

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 [dʰ] are allophones of a single phoneme (1995: 16), that phoneme is the one Grassmann and other recognized as **dʰ*; Grassmann's **d* would be the equivalent of Gamkrelidze and Ivanov's *+l'*, so it is never an allophone of **dʰ*. Gamkrelidze and Ivanov's claim that [t] and [tʰ] are allophones of their *+t* (traditional **t*) and [d] and [dʰ] are allophones of their *+d* (traditional **dʰ*) was not intended as and cannot be misrepresented as a claim that traditional **d* and **dʰ* (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>dʰer-</i> 'fasten, fix' LIV 145	<i>uedʰ-</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:xxxii-xxxiii), 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-sk̥e-ti*, OPers. *ayasata* < PIE **Ee-iṃ-sk̥e-to* ‘he took for himself’. PII **č̥* becomes *k* before *š* so that PII **č̥š* (from PIE **k̥s*) and PII **k̥š* merge as PInd. *k̥š* but appear in Iranian as *ṧ* (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.

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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

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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 Wodtko 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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