that "[t]he language of the *Avesta* is most closely allied to Sanskrit" (1892:xxxix-xxxii), he also notes correctly that "[t]he language in which the *Avesta* is written belongs to the Iranian branch of the Indo-Germanic tongues" (1892:xxx).

Avestan, like all Iranian languages shares a number of innovations with Indic and which are more apparent in the earlier languages (Vedic and Sanskrit for Indic and Avestan and Old Persian for Iranian). If a passage of Avestan can be turned into Sanskrit by the application of a few sound laws, it can be changed into Old Persian by even fewer. The famous lines of Yasna 9.5.1 describing Yima's Golden Age, *Yimahe xšaθre aurvahe* 'In the kingdom of swift Yima, [there was neither cold nor heat]' would be in Old Persian *Yamahyā xšaçaīy aruvahyā* while the Sanskrit would be *Yamasya rāṣṭre arvantah*, where only one of the words, the proper name, would be the same.

Iranian is marked by three striking innovations, all of which are found in Avestan. PIE *s before sonorants becomes [h] (thus Av. *haurvō* 'all, entire', OPers. *haruva*, but Skt. *sarvaḥ*) PIE voiceless stops become spirants before non-syllabics (Av. *xratūš* 'intention', OPers. *xratu* 'wisdom', but Skt. *kratuḥ* 'power, purpose'. And the distinction between voiced stops (Av. *dadātu*, OPers. *dadātu*, Skt. *dadātu* 'let him give' <

PIE **dedeOtu*) and voiced aspirates (Av. *adāt* [Y. 44.3], OPers. *adā*, but Skt. *adhāt* ‘he put, made’ < **Ee-d^heE-t*) is lost in Iranian but preserved in Indic.

Indic also has two striking innovations, neither of which appear in Avestan. Original PIE **sk̥* becomes PII **s̥c̥* which assimilated to PIr. **ss*, which, after the loss of simple *s*, became [s]: Av. *yasaiti*, Skt. *yacchati* ‘he reached out’ < PIE **iṃ-skē-ti*, OPers. *ayasata* < PIE **Ee-iṃ-skē-to* ‘he took for himself’. PII **č̥* becomes *k* before *š* so that PII **č̥š* (from PIE **k̥s*) and PII **k̥š* merge as PInd. *kṣ* but appear in Iranian as *š* (Av. *tataša*, Skt. *tatākṣa* ‘he fashioned’ cf. OPers. *us-tašanā* ‘staircase’, and *xš* (Av. *vaxšaiti*, Skt. *vakṣati* ‘it grows’ cf. OPers. *Uvaxštra* ‘Well-Grown’). The failure of Avestan to show any of the striking Indic innovations (including the development of the retroflected stops) and its agreement with Old Persian in all of the major Iranian innovations makes Beckwith’s claim untenable.

3. The claim first put forth by Trubetskoy that Proto-Indo-European might have arisen from a creole is similarly untenable. In the twenties and thirties of the last century, with little first-hand experience with living creoles, Trubetskoy’s wild guess could be taken seriously, but now that scholars have studied creoles intensely and have described their properties, among which include a simplification of grammatical (i.e. morphological) categories, such a claim for Proto-Indo-European makes no sense. Instead of a simple grammar with regular verbs inflected by largely analytic means (*I carry, shall carry, ought to carry*) and phonologically simple variants (*carrying, carried*) or minimal noun forms (often without marking for number), Proto-Indo-European confronts us with aspectual oppositions of durative (present), perfective (aorist), and stative (the traditional perfect) in a verb which is largely suppletive. The preterite form of Lat. *fero* ‘I carry’ is *tuli*. The comparable Greek form is *ἔνεγκα* beside present *φέρω*; Albanian *bie* ‘bring’ is matched by *prura* and the same situation obtains in Old Irish as well (*do-beir* vs. *ro-icc*). In addition to eight cases for the noun, we also must deal with an animate/inanimate opposition and three-way sex-based gender and three numbers—singular, dual, and plural—one of which, the dual is used by itself as in Skt. *pitṛāu* not to mean ‘the two fathers’ but instead ‘father and mother’. This is hardly the stripped down, economy-class grammar of a creole language.

Legend has it that Hittite presents a simpler, earlier grammar, but while Hittite lacks a number of cherished features of classical grammars like the sigmatic aorist and the irrealis or the a-stem nouns, the Hittite distinction between *mi-* and *hi-* conjugations, the nasal infixing classes of verbs, the preservation of a ninth case, the allative (shown by Harry Hoffner and Craig Melchert 2008:76), the array of thematic and athematic noun classes, and the use of a collective as well as neuter plural reveal a grammatical structure that is every bit as complex, quirky, and natural as any classical language and very far removed from any appearance of a creole.

4. The argument that Central Asia played an important role in Eurasian culture and history, the central thesis of Beckwith's book, and that that role was often played out on what later became the Silk Road, should not be denied. In fact, a linguistic examination of the most important commodity, silk, to pass over that road strongly supports his claim, even if it shows how complex the passage of influences was.

A common lateral phoneme rather than the usual rhotic in the word for 'silk' in Germanic (OE *sioluc* [an a-stem], ON *silki* and OHG *silehho* [n-stems]), Baltic (OPrus. *silkas*, Lith. *šilkas*, Žemaitian dialect *silkas*, and Latv. *silks*) and Slavic (ORuss. *šilkŭ*, R. *šělk*) points to a common source, also characterized by a palatal glide after the initial sibilant, for all three of these northern European languages. This common source was probably Scythian where the change of **rj* to [l] is regular (cf. Lat. *Alan-ī* 'Scythians' < PIr. **aryan-ās* 'Iranians'). Thus, Scythian **sjiloka-* from PIr. **sjirjāka-* (the change of PIr. **ā* to [o] is regular for Scythian (cf. the river *Don* < PIr. **dānu* 'river'). The palatalizations in the Iranian forms, however, are peculiar; but palatalization is a regular feature of Tocharian, thus the Iranian loanword is itself a metathesized loan from Tocharian, PT **sjārjika-*,¹ which represents the regular Tocharian outcome of **sērīko-*, the same word denoting silk that is seen in the Greco-Roman world, where silk goes by the Latin name *sēricus*, which is obviously a direct adaptation of Gk.

¹ That earlier **ē* caused palatalization and lowered to a vowel resembling the reflex of PIE **o* is clear from examples such as TA *want*, TB *yente* 'wind' < PIE **A₁ue₁ntos* which show palatalization of the [w] in Tocharian B and exhibit the same vowel correspondence seen in TA *ak*, TB *ek* 'eye' < PIE **Ook^w-s*. As Ringe noted, the later loss of length phonemicized palatalization in Tocharian (1996:131).

σηρικός. This Greek word, in turn, is an obvious adjectival derivative of Σήρες one of the names by which the Chinese or some other Asian people were known in the Mediterranean world.

That silk should be the “Chinese” cloth is hardly surprising. What is interesting is the possibility that the Tocharians had a similar designation. Although the adjectival suffix **-iqo-* is a commonplace in several Indo-European branches (it is for example the source of the New English adjectival suffix *-y* in words like *thirsty* and the nominal suffix *-ec* in Russian words like *grebéc* ‘rower’), it is not particularly productive in Tocharian, and the close similarity to Gk. *σηρικός* and Lat. *sēricus* suggests a Greek origin. Thus, the history of NE *silk* reveals a Chinese source (Gk. Σήρες), Greco-Roman consumers, whose appetite for silk influenced the language of Tocharian intermediaries and was passed on to Scythians, who in turned passed it on to tribes in northern Europe. Certainly the people of Central Asia played a vital role in the cultural and economic development of both the Greco-Roman and Chinese civilizations as Beckwith points out, and the linguistic development of one of the most important trade goods passing through that region, silk, shows their enduring influence.

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Asko Parpola, B. M. Pande and Petteri Koskikallio (eds), *Corpus of Indus Seals and Inscriptions. Volume 3: New material, untraced objects, and collections outside India and Pakistan, Part 1: Mohenjo-daro and Harappa*. Helsinki, Suomalainen Tiedeakatemia, 2010. 442pp. ISBN 978-951-41-1040-5, ISSN 1239-6982. 295 euros.

The present volume continues a massive project begun more than 35 years ago to provide a proper catalogue of all the Indus seals and inscriptions. Previous volumes dealt with the collections held in India (Volume 1, 1987) and Pakistan (Volume 2, 1991). However, these did not contain all the inscriptions known as a very substantial number, although often recorded, photographed and entered into a museum, have since been either stolen or simply disappeared. Also, there are 42 objects (recorded in this volume) that are housed in 14 different museums in eight countries outside India and Pakistan. Moreover, further excavations have augmented the number of seals. The editors have brought all this material together for Volume 3 of the project where it derives from the key sites of Mohenjo-daro and Harappa.

In addition to the catalogue there are several articles. One, by Ute Franke, discusses fragments of two metal seals that were recovered from Mohenjo-daro but were imported from the BMAC in the centuries around 2000 BC. They attest to the expansion of BMAC trade connections with the Indus civilization on the eve of the collapse of Indus urbanism. The second article by J. Mark Kenoyer and Richard Meadow provides a very useful survey of the context and date of inscribed objects (seals, graffiti, tablets, etc.) from the excavations at Harappa undertaken in the period 1986-2007. With much greater contextual and chronological control they examine the changes in the Indus seals over the approximately 700 years of their existence, illustrating their observations with references to the accompanying catalogue.

Volume 38, Number 3 & 4, Fall/Winter 2010

The final article is a brief contribution by Asko Parpola on the identity of Major General M. G. Clerk who was the owner of the first inscribed seal from Harappa that helped stimulate the earliest research into the Indus civilization.

Needless to say, the vast bulk of the volume comprises the 412 pages of photographs, including 46 pages in color and the accompanying catalogue. These constitute what Kenoyer and Meadow rightly describe as “monuments to Asko Parpola for his foresight, efforts, stamina, and patience in preparing a compendium to which we can all refer with confidence”. But the monument is still not complete as the author is currently working on a Volume 4 that will present material from all the other sites and provide an updated computer edition of the corpus along with the concordances and statistics that take into consideration all the newly published material.

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Culture

Nikolai Tolstoy, 2009, *The Oldest Prose Literature, The Compilation of the Four Branches of the Mabinogi*. Lewiston, NY, Queenston, Ontario, and Lampeter, Ceredigion, Wales; The Edwin Mellen Press. Foreword by Nicolas Jacobs, (i-iii), preface (v – vii), text 1- 543, references, 545-58, index, 559-67, biographical paragraph at end page.

The book is a vast and authoritative work on the *Mabinogi*, the Welsh book of myth-like tales, divided into four “branches.” (‘Mabinogion,’ for many the more familiar term, is a *hapax* and an apparent scribal error.) The scholarship is prodigious. Tolstoy’s command of the literature is exhaustive. While the current reviewer is not a Celticist, it would seem that every contributor to the field and every argument is addressed by Tolstoy at length. The main arguments proceed in the text, while most arguments with other authorities, including the occasional correction, are in the massive footnote apparatus, where citations can also be found. At times Tolstoy argues in meticulous detail and at others he summarily and usually convincingly disposes of some line of contention by falling back on what is an obvious and sensible assessment

The Journal of Indo-European Studies

of some crucial passage or narrative line. Not only does Tolstoy deal with a vast array of scholarship, he also commands a wide range of languages. Citations in Old Welsh, Old Irish, Latin, Old English, and Old Norse, pepper the book, usually without glosses but with enough context to make their sense clear. This is in addition to such stock phrases as Latin *terminus a quo* 'starting point', *terminus ad quem* 'end point.' Old Welsh terms, such as *cyfarwydd* 'poet, bard,' pepper the text and the reader must become accustomed to the peculiar orthography.

This is clearly a book for a specialist audience, but would be profitable for the comparativist as well. The book is in some sense a mirror image of a comparativist study. Plausible interpretations are offered for the many unusual features of the *Mabinogi*, but virtually no comparands are put forward. Instead what Tolstoy has done is seek to explain how the work has evolved from those presumed originals to its attested form. Comparativists generally dismiss or minimize the factors that have brought an attested work to its form, citing such processes as historicization (Dumézil), epicization, or moral inversion (the last two concepts devised by Puhvel). Tolstoy is much more precise. He seeks out specific events that have led the bard or the bardic lineage to render earlier forms into culturally comprehensible forms.

Tolstoy's arguments are pinned to two crucial points. The first is that the *Mabinogi* as attested is the work of one author. Second, that this bard lived at a particular period. From these two "axioms," Tolstoy then builds up a series of plausible original narratives, and explains the distortions that have led to the existing product in terms of specific events and personages familiar to this nameless bard and to the audiences of his place and era. In general outline the book runs as follows.

Chapter one argues that the work is that of a single author, with mindful caveats regarding all the difficulties such bardic work exhibits, seen in Homer, etc. Note in chapter two, p.43, "A unity of style, lucidity of dialogue, skilled characterization, and generous humour permeate the four tales [Branches] to an extent irresistibly suggestive of the work of a single creative mind." As this quote shows, it being taken from the next chapter, Tolstoy's style of arguing often extends over several chapters and builds as new material is explicated. So, although the topic of unity of authorship is

introduced in chapter one, it is taken up again in chapters two and eight. This reader required some habituation to this episodic argumentation, but once it became evident as a medium for handling complex lines of thought it became compelling and at times even elegant.

Chapter two focuses on the dating of the work, subject to difficulties of modernizing overlays, and archaic retentions. The most interesting argument in this chapter is that of a myth and ritual posited by Tolstoy (p. 59): Manawydan's act of sparing from hanging the wife of the magician Llywd vab Cil Coed, who has been turned into a fat field mouse by her husband, is a relic of an old myth and ritual intended to ward off the ravages of rodents. This is an example of one of Tolstoy's vivid plausibility arguments. Tolstoy argues in this chapter that the work was composed between 974 and 1093 because of references to historical personages or events (pp. 81-82). Having set these two premises in place, the remainder of the work addresses the historical forces prevalent in that era that have gone into creating the attested *Mabinogi*.

Chapter three addresses Branch two, the tale of Branwen, (Branches 1 and 4 are treated largely in passing in chapters 3, 6, and 8.) Her brother, Bran, always termed Bran the Blessed (*Bendigeidfran*, where *fran* [vran] is the lenited form of Bran), crosses the Irish Sea to rescue her from an abusive husband, Matholwch. The whole is seen as a reworked account of passing to the land of the dead, Annwfn or Caer Sidi, with rivers, Lli(non) 'stream, current, flood' (p. 154) and Liffey, with a third, the Archan, Achren/Ochren, being derived from the name of the Acheron in Greek, an indication of Classical learning in the bard (pp. 155-156). This is an excellent explication of narrative as an elaboration of older myth. The rivers are comparable to the Styx and other rivers of Hades in Greek myth. Bran is clearly mythical in that even in the attested text he is so large as to be virtually impossible to house. He uses his enormous body as a bridge for his troops to pass over.

Tolstoy explicitly argues that Bran is a psychopomp and cites a parallel with Mithraic traditions (pp. 135-140). He is literally a bridge for his people to an "other" world. I might add here as a comparativist that this function of Bran has an important semantic parallel in the Latin *pontifex* 'way maker, pace setter, expediter' and Vedic *pathi-krt-*, terms used of both

gods and priests (Puhvel 1987: 148). Bran is literally a pontifex retaining full watery connotations, since Indo-European **p(o)nt-H-* seems originally to have denoted a lake or watery boundary. Tolstoy expresses some puzzlement as to why Bran bears the inseparable epithet 'Blessed,' since as a figure he is thoroughly pagan, but his blessed status would be of Indo-European origin.

I might suggest here that the devastating war between Bran's followers and the Irish, a war that leaves a mere seven survivors to replenish the population, has a strong eschatological tone to it and might reward further comparison with Norse Ragnarök.

Chapter four examines the historical foundations for the details in the tale of Branwen, especially regarding the "Bridge of Hurdles." Tolstoy shows how the original myth was altered to reflect a fictitious British war with Ireland. He astutely examines the Old Welsh annals and toponymy.

Chapter five address a number of themes. First Tolstoy builds a case for seeing the attested Bran as modeled after a king of Munster, Brían Bóromha. Similarly a strong case is made that his brother-in-law Matholwch has been modeled after Mael Sechnaill, ard rí (High King), with some features of Sigtryggr of Dublin, a Viking king added. Upon Matholwch's arrival to seek Branwen's hand his horses are quartered throughout Britain. Tolstoy shows that this odd detail is based on actual Viking practice. Tolstoy argues that the enigmatic name Branwen has been reshaped after Ronwen, Viking wife of Vortigern, daughter of the Saxon, Hengist.

Second he takes up the theme of the Iron House, a warrior icon with its cauldron of rebirth (pp. 333-51). Llassar and his wife, Cymidei are an evil couple whose evil offspring spread first across Ireland. The Irish attempt to kill the two by trapping them in an iron house that is then heated red hot. The couple escapes to Britain and brings with them a cauldron of resurrection. The fable is confusing and probably has multiple roots, but Tolstoy argues that its chief inspiration may be Æthelred's attempt to slaughter the Danish settlers in England. This met with retribution from the Danish king, Sveinn, who conquered all of England after 10 years of vengeful attacks.

Third he examines the underground treasures that are plundered. He argues that this theme reflects Viking raids on

grave mounds, *sidhe*.

Left unexplained, and seemingly inexplicable, is the figure of Efnisien, half-brother to Branwen, who commits much bizarre mischief, even killing the child of Matholwch and Branwen, Gwern, by holding him headfirst in a fire. His spiteful and brutal behavior suggests parallels with Norse Loki or Ossetic Syrdon, but his murder of Gwern suggests a Dumézilian second function royal sacrifice, which might then point to a parallel with Norse Starkadr.

Tolstoy argues in chapter six that the third Branch, *Manawydan, son of Llyr*, is an authorial creation. It has historical roots, but more than the other three branches it shows the artistic freedom of the bard. Tolstoy argues that the villainous figure in this Branch, that of Caswalawn, has been inverted from the historical and heroic resister of the Roman invasion to the evil, magical invader, which if true is a sadly ironic fate for this figure. Three disasters or evil fates (*gormes ~ gormeseodd*, much like the Irish *geis ~ geasa*) are discussed, hinting at Dumézilian functional tripartition. Ultimately this figure is linked to the invasions of the Dane, Sveinn, and his son and successor, Knutr. On pp. 431-433, Tolstoy draws up tables listing parallels between the *Mabinogi* and historical events of the early 11th century. These parallels confirm the date he argued for in chapter two (see pros and cons of dating as summarized on pp. 424-430).

In chapter seven Tolstoy summarizes the distinction between old mythic elements and contemporary historical influences, while continuing to address the third Branch. The former are: Brans' campaign in Ireland (= journey to the Otherworld), abduction of the Maiden Sovereignty, Bran's journey to the Otherworld to retrieve an enchanted cauldron, Bran's unhealable wound, the sacral function of Bran's head.

Tolstoy cites Eliade's example of the Campa tribe of the Peruvian mountains, and how they incorporated the Spanish conquest into their mythology. "Thus the recasting of *Branwen* and *Manawydan* in terms of early eleventh-century politics reflects a widespread mythographic pattern." (p. 440).

Tolstoy concludes this massive book with chapter eight in which he attempts to identify the author of this work. He argues for an exact site of the author's writing, mostly *Dyfed*, but the fourth Branch shows some source from *Gwynedd*. His findings are summarized on pp. 538-539, especially point 8,

that the author was the *pencerdd* (roughly ‘privileged bard’ or ‘head bard’) of Tenby. Remarkably Tolstoy has given us a plausible identity and locus for the writer of this civilizational work. Only his name is lacking.

In addition the book offers an uncanny sense of an alternative Britain, a Celtic world underlying the Germanic one. In this regard the work almost unintentionally transcends the usual scholarly effort on Welsh lore. As one marginal to British society, both ranked as a peer with its aristocracy and yet not a member thereof, Tolstoy exhibits a unique sensibility as a historian and Celticist. In effect he is aware of layers of “Britishness” beyond the ken of those more conventionally placed in that society. The current work reflects his expertise as a scholar, but also resonates with insights from this penetrating vantage point. Only someone steeped in the history and culture of Britain but reared in an alternate tradition, that of Russian Orthodoxy such as Count Tolstoy, someone sensitive to the mutations that time can impose upon, culture, language, and sensibilities, could have given this account of the remote underpinnings of British society. A subtext emerges from the scholarly analysis, namely, the shock and desperation of a Celtic world beset by invaders and reeling back from what was once its full domain of “Prydein,” that is, Britain. I for one shall never look upon Britain and its Celtic margins in the same way.

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Linguistics

Barry Cunliffe and John T. Koch (eds) *Celtic from the West: Alternative perspectives from archaeology, genetics, language and literature*. Oxford, Oxbow, 2010. 384pp. ISBN: 978-1-84217-410-4.

This volume of eleven papers follows a conference on the same subject and is largely driven by the desire to explore a

new hypothesis regarding Celtic origins. The editors have found the traditional model of a Hallstatt/La Tène origin for the Celts to be increasingly limited (in some cases totally repudiated by all available evidence) and, rather, they believed it was worth while exploring a different hypothesis: “Celtic probably evolved in the Atlantic Zone during the Bronze Age”. This forms the primary agenda of the volume, arranged under the headings of ‘Archaeology’, ‘Genetics’ and ‘Language and Literature’, although not all the contributors adhere to an Atlantic origin.

The first paper by Barry Cunliffe reviews the historical and archaeological evidence in support of an Atlantic origin. The primary focus is on the initial spread of the Neolithic economy to Portugal and then the opening up of the Atlantic façade as an interaction zone seen in such phenomena as the spread of the Neolithic, megaliths, areas of exchange in stone axes, and later the Atlantic expansion of the Beakers and later Bronze Age horizons of metal working that suggests that there was a distinct cultural zone for the Atlantic from the Neolithic onwards. Cunliffe concludes with a series of questions that suggest the possibility that the Indo-European languages were carried into Atlantic Europe across the Mediterranean, that Celtic may have formed either during the Neolithic or, possibly, with the rise of the Beakers in Iberia from whence they spread across the rest of the Atlantic zone. Alternatively, he recognizes the possibility that there is an ‘eastern’ component in the Beaker phenomenon and that Celtic may have spread this way as well. In any event, he opens the possibility that Celtic is quite early in Indo-European expansions (a Celtic formation and expansion before *c* 2000 BC would place it on the rough temporal plain often imagined for Indo-Iranian or Greek) and that its point of departure was the far west rather than the north Alpine region most often proposed.

Raimund Karl examines how both our lack of precise definitions for what the word ‘Celt’ means and the elusiveness of any attempt to track such a phenomenon to its point of origin has hindered research into more important questions. He provides a definition for Celt that is discipline sensitive: “a Celt is someone who *either* speaks a Celtic language *or* produces *or* uses Celtic art *or* material culture *or* has been referred to as one in historical records *or* has identified himself *or* been

identified by other as such &c.” He illustrates this with a detailed example of associative ‘Celticity’, how tracing archaeological phenomena and language identification through time yields a complex system of networks that defies a single point origin.

Amílcar Guerra reports on the discovery of several new Tartessian inscriptions from south-west Iberia. Among these the most important is a stela from Mesas de Castelinho which now constitutes the longest Tartessian inscription known. The inscription comprises 82 signs of which a phonetic value can be given to all but two. The article also contains a valuable map of all the Tartessian inscriptions known. Tartessian is certainly one of the main themes of this volume and Philip Freeman provides a list of all significant classical references to Tartessos to the collapse of the western Roman Empire.

By far the longest article is John Koch’s extensive treatise on Tartessian as a Celtic language, a subject which he has also tackled as a monograph (Koch 2009). Here we are dealing with 95 inscriptions so far for a language which has in the past been regarded as purely non-Indo-European, non-Indo-European with some Celtic loanwords or personal names, and now, according to Koch, a purely Celtic language. As Tartessian is attested from *c* 700 BC, it also becomes the earliest attested Celtic language depriving Lepontic of its claim to priority. Koch emphasizes that any traditional equation of La Tène or Hallstatt with Celtic fails to explain the Iberian evidence and that Celtic origins must be sought earlier. He also dismisses the Urnfield culture on various grounds among which it also provides a source for other very different Indo-European languages. Rather, he emphasizes that Tartessian was fully integrated in the Bronze Age network of circum-Atlantic cultures that included Britain and Ireland (he still adheres to a Late Bronze Age horizon of *c* 1200-600 BC for the spread of Celtic to Ireland). He remains agnostic concerning the attempt to associate Insular Celtic with Hamito-Semitic, with the latter generally seen as a substrate, but does suggest that some Hamito-Semitic loans might have passed from Phoenician through Tartessian into some of the other Celtic languages. The core of Koch’s paper is a presentation of about eighty Tartessian texts including facsimiles of the inscriptions, transliterations and interpretations coupled with a Tartessian vocabulary and sketch of its syntax and dialectical position.

The other Indo-European language of Iberia, Lusitanian, is surveyed in some detail by Dagmar Wodtke who provides a valuable overview of the language and discusses in some detail the difficulties of discerning where Lusitanian may leave off and Celtic begin.

David Parsons attempts to examine whether it is possible to derive prehistoric information as to the origins and dispersal of the Celts by examining place-names. He initiates his study by showing to what (little?) extent we can follow the course of the Anglo-Saxon conquest through place-names and then shifts his focus to the Celtic evidence. Here, isolating out earlier layers (to explain, for example, putative temporal differences between **brigā* and **dūno-*) proves doubtful and the fact that Celtic place-names from Ireland to central Europe seem to be so little differentiated raises major hurdles for anyone trying to build directionality into the evidence.

Not all contributors are supportive of a western origin for the Celts as can be seen in G. Isaac's "The origins of the Celtic languages: Language spread from east to west". Dismissing the concept of Italo-Celtic, Isaac emphasizes that the diagnostic indicators of Celtic are firmly with more easterly languages. These include the relative pronoun (**ios, *ieh₂, *iod*) which is shared with Slavic, Greek, Indo-Iranian and Phrygian; the future tense suffix **-sie-/-sio-* which is also found in Baltic, Slavic, Greek and Indo-Iranian; and the reduplicated thematic sigmatic desiderative which is shared only with Indo-Iranian. He argues that all of these innovations suggest that the ancestors of Celtic were proximate to the other late IE groups (Baltic-Slavic, Indo-Iranian, Greek) and should have occupied eastern Europe around the 4th and 3rd millennium BC. Other examples of features that anchor Celtic in the east are the treatment of some laryngeals in initial position that is only shared with Tocharian.

The volume also includes three papers employing a genetic approach to the problem. Ellen Røyrvik surveys the methodological issues of distinguishing the dispersal of the Celtic languages, either from a central-west European or Atlantic core, in terms of what would be required of the genetic evidence. Brian McEvoy and Dan Bradley summarize the research on "Irish genetics and Celts". Genetically, Ireland is clearly aligned more strongly with the rest of Atlantic Europe, presumably as a result of post-glacial expansions across

the region from Iberia and southern France. There are also traces of 'Neolithic = Near Eastern' genes within the region that diminish the further west one samples. Conversely, there is no case to be made for a major influx of central European genes from a putative Celtic homeland that some have envisaged although the authors admit that small scale migrations might have occurred and they even suggest one such possibility for the northeast of Ireland which might have been associated with the spread of the La Tène to Ireland. The last of the genetic papers, by Stephen Oppenheimer, re-examines the genetic evidence for migrations to Britain and Ireland from a Celtic perspective. As is the case with the other studies, the overwhelming genetic composition of the modern people of Britain and Ireland appears to derive from the post-glacial movement of populations from refuge areas in Iberia northwards along the Atlantic followed by some contribution from eastern populations during the Neolithic. Since this renders most of the population either indigenous Mesolithic or partly Neolithic, it undermines any claims that the majority of the British or Irish population could be genetically 'Celtic' (i.e., derive from a migration of Celtic speakers) and it also challenges any claims that the Anglo-Saxon migration resulted in the genocide/replacement of the 'native' population. He does find some potential later genetic linkages between Britain, Ireland and Iberia which just might be associated with the spread of metal prospectors/workers from Iberia to the north.

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Volume 38, Number 3 & 4, Fall/Winter 2010

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Odin and the (Indo-) Germanic Männerbünde**

Monograph No. 36 — By Kris Kershaw

Abbreviations; Glossary. *THE EYE IN THE WELL*: *grandaevus altero orbus oculo*; Odin's pledge; *Heiti* relating to Odin's eyesight; *Blindr*; Other possible *heiti*; "Blind," not "the blinder"; Other depictions of Odin/Wodan; Snorri and Saxo; Wodan, Woden, et. al; Iconography; Odin the Wolf-god; An overview of the book; **PART I. HERJANN: THE EINHERIAR**: Snorri's description of the *Einheriar*; The word *einheriar*; *Herr and Herjann*; PIE **koryonos*; Thor *Einheri* and the *Einheriar*. *DER SCHIMMELREITER*: The Host and the Hunt; Legend, myth, and cult; The matter of the *Männerbund*; The Dead and the living; Age sets and ancestor cult; Masks; Masks and ancestor cult; Demon horses; The Rider-god; The Ancestors bring blessings; Feasts of the Changing Year; Harlequin. *FERALIS EXERCITUS*: *Harii*; *Chatti*; *Weihekrieger*; *Civilis*; Haraldr Háfagr; The hairstyles of the *Suevi*; Procopius and Ammianus on youthful warriors; An analog from Doric Greece; The liminal state (*marge*); Exiting *marge*; Demon warriors; An initiation scenario in *Völsungasaga*; Dæmon warriors among the Chatti; Two armies of the dead; *Mercurius*; Hermes at the boundaries of space and time. *FUROR TEUTONICUS*: **wob-*; *Ódr*; *Óðinn*; The suffix -no-; Examples; A "Führersuffix"?; The suffix -no- in divine names; Poets' god and Rune-master; Ecstasy, Possession, Inspiration, Madness; Ecstasy; *Furor heroicus*; War dances; Dancing gods; *Mysterium* and *Mimus* in the *Anabasis*; The sword-dance in Germania; Mars and the *Salii*; *Κούρητες, Κουρήτες, Κορύβαντες*; The *Maruts*; Some conclusions; *Veratyr*; **PART II. THE INDO-EUROPEAN MÄNNERBUND**: **TEUTĀ* AND **KORYOS*: *Equites* and *pedites*; Village and Forest. *THE *KORYOS*: *Raubrecht*; The *Manes*; Animal transformations; *Ἑκστασις*; Times and seasons; Festivals; Seasonal activities; Sub-groups; Small bands; Groups of 50; Older **koryos-bündler*; *Weihekrieger*; Men without property; Robbers and riff-raff; *Männerbund* and *Gefolgschaft*. *CANIS AND THE *KORYOS*: Wolf and Dog; Ethnonymns and *Männerbünde*; "Wolf"-men as founders of city-states; An historical example; Mythical foundation stories a) The founding of Rome b) Caeculus and the founding of Praeneste; The *ver sacrum*; Cyrus, the founder of the Persian Empire; Wolf-folk and Dog-folk; Ethnic names a) Wolf-folk of Anatolia b) An Ossetian wolf-clan c) Other wolf-folk d) Dog-folk e) *Hundingas* and *Ylfingar*; Wolf-priests a) *Hirpi Sorani* b) Some Hittite cult functionaries; Mythical ancestors a) Miletos b) Lamissio, king of the Langobards c) The Welfs of Swabia; Wolf-men and Dog-men; The Lombards; Germanic wolf-names; Irish dog and wolf names; Greek heroes with wolf-names; Wolf- and dog-men of the Scythians; Slavic wolf-men; *Κυνοκέφαλοι* a) "Zu den litauischen Werwölfen" b) Slavic dog-heads

c) Survivals of the *κυνέη/ λυκέη*; *Ἄλκιμώτατοι κύνες*; “*Ver sacrum* bei den [Indo-]Germanen?”; Canine/ lupine qualities; *ODIN ANALOGS*: India; Iran; The Ossetes; The Celts; The Balts and Slavs; Greece; Italy; Mars; Faunus; Veiovis; **teutā*-god and **koryos*-god; *Un rite d’agrégation*; **PART III. THE VRĀTYAS: WARRIOR-BRAHMINS**: An oath-brotherhood; Vratya clothing and weapons; Seasonal activities; The brahmācārin; The education of a brahmin; The vrātyastoma; *Vrātyastoma* and *sattra*; *Sattra*; *Dakṣiṇā*; Vrātyas and sattrins; *RUDRA*: Rudra’s armies; *Ganapati*; The rudras; The Maruts; A troop; The Maruts are both like and unlike Rudra; The Maruts as **koryos*; I-Ir. **marXa*; Marut epithets; Priestly activities; War-god; The Wild Hunter; *Canis*; Dogs and the Lord of Dogs; *Dasarā*; *Le jeu du Cheval*; *Têtes coupées*; The hunter with the spear; Some conclusions; Death; Fertility; *Ekstasis*; The Feast of the Changing Year; The Dragon-Slayer; Initiation; *Natarāja*; The *Ekavrātya*; *CHOOSING A LEADER*: The *Sūlagava* sacrifice; Two stories; The vrātyas as rudras; The dice game in early India; The “dice”; How it was played; An army of dice; The dog and the dog-killer; Kali, the dog; Kali *eko’kṣah* and *ekākṣa*; The One and the Dog; The ritual dice game; *senānīr mahatō ganāsya*; *Sabhā* and *irinā*; *Sabhā* and solstice; *Hejann*; *Excursus*—The Vrātyastomas; *DARKNESS, DOGS, AND DEATH*: Conclusion—The Wolf-god and the Eye in the Well; Bibliographies; Primary Sources; Works Cited; Index.

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2000, Pages 306, Paperback: \$48.00

The Historical Morphology of the Baltic Verb

Monograph No. 37 — By William R. Schmalstieg

The purpose of this book is to suggest a possible scenario for the history of Baltic verbal morphology with relatively little attention to semantics and syntax. The various stages of development from a reconstructed Proto-Indo-European verbal system to the attested systems of the extant Baltic languages are proposed. Various innovative theories of the author and other contemporary specialists in Baltic historical linguistics are discussed and evaluated, in many cases making available the results of their work available in English for the first time. In addition to a large bibliography on the Baltic verb the book is supplied with an index of each word form discussed.

ISBN 0-941694-76-3

2001, Pages 445, Paperback: \$56.00

Greater Anatolia and the Indo-Hittite Language Family:

Papers presented at a Colloquium hosted

by the University of Richmond, March 18-19, 2000

Monograph No. 38 — Edited by Robert Drews

Robert Drews: Introduction and Acknowledgments, Opening Remarks; **E.J.W. Barber**: The Clues in the Clothes—Some Independent Evidence for the Movement of Families; **Paul Zimansky**: Archaeological Inquiries into Ethno-Linguistic Diversity in Urartu; **Peter Ian Kuniholm**: Dendrochronological Perspectives on Greater Anatolia and the Indo-Hittite Language Family; Discussion Session, Saturday Morning; **Colin Renfrew**: The Anatolian Origins of Proto-Indo-European and the Autochthony of the Hittites; **Jeremy Rutter**: Critical Response to the First Four Papers; Discussion Session, Saturday Afternoon; **Margalis Finkleberg**: The Language of Linear A—Greek, Semitic, or Anatolian?; **Alexander Lehrmann**: Reconstructing Indo-Hittite; **Vyacheslav V. Ivanov**: Southern Anatolian and Northern Anatolian as Separate

Indo-European Dialects and Anatolian as a Late Linguistic Zone; **Bill J. Darden**: On the Question of the Anatolian Origin of Indo-Hittite; **Craig Melchert**: Critical Response to the Last Four Papers; Discussion Session—Saturday Morning; **Robert Drews**: Greater Anatolia, Proto-Anatolian, Proto-Indo-Hittite, and Beyond; **Geoffrey D. Summers**: Appendix—Questions Raised by the Identification of Neolithic, Chalcolithic and Early Bronze Age Horse Bones in Anatolia. Index.

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2001, Page xiv and 305, Paperback: \$52.00

A Definitive Reconstructed Text of the Coligny Calendar

Monograph No. 39 — By Garrett S. Ostmsted

The fragmentary calendar plate from Coligny (near Lyons) apparently dates to the second-century AD, although the Gaulish calendar engraved on this plate is plainly the result of a long transmission process. The 25-year-cycle calendar, the final system of this transmission process, probably originated early in the first-century BC, before Caesar's conquest. It is within this late pre-Roman period that the calendar took on its final form and notation to enter a two-century long transmission process. Since only 40% of the original Coligny calendar survives as a fragmentary mosaic, the reconstruction of the original whole depends upon recognizing repetitive patterns and filling in the missing sequences of these patterns. The most significant of these patterns is that discerned in the schemes of the TII and the N lunar/solar counting marks and their associated notation. Here the chronological cycles implied by these notational patterns are explained in detail. Also provided is a glossary of the functional and etymological significance of terms utilized in these daily notational patterns. The fragmentary calendar is brought to photographic completion utilizing the original wording and engraving found on the surviving fragments.

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2001, Pages 120, Paperback, 70 plates: \$40.00

Proceedings of the Twelfth Annual UCLA Indo-European Conference: Los Angeles, May 26-28, 2000

Monograph No. 40 — Edited by Martin E. Huld, Karlene Jones-Bley, Angela Della Volpe, Miriam Robbins Dexter

Introduction; Language Abbreviations; **PHONOLOGY AND MORPHOLOGY—SOUND AND SENSE**: The Sound-Systems of Proto-Indo-European, **George Dunkel**; Against the Assumption of an IE “**k^w* etuóres Rule”, **Jens Elmegård Rasmussen**; The Reflexes of Indo-European *#*CR*- Clusters in Hittite, **Aleksei S. Kassian and Ilya S. Yakubovich**; Proto-Indo-European Root Nouns in the Baltic Languages, **Jenny Helena Larsson**; Verb or Noun? On the Origin of the Third Person in IE, **Birgit Anette Olsen**; Indo-European **b^h*uH- in Luwian and the Prehistory of Past and Perfect, **Vyacheslav V. Ivanov**. **EPIGRAPHY AND ETYMOLOGY—WORDS AND THINGS**: The Poggio Sommavilla Inscription, **Giovanna Rocca**; The Etymology of Some Germanic, Especially English Plant Names (*Henbane, Hemlock, Horehound*), **Anatoly Liberman**; ‘Elephant’ in Indo-European Languages, **Václav Blaz'ek**. **MYTHOLOGY AND POETICS—FORM AND FANCY**: The Persistence of the Indo-European Formula “Man-Slaying” from Homer through Gregory of Nazianzus, **Edwin D. Floyd**; Hermes and Agni—a fire-god in Greece?, **Paul-Louis van Berg**; Dumézil, a Paradigm, and *Iliad*, **Thomas R. Walsh**; Dumézil

Volume 38, Number 3 & 4, Fall/Winter 2010

in 2000—An Outline and a Prospect, **Dean A. Miller and C. Scott Littleton**. **RETHINKING ARCHAEOLOGY—MYTH, CULTURE, AND MODELS:** The Bird Goddess in Germanic Europe, **Mary Lynn Wilson**; Village Life to Nomadism—An Indo-Iranian Model in the Tien Shan Mountains (Xinjiang, China), **Jeannine Davis-Kimball**; Perpetuating Traditions, Changing Ideologies—the Bell Beaker culture in the British Isles and its implications for the Indo-European problem, **Marc M. Vander Linden**; Towards an Understanding of the Indo-European Origin Problem—Theoretical and Methodological Interfaces, **Bryan K. Hanks**. **EPILOGUE—NEW RESEARCH TOOLS:** The Internet and Publication and Research in Indo-European Studies—Present State and Future Prospects, **Deborah Anderson**; Index. ISBN 0-941694-79-8 2001, Pages 326, Paperback: \$46.00

Pre-Indo-European

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THE BASES FOR RECONSTRUCTING PRE-IE: Advances in the Sciences and Fields Relevant for Indo-European Studies; Pre-Indo-European—an Active Language; Genetics and its Importance for Identifying the Indo-European Speakers in their Spread; Archeology and its Contribution to our Information on the Early Period of Indo-European Speakers; Indo-European as one of the Nostratic Languages; The Primary Bases for Reconstructing Pre-Indo-European. **FROM PIE TO PRE-IE:** The Common Source; The Comparative Method; The Method of Internal Reconstruction for Morphology and Syntax; The Use of Residues; Determination of Chronological Strata in Language; Typological Findings as Guides to Interpretation of Data; Characteristics of Active Languages; Inferences Based on Application of these Methods and Conclusions concerning Language Structures; Earlier Analyses of the Lexicon that Support the Assumption of Pre-Indo-European as an Active Language; Stages of Proto-Indo-European. **RESIDUES IN PIE THAT PROMPT ITS IDENTIFICATION AS A REFLEX OF AN ACTIVE LANGUAGE:** The Importance of Examining Residues as Illustrated by the Clarification of Germanic Phonology by Jacob Grimm and his Successors; Explanations of such Residues by a Historical Approach and the Assumption of Stages in Languages; Pre-Indo-European as an Agreement Language of the Active Sub-type; Doublets as Reflexes of Earlier Active Structure in the Lexicon; Reflexes of Active Languages in Nouns, Verbs, and Particles; Sentence Patterns of Active Structure as Found in the Early Dialects; Morphological Patterns Reflecting the Earlier Active Structure; Previous Recognition and Explanation of Active Language Characteristics in the Indo-European Languages; Conclusion. **LEXICAL STRUCTURE:** The Lexicon in Active Languages—Nouns, Verbs, and Particles; Nouns—Active/Animate and Stative/Inanimate, and the Introduction of Gender Classes; Sets of Nouns in Accordance with their Meaning; Words for the Family and its Arrangements; Verbs—Active and Stative; Involuntary Verbs; Centrifugal and Centripetal Uses of Verbs; Particles; The Particles Proper. **SYNTAX:** Active Language Syntax in Pre-Indo-European; Basic Word Order in the Sentence; Complex Sentences; the Use of Participles and Other Non-finite Verb Forms; Uses of Participles in the Early Dialects; Subordinate Clauses and the Development of Particles to Conjunctions; Classes of Particles; The Meanings and Origins of Selected Particles, and their Application as Morphological Markers; The Position of Particles with Reference to Nouns

The Journal of Indo-European Studies

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Volume 38, Number 3 & 4, Fall/Winter 2010

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The Journal of Indo-European Studies

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Volume 38, Number 3 & 4, Fall/Winter 2010

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Monograph 46 — Edited by Karlene Jones-Bley D. G. Zdanovich

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INDEX TO VOLUME 38

Barber, E. J. W. On Middle Minoan Sites and Sight Lines: Communication Strategies in the Bronze Age Mediterranean	1
Brosman, Paul The Cognates of the Gothic <i>u</i> - Stems	384
Carey, John Donn, Amairgen, Íth and the Prehistory of Irish Pseudohistory	319
Cebrián, Reyes Bertolín Some Greek Evidence for Indo-European Youth Contingents of Shape Shifters	343
Ferrer, Eduardo Blasco Iberian ordumeles, Paleo-Sardinian Ortumele, Ortarani and Araunele. Cognitive Semantics and Substrata Research	373
Fournet, Arnaud About the Mitanni-Aryan Gods	26
Greppin, John A. C. Armenian <i>t</i> - ‘not’ and the Proto-Lezgian Dental Negative	203
Jia, Peter Wei Ming and Alison V. G. Betts A Re-analysis of the Qiemu’erqieke (Shamirshak) Cemeteries, Xinjiang, China	275
JIES Reviews Archaeology	210, 431
Culture	233, 444
Linguistics	449
Jurić, Dorian A Call for Functional Differentiation of the South Slavic <i>Vila</i>	172
Kazanas, N. <i>Rgveda</i> 7.95.2 and Karen Thomson	409
Kroonen, G. J. Proto-Germanic <i>*krēþja-</i> and Proto-Slavic <i>*krěþb</i> ‘strong’	402

Peyrot, Michaël Proto-Tocharian Syntax and the Status of Tocharian A	132
Razmjoo, Seyyed Ayatollah, Farrokh Hajiani, and Jalal Rahimian On the Derivational History of Iranian <i>*mairya</i> and <i>*mariyaka</i>	359
Thomson, Karen The Plight of the Rigveda in the Twenty-First Century. Response to N. A. Kazanas	422
West, Emily A Quartet of Graeco-Aryan Demi-goddesses: Leukothea, Eidothea, Ulūpī and Vargā	147
Woudhuizen, Fred C. Towards a Chronological Framework for Significant Dialectal Tendencies in Indo-European	41