

## Celtic origin: location in time and space? Reconsidering the “East-West Celtic” debate

The article constitutes primarily a review of recent publications concerning the problem of the Celtic homeland. Until recently, the classic ‘Central European’ model used to locate it near the High Danube (according to Herodotus) and linked the Celtic linguistic and cultural unity with the so-called Hallstatt archaeological culture. However, new discoveries in South Portugal opened a series of long debates, spearheaded by such specialists as the well-known linguist, Prof. John Koch, who allegedly identified Tartessian funeral inscriptions as Celtic, and the archeologist Barry Cunliffe, who suggested that ‘Celtic’ evolved in the Atlantic Zone during the Bronze Age, where it arrived from the East by the Mediterranean. This theory was harshly criticized by other Celtologists, but the matter remains unsettled. Other historical models, distinct from both the ‘Central European’ and the ‘Atlantic’ theory, are also possible, including an ‘Eastern homeland’ hypothesis that is proposed here by the author. In addition, the article also includes the text of “Celtic from the West?”, an address by Prof. Wolfgang Meid, provided specially for the *Journal of Language Relationship*.

*Keywords:* migrations, Celts, Iberians, Tartessian language, archaeology, genetics, Celtic homeland.

In September 2014, I happened to take part in two conferences. One was the 7<sup>th</sup> *Colloquium of Societas Celto-Slavica* held by the University of Bangor, Wales; the other was *Kelten, Römer, Griechen — Sprach- und Kulturkontakte im Römischen Reich und seinem Umfeld* at the Universität Heidelberg. The former was opened with a presentation by John Koch called “Celtic from the West”; the latter — with “Celtic from the West?” by Wolfgang Meid. This seems very symbolic of present-day Celtic studies, where the somewhat controversial and vexing issue of the original Celtic homeland has become the subject of numerous debates.

For a long time, it had been accepted that a kind of Celtic linguistic and cultural unity emerged about mid-2<sup>nd</sup> millennium BC in a region north of the Alps. Its emergence has been linked to a certain community’s abandoning the Indo-European tradition of mound burials and adopting instead the so-called Urnfield Culture. “Pre-historians with a very different viewpoint call these Urnfield people the very first Celts” [Herm 1976: 104].

However, according to new archeological data, the real situation was significantly different. Most likely, the culture that had emerged at that time and then later, by the 7<sup>th</sup> century BC, gave rise to the Hallstatt culture, and still later, to the La Tène ornamental style, should rather be defined as a ‘new Proto-Celtic group’ that only arose after the merger of the original Urnfield people and the mound-builders, when their different cultural traits became fused. Glottochronological data suggests that Celtic diverged from Proto-Indo-European quite early, about 3500 BC; for such a remote past, defining a specific ‘homeland’ as a shared linguistic, social and cultural space, let alone locating it geographically, seems to be of little use. As a certain vague starting point of the Proto-Celts’ origin and expansion, East Europe has been tentatively proposed, although it was thought that the precise location of this point either in time or

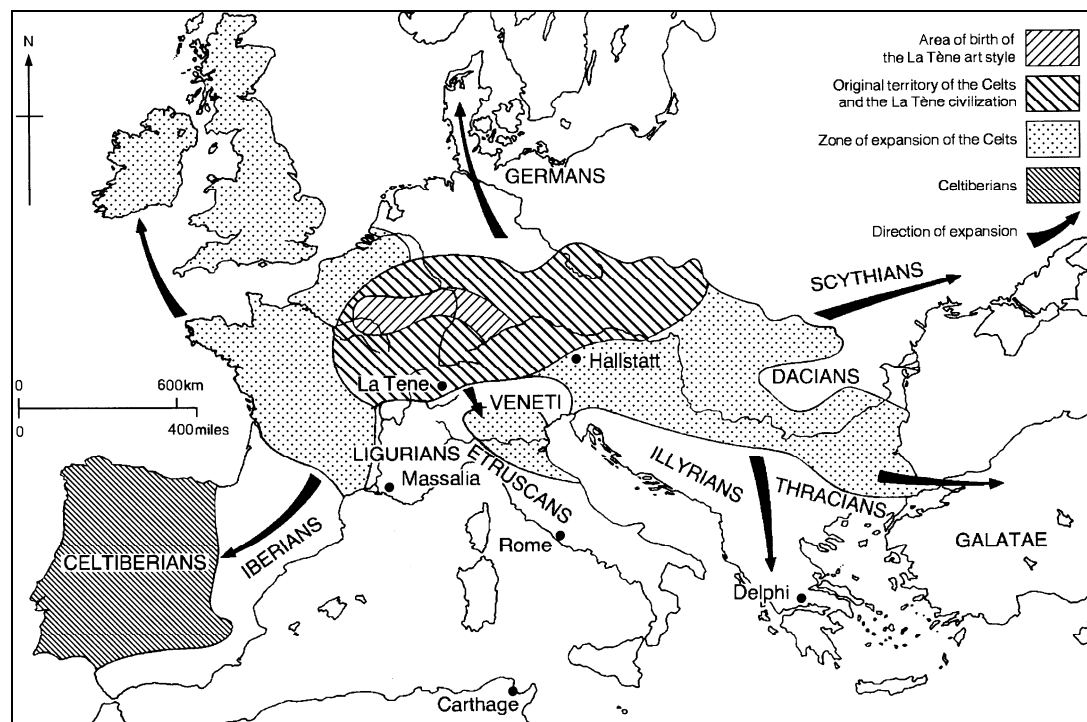


Figure 1. The territories occupied by Celts from the 5<sup>th</sup> century BC until the Roman conquest (adapted from [Megaw and Megaw 1989]).

in space was impossible to identify [Sims-Williams 1998]. Presumably, this ‘point’ must have been what is called ‘second homeland’, whose location remains controversial. As J. Collis wrote in his resumptive work on the subject, “We could recall that we have the three conflicting theories on the location of the Celts in the fifth century, what I have labeled (working from west to east) ‘interpretation 1’ (central and northern France); ‘interpretation 2’ (northern Gaul and southern Germany, which has been adopted by the majority of modern authors); and ‘interpretation 3’ (northern France, southern Germany and Bohemia)” [Collis 2010: 117] The only indisputable fact is the presence of peoples, later identified by Graeco-Roman authors as Celts, in the 7<sup>th</sup> century BC in Southeast Europe, namely, South Germany, West Bohemia, France, and Belgium. It was assumed by default that the Celts had occupied these areas even earlier, of which basically no evidence survived, and it was from there that they spread into Western and Southwestern regions, as well as to the East (Asia Minor). The map in [Megaw and Megaw 1989], reproduced here as Fig. 1, is a definitive outline of both the ‘second homeland’ of the Celts and their later migrations.

Still, the origins of these peoples and the pathways of their earlier expansion “are to a great extent hidden in the time-depths of prehistory” [Meid 2010: 11]. Naturally, the area of the Celtic expansion (including the earlier periods) would comprise both the British Isles and the Iberian Peninsula, but whence, where exactly and when the Celts arrived there (either by land or by sea) remains a matter of sheer speculation.

The story truly begins with Tartessian. For the longest time, this language, attested in about a hundred epitaphs and some personal names from the southwest of the Iberian Peninsula, had been thought to belong to a tentatively reconstructed group of Mediterranean languages. As B. Cunliffe wrote on this subject back in 1997, “Indigenous non-Indo-European languages survived into classical times in the south-east, as Iberian, and possibly in Tartessus, while in the north Basque is also the remnant of a separate and once more-extensive non-Indo-

European group. On this basis it is argued that Celtiberian was intrusive, arriving some time before the earliest mention of the Celts in Iberia in the sixth century” [Cunliffe 1997: 24].

Even such an enthusiastic linguist as H. Sverdrup was apparently wary enough to write about Tartessian: “Before studying the transcriptions closer, we can observe that the set of inscriptions are in the same language. Crosschecks of the reoccurring parts of vocabulary confirm this. It is equally certain that this language is significantly different from all other Indo-European or other Mediterranean languages” [Sverdrup & Guardans 2002: 130].

However, it must be pointed out that the possibility of identifying Tartessian with a dead-end branch of Indo-European languages was proposed by the archaeologist A. Tovar [Tovar 1961; 1986] and then supported by the historian H. Birkhan, who relied on Graeco-Roman records [Birkhan 1997: 152]. Thus, Herodotus in his *Histories* (I.163) mentions the name of a Tartessian king, *Arganthonios*, clearly related to the IE nomination for ‘silver’. The same Herodotus (II.33) refers to the Celts as inhabiting the land near the source of Istros (Danube) which is located by him at the Pillars of Hercules, that is, in the southwest of the peninsula. Moreover, Pliny (*Naturalis historia*, 3.13) mentions a people called ‘Celtici’, also inhabiting the southwest of the Iberian peninsula. This led to the idea that this language bore IE traits, some of which were seemingly recognized as Celtic [Villar 2004: 65], although a general ‘Indo-European’ affiliation would be more likely, since the language could have been influenced by early contacts with both Celtic and Italic speakers. As Villar himself wrote back in 1990: “Même si on laisse de côté tout ce qu’il puisse y avoir de symbolique de la part des historiens grecs pour appeler “Argantonios” le roi du pays mythique de l’argent, et on accepte qu’en fait le nom de ce roi aurait été vraiment indo-européen, ça nous permettrait tout au plus de supposer qu’il se serait produit une certaine pénétration indo-européenne dans le royaume de Tartessos qui aurait peut-être constitué une espèce d’aristocratie dominatrice, comme il s’est passé dans d’autres endroits” [Villar 1990: 376].

J. Koch can be doubtlessly given credit for publishing the inscriptions, complete with accurate drawings, and for the discovery of even more texts in the storerooms of Portuguese museums. Yet his conclusions might be viewed as somewhat far-fetched. In arguing for the Celtic origin of Tartessian, he relies on the famous above-mentioned claim of Herodotus, according to which the Celts lived by the source of the Danube, near the Pillars of Hercules. This obviously self-contradictory statement may at best contain a half-truth: either the Celts lived by the source of the Danube *or* by the Pillars of Hercules. In any case, locating the source of the Danube on the Iberian Peninsula is obviously wrong. The academic tradition has held so far that the linking of the Celts with the Danube was correct, while placing them by the Pillars of Hercules was erroneous. Koch, however, suggests an inverse reading of Herodotus’ claim, seeing it as evidence for the presence of some Celtic people on the south of the Iberian Peninsula as early as the 5th century BC — and for a rather widely shared knowledge of this fact.

The linguistic arguments for identifying Tartessian as Celtic, given in his book, are biased<sup>1</sup>, and his readings of the inscriptions, along with attempts at translation, seem to be far-fetched. Moreover, the very composition of the inscriptions, going in spirals around the tombstone, does not allow to see clearly in what direction they are to be read, and things are made even worse by the absence of word dividers. It should probably be concluded that up to the present day, Tartessian remains a linguistic mystery.

In 2011, the monograph was reviewed in the authoritative journal *Zeitschrift für celtische Philologie* by the well-known historian and archaeologist Michael Koch<sup>2</sup> (Europäische Akademie

<sup>1</sup> For details, see my earlier review [Mikhailova 2010] and the present work below.

<sup>2</sup> It can hardly be a coincidence that the editor-in-chief, S. Zimmer, entrusted the review of the book to a specialist whose name is spelled the exact same way as the author’s!

Otzenhausen), whose conclusion seems to leave little room for any further development of this theory: “John T. Kochs Beitrag bedeutet eine Anregung, keinesfalls eine Problemlösung. Er enthält eine Fülle methodischer Unzulänglichkeiten im Hinblick auf die Interpretation antiker Schriftquellen und archäologischer Befunde. Seine Hauptthese verläßt allzu oft das sichere Terrain des wissenschaftlich Beweisbaren in Richtung auf Hypothetisches. Die Zukunft wird erweisen, ob Kochs Richtung stimmt — oder auch nicht“ [Koch 2011: 259]<sup>3</sup>.

Why is a proof of the Celtic affiliation of Tartessian so crucial for solving the broader problem of the Celtic homeland? The dating of the inscriptions (8<sup>th</sup> to 7<sup>th</sup> centuries BC, which seems correct) would inevitably break the link between the Proto-Celtic unity and the archaeological cultures of Hallstatt and La Tène, which the Central-European theory has generally accepted to have been the basis for shaping it. Theoretically, the suggested scenario of a Celtic migration from Iberia, followed by their further settlement in Central Europe where they would form a unity with a shared language and archaeological culture, is not impossible. Yet in practice it seems quite bizarre, leaving without answer the primary question: when, how, and whence could the Proto-Celtic tribes have arrived at the Iberian Peninsula itself?

Despite harsh criticism of his Tartessian theory, J. Koch won over to his side a noted archaeologist, B. Cunliffe; in his no less controversial book [Cunliffe 2001], the latter proposed an “Atlantic” theory of the Celts’ origin. According to his bold assumption, some waves of Indo-European tribes settled on the Iberian Peninsula, where they shaped a true linguistic unity and from where they subsequently would migrate to both the British Isles and the European Continent. In 2009, the two initiated a one-day conference, proceedings of which were published in the collective volume *Celtic from the West*. As the editors write in the preface, “This book and the conference on which it is based, came about because one archaeologist and one linguist, following separate lines of inquiry, arrived at the same novel hypothesis: **Celtic probably evolved in the Atlantic Zone during the Bronze Age**”<sup>4</sup> [Cunliffe, Koch 2010: 1].

In 2012, the book was republished in a paperback edition and nominated for 2012 Book of the Year by *Current Archaeology*, for which there are indeed some grounds: even the severest critics of the Atlantic theory cannot but admit the high quality of print and paper, the beauty of illustrations showing Tartessian tombstones and the perfect design of maps in full colour (see, for instance, [Falileyev 2012: 170]).

The book has three parts, corresponding to what may be called the three foundations of the present-day ethnological reconstructions: Archaeology, Genetics, Language and Literature.

The first part opens with an article by B. Cunliffe, “Celtization from the West: The Contribution of Archaeology” (pp. 13–38). As early as 1997, he had proposed a theory that the Iberian Peninsula had been populated by migrants from Central Europe via the Pyrenees, writing on this subject: “If we are correct in assuming that the Celtic language crystallized among the Late Bronze Age Urnfield communities of Middle Europe, the extensive links which must have existed with Atlantic Europe at this time would have provided a context for Celtic to have been widely adopted as a *lingua franca*, accompanying the flow of goods and technological information. In this way the language may have spread through the Pyrenees to northern Iberia to emerge as Celtiberian, and through the Alpine passes to the Italian Lakes where Lepontic was to develop” [Cunliffe 1997a: 27]. In the beginning of his article, B. Cunliffe gives a brief review of these dated ‘mainstream’ theories and poses the question: “But what of the supposed movements to Atlantic Europe?” (p. 13). Arguing for the Atlantic origin of the Celts, he uses (instead of archaeological data, which, as he correctly notes, rather represents a type of

<sup>3</sup> See also some harsh criticism in a review of the enlarged edition (Koch 2011) by J. Eska (2013).

<sup>4</sup> Bold font as in the original.

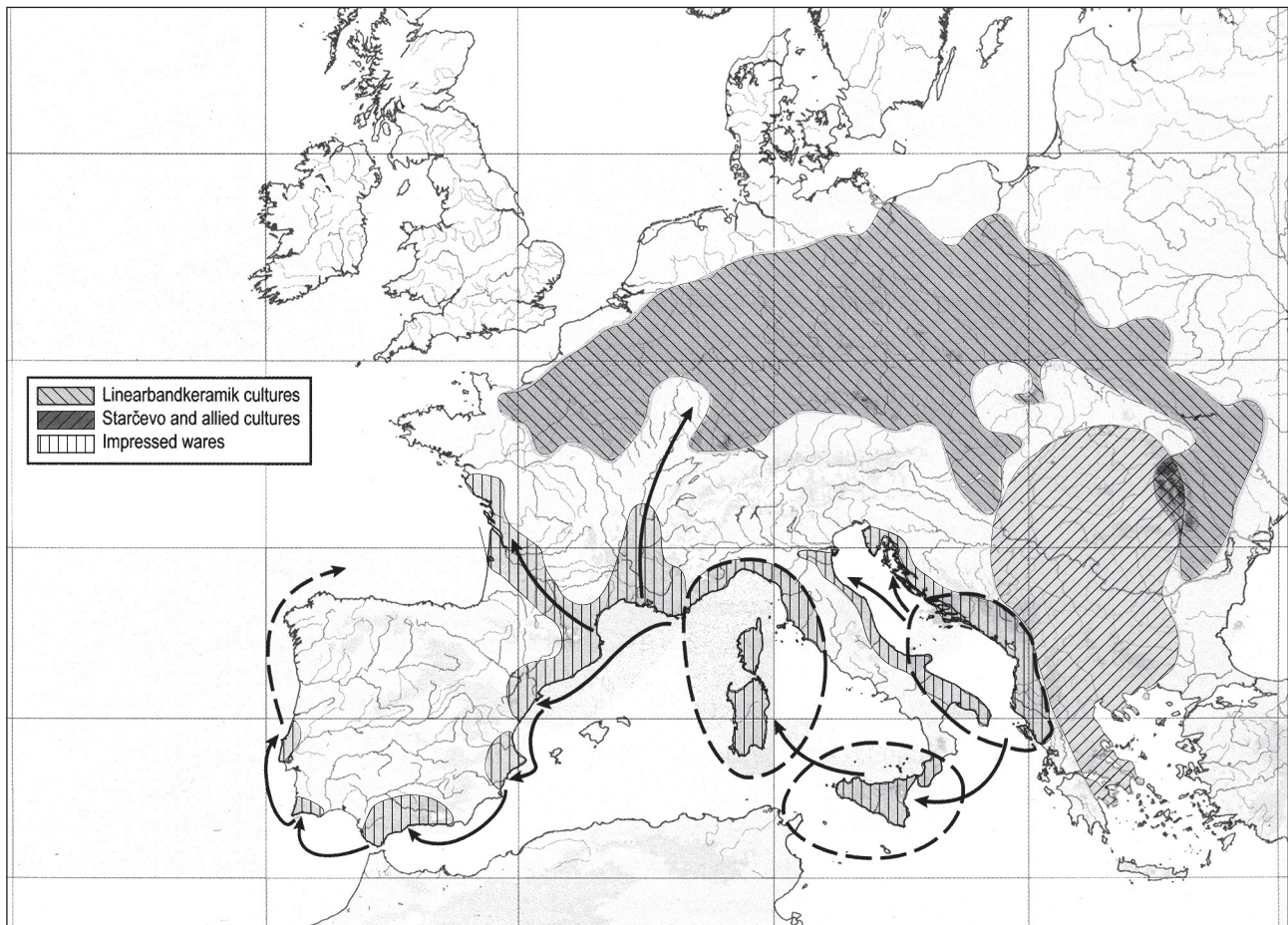


Figure 2. Enclave colonization. Europe in the period c. 5500–4100 BC, showing two principal routes by which the Neolithic way of life spread through Europe from the southern Balkans (adapted from [Cunliffe, Koch 2010: 23]).

material culture than ethnic identity) the early place names. He relies on the work by Sims-Williams [2006] who, using over 20 thousand place names from classical antiquity, created a map demonstrating the highest density of Celtic place names in Western France, Southern Britain, western parts of the Iberian Peninsula and Northern Scotland. In comparison, Central and East Europe look quite pale on it, and lands east of the Balkans are mere blank areas, which, as Cunliffe himself justly notes, only means that these lands did not get a proper description from Graeco-Roman historians and geographers. The map, like many other maps in the edition under review, is beautiful, yet not very informative. Indeed, dark brown areas in the north and west of France might simply indicate a strong Graeco-Roman interest in these lands rather than the actual distribution of Celtic toponymy. It is hardly a coincidence that West Ireland is left pale green — there is simply no information on this area. Turning to the earliest Graeco-Roman references to the Celts (in Herodotus and Hecataeus, 5<sup>th</sup> century BC), the author, who seems to have forgotten about the ‘source of Istros’, inscribes mercilessly above the red line north of the Alps and the Balkans: UNKNOWN. However, if Greeks knew too little about these areas around the 5<sup>th</sup> — 4<sup>th</sup> century BC, this does not necessarily mean that there were no Celts there; moreover, the author inexplicably omits any mention of the famous *Massaliote Periplus* (5<sup>th</sup> century BC) where the names of Ireland (Ierne) and Britain (Albion) are first attested.

Skippping over a detailed analysis of Cunliffe’s arguments (although it is worth mentioning once more that they are based not on actual archaeological data, but rather on a recon-



struction of possible ways of Proto-Celtic migrations — see Fig. 2), we can summarize his principal conclusion as follows: the Celts (or, rather, their ancestors) split off from the IE unity as early as 5,000 BC, and “there is ample evidence of the small-scale exchange of commodities among coastal hunter-gatherer communities. These early networks, conditioned by the need to maintain systems of social interdependence, dominate the next five millennia of prehistory” (p. 22).

According to Cunliffe, the Celtic ‘origin’ is connected with “the earliest form of Bell Beaker — the so-called Maritime Bell Beaker — probably originated in the vibrant copper-using communities of the **Tagus estuary** around 2800–2700 BC and spread from there to many parts of western Europe” (p. 27). In other words, he suggests that the Celts as a unity emerged in Spain, and that was the starting point of their further journey to both the West (the British Isles) and the Northeast via the Pyrenees, from where, one would still hope, they did not fail to get to the source of the Danube and in the 7<sup>th</sup> century BC founded their Hallstatt realm. Indeed, the conventional placing of the Celtic Urheimat in Central Europe nowadays hardly looks justified. Yet the problem is time rather than location. If one accepts the data of glottochronology, which indicates that the Celtic split into *p*- and *q*-branches began about 1200 BC, and, on the other hand, considers the widely held opinion that Celtization of the British Isles may have begun as early as 1500 BC, locating the Celtic ‘origin’ in Central Europe and, even more notably, linking it to the Hallstatt culture becomes void of any logic or sense. The dates suggested by B. Cunliffe seem to be more prospective. Yet it is still unclear why it is precisely Spain that should be considered the Homeland. How, after all, did the Celts get there? To this question the work provides a fairly clear answer illustrated by fine maps: the Proto-Celtic tribes would have come to the Iberian Peninsula by sea, via the Mediterranean, founding temporary colonies on Sicily, Sardinia and Corsica. But if so, why did not they leave any traces there?

The lingering questions are indeed too numerous, and the author is well aware of this fact. At the end of his work, he boldly lists a number of unsolved problems and challenging contradictions, starting with the question of “is it possible that the Indo-European languages reached the Atlantic Zone c. 5000 BC?” (p. 34). Yet the work concludes with an optimistic statement: “By asking this torrent of questions I hope we may begin to open a new debate about the origins of the Celtic language and of the Celts” (p. 35).

The alleged debate is opened within the very same volume, with an article that bears a provocative title: “The Celts from everywhere and nowhere: a re-evaluation of the origins of the Celts and the emergence of Celtic cultures” (pp. 39–64), by a renowned archaeologist, Raimund Karl. From the outset, the author demonstrates the flawed character of the mainstream ‘Central European’ theory of the Celtic Urheimat, but then finds the Cunliffe-Koch Atlantic theory equally unconvincing and also based on far-fetched juxtaposition of archaeological, genetic and linguistic data. An archaeological culture, he supposes, does not necessarily have to be linked to a specific ethnic community with a shared language, and the Indo-European diffusion across Europe must have inevitably involved genetic changes. Besides, it is not clear whether the Atlantic peoples who were spreading north and east would identify themselves as Celtic and whether they spoke a single Proto-Celtic dialect or several dialects that evolved simultaneously. The author gives a definition of “Celtic” which became widely known: “A Celt is someone who either speaks a Celtic language or produces or uses Celtic art or archaeology or has been referred to as one in written records or has identified himself or been identified by others as such” (p. 45).

His conclusion: “Indeed, not only is the question where and when ‘the Celts’ came into being impossible to answer, it is, ultimately, also perfectly meaningless” (p. 62). One may as-

sume, then, that the article was included in the volume by the editors quite deliberately, so that these harsh statements could serve as an advantageous background for their own academic archaeological and linguistic reconstructions.

The “Archaeology” part is concluded by the paper of the Lisbon archaeologist Amílcar Guerra “Newly discovered inscriptions from the South-West of the Iberian Peninsula” (pp. 65–78). In his review, Falileyev suggests that this text should have been placed in the “Linguistics” part [Falileyev 2012: 170], yet there is little reason to accept his point of view. The main subject of this compact work is the history of an important finding, the stele from the site of Mesas do Castelinho, bearing the longest of the extant Tartessian inscriptions (a total of 82 characters). It is not only its length that is unique: “the third to the last symbol in the final lane is represented by a character of unique shape. Like other unique signs occurring elsewhere in the Tartessian corpus, the one here raises several unresolvable questions” (p. 72). As demonstrated by Guerra, the inscription cannot be reliably deciphered and interpreted, at least not completely. However, he does attempt a hypothetical transliteration (as stated in the footnote, coordinated with J. Koch) and even manages to find some regular formulas in it, such as **ba<sup>a</sup>re na<sup>a</sup>fk<sup>a</sup>**], for which he finds many parallels, yet has difficulties with interpreting it. Notably, in Koch’s book *Tartessian: Celtic in the South-west at the Dawn of History* [Koch 2009] the formula is interpreted as Celtic, with a proposed reading ‘I have received lies dead’ (i. e., the grave itself), yet Guerra never refers to this book in this article.

However, there are some attempts at linguistic generalization in Guerra’s work. Thus, comparing the forms **ba<sup>a</sup>ne<sup>5</sup>** and **ba<sup>a</sup>nti<sup>i</sup>**, he infers that these may have been verbs in 3 sg. and 3 pl. respectively, which allows him to conclude: “Most likely, as many investigators have already pointed out, we have in these series verb forms in **-ntii**, preserving the Indo-European third person plural active ending, also found in Tartessian **lak.enti<sup>i</sup>**” (p. 74). The author’s conclusion is reasonably cautious: “it is expected that ongoing project aimed at a systematic exploration in areas with the greatest potential will continue to provide results in several key areas: the knowledge of the writing system and the language that it embodies, its archaeological context, and its chronological and cultural associations” (p. 78). In other words, it is not yet time for pleasure — there is too much business to be done.

I must admit that the “Genetics” part remains somewhat obscure for me, due to my lack of special knowledge on the subject. However, one may make some conclusions drawn mostly from the authors’ own summaries of their observations<sup>7</sup>.

The work by Ellen C. Røyrvik (“Western Celts? A Genetic impression of Britain in Atlantic Europe”, pp. 83–106) is of a generalizing and introductive nature. As she writes, “the application of population genetics to anthropology and archaeology has had, and in truth is still having, numerous teething problems. These can broadly be divided into two camps: the issues of adequate sample sizes and sampling locations, and the problem of creating useful and rigorous definitions for the field” (p. 85). Moreover, genetic mutations, as it has been noted, may be expressed within a single family lineage, without detectable influence upon ethnic variation. As an example, Røyrvik investigates the gene responsible for the variety of pigmentation. She writes, for instance, that “the black-haired citizens with brown eyes are traditionally more likely to be found in Rome than in Uppsala” (p. 89). Yet at the same time the widely held notion of a red-headed Celt turns out to be a myth: the red hair gene originates from Scandinavia

<sup>5</sup> According to J. Koch, **ba<sup>a</sup>ne** — ‘for the woman’, dat. sg. [Koch 2009: 100].

<sup>6</sup> According to J. Koch, ‘they lie down’ [Koch 2009: 72].

<sup>7</sup> It is interesting to note that the next volume (Celtic from the West — 2) has no publications on genetic issues at all.

and also occurs in England, perhaps even more often than in Ireland (the distribution map is given in the article). One is tempted to ask then: what is the case with dark-haired Icelanders? Besides, the people in question are present-day Europeans; we cannot know for sure whether Romans of the classical era were dark-haired and dark-eyed. Geneticists probably have their answers to these questions, yet my point is that both Icelanders and Romans have retained their languages and ethnic identities. It is an accepted view that the best information is yielded by the distribution of the Y-chromosome, and here Røyrvik again notices the presence of its Scandinavian variety in today's Britain, especially on the Orkneys. But this fact gives little support to the Atlantic theory of the Celts' origin. In tackling this question, she apparently feels at a loss.

The title of the final section of her article is revealing: "Celts — Atlantic, Central, or Both?" (p.96). After a series of comments on the multiple difficulties encountered while researching the mechanisms of genetic mutations, she is still able to make a cautious inference: "The Atlantic scenario of Celtic speech and cultural identity associated with it developing largely in situ on the western seaboard is, from the genetic point of view, a less clear-cut problem than the Central Celtic one. The timing is less restricted, the areas are less well-defined, and written records for any related events are completely lacking" (p. 98). Presumably, the author avoids jumping to conclusions and only points at the lack of sufficient data for generalization. As for research on genetic data from Britain, more familiar to her, her conclusions are more positive: the Britons had likely come from north France. "For the Atlantic scenario, a source area like that above is a less appropriate concept" (p. 101).

The article "Irish Genetics and Celts" (pp. 107–120) by two Irish geneticists, Brian P. McEvoy and Daniel G. Bradley, deals primarily with Irish matters, concerning a reconstruction of ethnic changes on the island in the Middle Ages. This paper is of much interest indeed, but has little to do with the issue of the Celtic homeland.

The last article in this part is by a renowned geneticist specializing in the British Isles, Stephen Oppenheimer: "A Reanalysis of Multiple Prehistoric Immigrations to Britain and Ireland Aimed at Identifying the Celtic Contributions" (pp. 121–150). Several years ago I had read, with great interest, a popular book by B. Sykes, "Blood of the Isles", whose author makes a conclusion that should be pleasing for anyone in the field of Celtic studies: "Overall, the genetic structure of the Isles is stubbornly Celtic, if by that we mean descent from people who were here before the Romans and who spoke a Celtic language" [Sykes 2006: 338]. However, I did not get any answer to the question where the Celts had come from and whether genes in the population of the British Isles are related to those of France or Spain. Moreover, a direct answer to this question is absent in a more academic work by S. Oppenheimer himself, "The Origins of the British" [Oppenheimer 2007]. Facing the challenge of finally coming up with a direct answer, the author openly admits that "this may be an impossible task, since genes do not carry ethnic or linguistic labels and there is no agreement as to who the Celts were or where they came from or at what time" (p. 147).

On the whole, the best we can do is to cite A. Falileev, who in his review feels that "reading the 'Genetics' part of the book, one is under impression that the works in it not only fail to provide any fresh support for the 'Atlantic' theory of the Celts' origin, but are unable to help solving this problem in principle" [Falileev 2012: 172].

The "Language and Literature" section opens with an article by G. R. Isaac: "The Origins of the Celtic Languages: Language Spread from East to West" (pp. 152–167). This compact work summarizes the author's ideas proposed earlier in [Isaac 2004; 2007] that basically boil down (as the title indicates) to the Eastern theory of Celtic origin. Moreover, in his approach to linguistic reconstruction the author rejects conventional lexical analysis, since "speakers of



languages converse with neighbouring speakers of other languages in many social contexts. They exchange objects, ideas, practices and thus words. So for considerable time in their history and prehistory, Celtic, Germanic and Italic were neighbours” (p. 158). He assumes that it is only shared and parallel grammatical innovations that “carry weight”. In other words, only grammar, in his view, can be “that we call the diagnostic features of prehistoric relations” (ibid.). As a supporting example, Isaac uses the presence of the relative pronoun form that Celtic shares with Greek, Slavic, Indo-Iranian and Phrygian (Proto-I-E. *\*ios*, *\*ieh<sub>2</sub>*, *\*iod*). Another of his cases is the ‘Italo-Celtic’ theory which also demonstrates a sequence of shared grammatical and phonetic innovations (such as *\*penk<sup>w</sup>e* > *\*k<sup>w</sup>enk<sup>w</sup>e*). Largely relying on the theory by K. H. Schmidt [Schmidt 1996], G. Isaac infers that “in summary, the innovatory morphological characteristics of the Celtic languages place them in their earliest accessible phase of becoming what they are in a dialectal complex that includes also the ancestors of Indo-Iranian, Greek, Baltic and Slavic” (p. 164).

As a possible date for the disintegration of this East European linguistic community, the author suggests the time period around 2000 BC, but when it comes to specifying an approximate location, his conclusion is more cautious: “Linguistic questions can only be given linguistic answers. And those answers are in eastern Europe” (p. 166). He definitely gives up any attempts at linking the reconstructed community with any of the known archaeological cultures from that time and area.

David N. Parsons’ work “Tracking the Course of the Savage Tongue Place-names and Linguistic Diffusion in Early Britain” (pp. 170–184) shows the challenges and pitfalls of toponymics, especially where historically important place names are concerned, the fact being that place names tend to change. Yet the author writes: “If there is no certainty as to the core Celtic area, place-name distributions are left floating, begging questions but providing precious few answers. And the problem of time-depth looms large. The enormous geographical spread of the Celtic language-group is largely securely attested by the later first millennium BC. How long could it have been there? If 500 years or more is a realistic suggestion, then we must question the chronological relevance of any patterns that we think in the distributions” (p. 182). If I understand this properly, the author assumes that any toponym attested over a period of 1,500 or 2,000 years may be considered a reliable argument for localizing a particular ethnic group in a given area. But is this really the case? In general, the article tackles complicated subjects which go far beyond linguistic reconstruction as such and are situated, so to speak, at the crossroads of history. But what precisely does the subject of *Urheimat* have to do with it?

As ‘ancillary studies’, the section also includes Philip Freeman’s article “Ancient References to Tartessos” (pp. 303–334) and Dagmar Wodtke’s “The Problem of Lusitanian” (pp. 335–367); the author does not support the idea that Lusitanian is a dead-end branch of Iberian IE languages, explaining the presence of IE traits in Lusitanian by prolonged contact with Celtiberians.

Substantially, most of Section III is comprised of John Koch’s work “Paradigm Shift? Interpreting Tartessian as Celtic” (pp. 185–301). This is essentially a slightly amended version of his 2009 book: the pictures are now in colour, and an enlarged preface is added, whose contents are more of a historical and theoretical nature rather than presenting specific arguments for reconstructing Tartessian as Celtic.

Notably, the section bears the title of “Language and Literature”, yet there is hardly any talk of literature at all. However, this subject would actually be worth discussing: for instance, as reconstructed by B. Cunliffe, the sea voyage of the Celts towards Spain and then to the British Isles strikingly resembles the Medieval Irish story of the Gaedil journey from Egypt to Spain across the Mediterranean and of the conquest of Ireland by the sons of Mil (see “The

Book of the Taking of Ireland”, Part II [Macalister 1939]). Perhaps the authors found it too embarrassing to refer to this kind of literature.

This time, there has been some feedback as well: thus, *Zeitschrift für celtische Philologie* (Bd. 60) published a somewhat critical, but mostly positive review whose author, H. Birkhan, wrote in the conclusion: “Alles in allem kann man den Herausgebern zu diesem Sammelband, der in der Keltologie vermutlich tiefe Spuren hinterlassen wird, nur gratulieren” [Birkhan 2013: 296]. The same year, the renowned archaeologist and Celtic/Indo-European scholar James Mallory wrote melancholically in his book “The Origins of the Irish”: “John Koch has recently argued that we should now include the language of southwest Iberia, Tartessian, in the Hispano-Celtic group, which would push the dates for this branch back to the VIII century BC” [Mallory 2013: 252].

The second conference on this subject was held in summer 2010 at Oxford, and its proceedings were published in the next collection, whose main topic, as John Koch writes in his ‘Prologue’, “is epitomized in the title and subtitle: *Celtic from the West 2: Rethinking the Bronze Age and the Arrival of Indo-European in Atlantic Europe*” [Koch, Cunliffe 2013: 1]. The very title of the Prologue sounds stridently and provocatively: Ha C1a ≠ PC (The Earliest Hallstatt Iron Age cannot equal Proto-Celtic). Generally, if we are allowed to skip ahead, one could hardly disagree with Koch on this point. However, his argument is still based on his interpretation of Tartessian, seemingly ignoring the issue of the British Isles whose archaeological data apparently does not fit into the Halstatt model. He makes a reference to the theory of G. Mac Eoin [Mac Eoin 1986] who assumed that Halstatt C sword-bearers “were still seen bringing Celtic to Ireland from west-central Europe”, soundly criticizing it for the inconsistency between the author’s logical inference (“Celtic was not spoken in the Atlantic zone until the Iron Age”) and the linguistic data. At the same time, Koch suggests the term ‘pre-Celtic’, indicating a kind of abstract ethnic unity, and again, in a way he may be right, yet the Prologue never tries to locate this unity either in time or space (although, after all, this is just a ‘prologue’).

The book opens with a chapter by James P. Mallory, “The Indo-Europeanization of Atlantic Europe” (pp. 17–40). The author is well aware that in this case the solution of the problem must not be centred around vague archaeological parallels, but rather that “any attempt to describe the spread of Indo-European languages to Atlantic Europe must involve itself in firm linguistic matters” (p. 17). Yet the inclusion of any new languages into the Celtic family would automatically entail a large-scale rewriting of the whole ‘Celtic Tree’. What are the challenges (and the consequences) of adding a new language? Mallory states that, if we accept J. Koch’s and B. Cunliffe’s theory that the Celtic homeland was originally Spain and that Tartessian was the earliest Celtic language attested, we should expect to find, in accordance with the general laws of language dissemination, a range of related dialects in the area of the supposed homeland (let us call them Hispano-Celtic). In this light, it would be natural to suggest the Celtic character of Lusitanian, whose Indo-European affiliation is now accepted by nearly everybody. But despite the scarcity of the Lusitanian data, “there is sufficient evidence to agree that Lusitanian does not behave like a Celtic language in its retention of PIE *\*p*, e. g., PORCOM ‘pig’” (p. 19). It is worth reminding that for Tartessian J. Koch postulates the loss of PIE *\*p* (e. g. **or-** < IE *\*super*, **ro** < IE *\*pro* etc. [Koch 2009: 197]).

Mallory also draws attention to another issue, overlooked in Koch’s work and concerning the interrelationship of the Celtic languages. Assuming that Tartessian was not a *p*-Celtic language (e. g. **eku-** in a Tartessian name, interpreted by Koch as ‘horse’), Koch fails to take into consideration another distinction drawn between Celtic languages, that is, Continental vs. Insular Celtic. Presently, there are two models:

- 1) step 1: *p-* ~ *c-* Celtic; step 2: *p*-Celtic splits into Gaulish-Leponitic and Brittonic; *c*-Celtic yields Goidelic and Celtiberian;
- 2) step 1: Insular ~ Continental Celtic; step 2: Insular Celtic splits into Goidelic and Brittonic; Continental Celtic yields Gaulish-Leponitic and Celtiberian.

Again, I will remind that Koch himself once used to be an ardent proponent of the Insular Celtic theory [Koch 1992]. According to this model, “both Britain and Ireland experienced fundamentally similar linguistic histories (from the same source?) and the two sub-branches had not differed significantly until the 1<sup>st</sup> century AD. The Insular Celtic model thus suggests that both islands received their Celtic languages either from a common Continental source or that this single source carried the ancestor of the Insular Celtic languages first to Britain and then on to Ireland at about the same time” (p. 20). Without sharing this point of view, the author once more lays out its key points, in order to illustrate the poor logic according to which the ancestor of the so-called ‘Insular Celtic’, which would also give rise to daughter-languages of the Continental Celtic kind, should be localized in Spain.

Another challenge which follows directly from Mallory’s reasoning is the issue of Italo-Celtic. He admits that although the theory of Italo-Celtic unity had been once placed under doubt by Calvert Watkins [Watkins 1966: 50], many specialists are presently reconsidering the idea, or at least accepting that a *Sprachbund* between Celtic and Italic may have existed at some time (see most recently [Eskola 2010]). Apparently, the ‘out of Spain’ theory cannot explain any Italo-Celtic parallels, and if isolated traits of grammar can indeed be interpreted as independent innovations, the presence of culturally significant shared vocabulary not only indicates, in Mallory’s opinion, strong genetic or areal ties, but also allows to approximately date the period during which these language branches co-existed. According to him, “in short, acceptance of Italo-Celtic appears to place the staging area for the Indo-Europeanization of Atlantic Europe in Alpine Europe rather than the Mediterranean and at some period after c.3500 BC rather than before it. It renders highly unlikely any attempt to associate the linguistic ancestors of the Celts or Lusitanians with the Corded Ware horizon. In sum, it is very difficult to see any attraction in associating the Indo-Europeanization of Atlantic Europe with the westward spread of the Neolithic economy through the Mediterranean” (p. 26).

One could simply stop at that, but Mallory recalls yet another aspect of the problem, totally overlooked by J. Koch: a series of lexical isoglosses that are shared between Celtic and Germanic and Balto-Slavic (North-West, Indo-European). “The NW lexicon shares items for flora, fauna” (p. 31), but there are few cognates for architecture and technology. So, according to Mallory, “the material culture for the North-West Group hardly provides definitive evidence as to its date or location although it is probably most consonant with a Bronze Age date” (p. 32). Yet, unfortunately, the author is unable to completely abandon the archaeological part of the issue of the Celtic homeland and ways of migration. His linguistic models, built upon pure logic, seem perfect and totally devastating for the Atlantic theory, but his shift towards a discussion of the distribution of the Beaker culture inevitably makes his reasoning weaker: “The reconstructed lexicon of IE languages, including those of the NW, suggests that Indo-Europeanization of Atlantic Europe should not have predated to any appreciable extent the appearance of the plough, wheeled vehicles, horses, metals, and wool in the cultures of Atlantic Europe. Archaeologically, the cultural evidence suggests a date of Indo-Europeanization no earlier than the late Neolithic or Beaker period or more recently. The initial Indo-Europeanization of Atlantic Europe is more likely to derive from a north Alpine linguistic dispersal than one that crossed the Mediterranean. In support of this model are the contacts between Celtic and Italic and other languages of the NW group. Against the Mediterranean hy-

pothesis is the presence of relict non-IE languages in the central and west Mediterranean that are much more easily associated with the spread of Neolithic communities” (p. 36).

Making use of the problem of the beakers, or of the so-called ‘beaker folk’ for solving problems of ethnic and linguistic history seems inappropriate to me, and I am not alone in sharing this opinion. Thus, A. Falileyev writes: “The Bell Beaker phenomenon presupposes movements not only from the Atlantic fringe to the rest of the Continent, but from other places and in various directions, and also from the Carpathian basin and Middle Danube westwards. Therefore, Celtic, not unlike some other Indo-European languages as well, could be in theory well involved in the phenomenon. A prospect that people who utilized the same pots, adhered to the same ideology and made use of the same mortuary practices spoke different languages does not upset scholars nowadays” [Falileyev 2015: 5].

Subsequent chapters of the collective monograph<sup>8</sup> consider exclusively archaeological issues (in particular, the beakers), which is beyond my current subject of discussion and should probably be reviewed elsewhere.

Against this background, however, we find an unexpectedly impressive chapter written by Dagmar Wodtko: “Models of Language Spread and Language Development in Prehistoric Europe” (pp. 185–206). Strictly speaking, it is only indirectly that it deals with the subject of the Celtic homeland. The most intriguing part of this work, I believe, is the model of language dissemination in multilingual regions, and, relatedly, the role of *lingua franca* that has been much underestimated. As the author suggests, certain sub-ethnic groups of proto-Celtic speakers would have moved westwards in ‘streams’, ‘flowing’ around regions inhabited by pre-Indo-European speakers. But for many social reasons, Celtic speakers would assume more privileged positions than the locals who, due to plain economic necessity, would have to learn Celtic, which would then become a kind of *lingua franca*, and later would become established as the main language of communication in the given region. However, according to Wodtko, this model works for linguistic and tribal migrations in general and may be applied to the expansion of any branch of Indo-European languages: “It is possible, then, that in this way the areas increases in which the Indo-European language is spoken at least as a second language and simultaneously its status as a supra-regional form of communication is enhanced, so that it will attract further bilingual speakers more easily. If this situation holds for several generation, the status of the language may not be reversed, even when a new power factor with a new linguistic background enters the area. The Indo-European language may then simply be accepted as the *lingua franca* for interacting with this region. New foreign speakers, although perhaps not adopting it for themselves, will use it for communication over all of that area. This may be to the detriment of the remaining indigenous languages, whose speakers are thereby only pushed further to abandoning their own form of speech for a more powerful one” (p. 201).

Presumably, this is how Romanization of West Europe was achieved; and this analogy further reaffirms the point of distinguishing between true ethnic migrations, on the one hand, and language spread, on the other. Nevertheless, there is still some doubt as to the legitimacy of the author’s usage of ‘the Indo-European’ as an umbrella term for several Western language

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<sup>8</sup> A.P. Fitzpatrick: “The Arrival of the Bell Beaker Set in Britain and Ireland” (pp. 41–70), Catriona Gibson: “Beakers into Bronze: Tracing connections between Western Iberia and the British Isles 2800–800 BC” (pp. 71–100), John T. Koch: “Out of the Flow and Ebb of the European Bronze Age: Heroes, Tartessos, and Celtic” (pp. 101–146), Dirk Brandherm: “Westward Ho? Sword-Bearers and All the Rest of it...” (pp. 147–156), Jacqueline I. McKinley, Jörn Schuster & Andrew Millard: “Dead-Sea Connections: A Bronze Age and Iron Age Ritual Site on the Isle of Thanet” (pp. 157–184).

branches (according to Wodtko, Proto-Italic, Proto-Germanic and Proto-Celtic); moreover, the author avoids the central issue of the debate, that is, the location of the Celtic homeland, prudently passing on to a discussion of how many extinct languages could have existed in South-West Europe in ancient times.

The monograph ends with what is unusual for West European linguistics but seems quite natural and logical for Russian linguistics — namely, discussion of glottochronological data. Although this method is not highly valued among West European specialists on language history<sup>9</sup>, from time to time, some original calculations (sporadic and mostly oblivious, in one way or another, of works by precursors) do appear in print. Indeed, the variation in results proves to be considerable, both for dates and for the structure of the IE tree itself. In his section, titled “Early Celtic in the West: the Indo-European Context” (pp. 207–217), Colin Renfrew does not suggest his own calculations, fully relying instead upon the data from [Gray and Atkinson 2003], which is in fact a four-page popular article. The table that is reproduced in the monograph (p. 213) dates the divergence of Proto-Celtic to about 4100 BC, and the split of the family to about 900 BC. Generally, these data do not run against the accepted view of Russian and other ‘regular’ glottochronologists (3500 and 1250 respectively, see [Blažek 2007]). However, instead of nitpicking over figures, it would be more reasonable to see what use one can make of this method to test the Atlantic model of Celtic homeland.

Naturally, glottochronological data leaves no place for the old Central European / Hallstatt model, because, as the author writes, “this would indeed imply that the process of differentiation or crystallization of their shared ancestor (Proto-Celtic) was completed during the Bronze, and indeed during the 2<sup>nd</sup> millennium BC” (p. 216). But if the divergence of Proto-Celtic had occurred by 4000 BC, it must be, according to the author, linked to early Neolithic sites in West Europe, and therefore, Atlantic would be the best solution. So the discovery of a new Celtic language (Tartessian), as C. Renfrew states, “sets a new context for the discussion, and presents a challenge to which some traditional linguists may find it difficult to rise” (p. 216).

I find this conclusion highly disputable, but, before making any specific objections, I would like to cede the word to Professor Wolfgang Meid, of whom I wrote above. Where Koch’s own presentation in Bangor was received quite frostily, Meid’s presentation in Heidelberg produced a far more enthusiastic response in the audience, which included, among others, Helmut Birkhan himself. Professor Meid kindly provided the English version of his text for the present publication:

**Wolfgang Meid:** *Celtic from the West?*

The debate on Celtic origins has recently been re-opened with an unorthodox proposition. I am referring to the alternative theory put forward recently by professor Barry Cunliffe, the noted British archaeologist, that Celtic originated as such in the extreme west of Europe, apparently as the language of the megalithic population of the Atlantic fringe. Professor Barry Cunliffe, in his booklet “The Celts: A Very Short Introduction” of 2003, posed a question which he was inclined to answer in the affirmative: “Could it be that, far from being a language introduced by invaders or migrants moving in from central Europe, it was the language of the indigenous Atlantic communities which had developed over the long period of interaction beginning in the fifth millennium BC?” [Cunliffe 2003: 26]. I opposed this view in my Rhys lecture

<sup>9</sup> “One technique, often cited but usually rejected (at least in the field of the Indo-European languages) is glottochronology” [Mallory 2013: 258]. But see: [Kassian, Zhivlov, and Starostin 2015].

of 2007 [Meid 2008], stating that a positive response would ignore the Indo-European origins and connections of Celtic, and that only the very late forms of Celtic could have developed in the British Isles. However, I may have misjudged what was really in Professor Cunliffe's mind because now, in the new publication "Celtic from the West" edited together with John Koch he expressly acknowledges the Indo-European origins of Celtic and also the Hallstatt and Latène set-up. But as he still prefers the view that Celtic first emerged in the west, at the Atlantic coast, apparently extending land-inward into France and further, the question remains, since Celtic is an Indo-European language, where its speakers ultimately came from, if they did not come by land from the east to the extreme west. The obvious answer which is suggesting itself is that they must have come by sea, which can only mean that they should have come from the Mediterranean, perhaps even from its eastern parts. Considering the lack of toponymical or other evidence for Celts or their Indo-European ancestors in that region, that would be a bold assumption. It is hardly likely that the Greeks would not have taken notice of them. But Professor Cunliffe goes even farther back in time, and asks, at the end of his contribution in the volume mentioned, "is it possible that the Indo-European language reached the Atlantic Zone c. 5000 BC as the result of enclave colonization bringing the Neolithic lifestyle from the Mediterranean?", and, "if so, is it possible that *Celtic* began to develop in the Atlantic Zone between 5000 and 3000 BC during the period when extensive connectivity was established along the Atlantic façade?", and further, "if a distinct Celtic language had emerged by the third millennium then could the period of rapid mobility ... have provided the context for the language to spread across much of western Europe?" [Cunliffe and Koch 2010: 34 f.].

These are three questions put to linguists and philologists, the second and third being conditional on the answer to the first: Is it possible that Indo-European speakers arrived at the Atlantic coast by sea as early as 5000 BC? This sequence of hypothetical questions suggests that Professor Cunliffe is not quite so sure himself about these assumptions. As I am concerned, the answer to the main question must be "no", which makes the dependent questions irrelevant. But granting that anything might be possible, we may say that the whole set-up has a very low rate of probability. At 5000 BC we envisage Proto-Indo-European still as a rather compact, if not small, linguistic community somewhere in the border region of eastern Europe and western Asia. The language had not yet spread to central Europe nor to the Balkans or the Mediterranean. It would have had to be brought there by sufficient speakers to have a tangible effect on the original populations. But perhaps Professor Cunliffe has Colin Renfrew's thesis in mind of Indo-European origins in Anatolia, according to which Indo-European would have spread together with the spread of farming since the 9<sup>th</sup> millennium from Anatolia by his "wave of advance" model throughout Europe, reaching the Atlantic — by land — about 5000 BC [Renfrew 1987], but this model which would account for very early Indo-Europeans in the Mediterranean, even in its revised form is also out of the question. It is both the wrong place and the wrong time for Indo-European.<sup>10</sup> The spread of agriculture is too early to be attributed to the Indo-Europeans. It must have been carried by a pre-Indo-European population. The Indo-Europeans at that time were still far away in the Russian steppes. Hittite — the oldest attested Indo-European language in Anatolia — is a newcomer there, its speakers having established themselves there at about 2000 BC, ruling over a thor-

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<sup>10</sup> Discussion and criticism in [Meid 1989].



oughly non-Indo-European-speaking native population. Indo-European did not originate in Anatolia; therefore there is no help from there.

The Indo-Europeans, as an inland population, lived on a cattle-raising economy. They were no sea-farers — they only learnt to navigate when they came to settle near the sea, as in Greece. But even there their preoccupation was cattle; the Homeric heroes did not eat fish, nor did the Irish warriors. In ancient Ireland the cattle-barons had high social status, whereas fishermen had a very low status. What I mean to say is that the Indo-Europeans — and the Celts — had no maritime ideology. So, the maritime network at the Atlantic coast, since 5000 BC, the megalithic culture in evidence there, is carried by a different population, not by Indo-Europeans. The same goes for the inland spread of agriculture from the Near East. This is also due to a pre-Indo-European population.

Actually the whole Mediterranean area is replete with non-Indo-European, that is pre-Indo-European toponymy, and the same is true for part of the Atlantic area: Aquitania, the southern part of Gaul, was originally of non-Indo-European language, as the onomastic show. This language was the ancestor language of the still spoken Basque language in the Pyrenees.

Actually the Celts were not the first Indo-Europeans to reach the Atlantic zone and the British Isles. There were previous waves of speakers of late Indo-European dialects who had spread almost over the whole of Europe including the Mediterranean parts. They are recognized by a quasi uniform name-giving system for water courses (larger as well as smaller rivers) which has been called the Ancient European Hydronymy. Wherever these Indo-European-speaking groups took land and settled down, they gave the many water courses in their environment names in a very simple and similar way, naming them by simple water-words and varying suffixes according to the rules of Indo-European word-formation. This system was discovered by Hans Krahe and applies also to the British Isles, to Italy and parts of the Iberian Peninsula. This linguistic stratum is earlier than the later attested individual Indo-European languages of Europe [Krahe 1949 ff; Krahe 1964; Nicolaisen 1955; Nicolaisen 1957; Nicolaisen 1960].

The movement of speakers of Indo-European language, by way of invasions, from eastern Europe into central and western Europe, must have occurred in the third millennium and continued in the second millennium, until in the first millennium the traditional language groups seem to have established themselves.

To give credit to Professor Cunliffe, although “Celtic from the West” is his favourite idea, he finally presents also “an alternative that might be worth considering. In the period c. 2800–2200 we can recognize a phase of exceptional mobility throughout Europe... Is it possible that this brief time of interaction” between different cultural traditions, including “the late Yamnaya horse riders from the east — was the time when Celtic emerged, somewhere in the broad zone of interaction in middle Europe?” [Cunliffe and Koch 2010: 35].

This alternative — a sort of compromise — is more acceptable than his first-mentioned favourite idea. The “late Yamnaya horse riders” of course are recognizable as Indo-European invaders who then had to come to terms with the original non-Indo-European population. Thus we are confronted in Europe with virtually two different populations: Indo-Europeans and non-Indo-Europeans (in the shape, of course, of many distinct language groupings). The chronological dividing line is c. 2500 BC. What is before must be considered non-Indo-European. That means, the Atlantic

fringe and its population in its early phase is non-Indo-European, the process of Indo-Europeanization however has moved from east to west and further to the south. The Celts or their predecessors did not arrive by sea, they came by land, as it seems on foot.<sup>11</sup>

Solely, as regards the language of the Tartessian inscriptions in the southernmost corner of the Iberian Peninsula which are claimed to be Celtic now by John Koch, one could concede that the speakers of that language (if Celts or other Indo-Europeans) might have settled there arriving via the Mediterranean, though only at some relatively late time. At about 1200 BC the so-called “Sea Peoples” were active in the eastern Mediterranean. Their ethnic identities are unknown, but it is likely that elements of Indo-European provenance were among them. At that time an arrival by sea could be imaginable, but evidence of that is lacking. On the contrary, as regards the *Celtici*, also attested in the south of the Iberian Peninsula, it is stated (by Pliny — *Naturalis Historia*, III, 13) that they had arrived there from the north. This is at the moment the most plausible explanation. In any case, “Celtic from the West” cannot explain how the isoglosses of Celtic shared with other Indo-European languages, especially with the eastern ones, could have come about, if a non-Indo-European linguistic zone had lain in between.”

**Postscriptum 2014.** At this point I must attach an epilogue. My original paper was conceived in 2012 and was partly directed against the provocative thesis of Barry Cunliffe, according to which the Indo-Europeanization, or Celticization, of the Atlantic zone had started already about 5000 BC. The question of how these Indo-Europeans could have arrived there at that time in the first place remained unanswered throughout the whole volume; in fact, the question itself was not even asked. The only assumption made was that of short-distance movements from the Mediterranean, but at a time when there could not have possibly been any Indo-Europeans present there. (It must be mentioned that the 2010 volume also contains an opposing opinion, represented by G. R. Isaac, who categorically rejects the thesis “Celtic from the West”.)

Last year, however, the matter took a new turn. Towards the end of October 2013 there appeared a second volume dedicated to this idea, entitled “Celtic from the West 2”. The editors are the same, but this time John Koch is in charge, while Cunliffe confines himself to a somewhat resigned epilogue. The result, in short, is that the thesis as such still stands, but is now considerably deflated. With the exception of Colin Renfrew (whose “wave-of-advance” theory apparently had prompted the early dating in the first place; however, Renfrew’s model assumed expansion from the east by land), there is no longer any mention of 5000 BC, but the emergence of Celtic is now, in a more realistic way, placed into the 2nd millennium BC, by which time the Mediterranean coastal regions had already been colonized by settlers of Indo-European origin, and the Atlantic coast could have been reached by vessels navigating the Mediterranean. John Koch takes Tartessian to be the embryonic form of Celtic, implying further expansion northwards; James Mallory instead prefers as more realistic the

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<sup>11</sup> I omit here the arguments based on the testimony of genetics. Gene-flow maps of course give no chronological indications, nor can they provide ethnic or linguistic identifications. There exist, as far as I can see, no maps which would support any movement of Celts or other populations from the Atlantic coast land-inwards. Thus, also by the interpretation of gene-flows, we are thrown back to the traditional model of Indo-European, including Celtic, arriving from the east.

traditional view of Celtic expansion over land from the east, and the colonization of the Iberian Peninsula from north to south, while Cunliffe finally contents himself with the middle of the second millennium, leaving all other questions unanswered for the time being.

In this new volume there are again presented a great number of archaeological arguments *pro et contra*. As a linguist, I am of the opinion that arguments based on material culture cannot prove anything decisively about the underlying ethnos or language of the people who left those remains. Language transfer, as it usually happens, is associated with human beings in close bi- or multilingual contacts, has sociolinguistic causes and conditions (with population movements on a larger scale) and, in addition, is strongly dependent on extralinguistic factors (such as natural catastrophes, famine, unrest, war, etc.), which may have caused these movements in the first place and which (together with status, power or prestige) also influence the general later direction of sociolinguistic developments.

So what is the bottom line? One cannot overlook the fact that W. Meid’s address, reproduced above, never mentions Hallstatt. Indeed, glottochronological data (even if one takes into consideration the discrepancies between statistical assessments by different linguists, on one hand, and the doubtless fact of Celtic migration to the British Isles around the mid-2<sup>nd</sup> millennium BC, on the other hand) clearly indicate that the conventional theory of the Hallstatt Urheimat in Central Europe has long become outdated. However, this does not mean that Herodotus was totally wrong (as Koch believes). Indeed, there used to be Celtic tribes in the upper reaches of the Danube, and, moreover, it was exactly there that the 7<sup>th</sup> century BC (or even an earlier period) saw the emergence of a cultural-linguistic community possessing high levels of “passionarity”, which allowed their further expansion to the east, west, and south. One may speak of a kind of ‘secondary homeland’ of the Celts, which gave rise to Continental Celtic languages. This location, if we may employ a term used by geneticists, could be called a ‘refuge’, where pre-Celtic people and pre-Celtic languages underwent a distinct stage of **‘pupation’**, vital for the emergence of a **‘butterfly’** (or, rather, a flight of butterflies disseminating language and culture all over Europe). What I mean by the ‘pupa’ stage is a certain stage in language evolution at which the same shared dialect begins to expand over a range of strata of a given society, diverging into sociolects, accumulating potential morphological and semantic models and increasing its vocabulary, partly due to contact with local population. This is what appears to have happened in Central Europe. However, calling it the true homeland of the Celts, that is, the location where around 3500 BC pre-Celtic started to diverge from other IE languages (other than Anatolian and Tocharian, of course) is scarcely possible<sup>12</sup>. Moreover, in theory we can surmise that the model suggested by me above may have worked more than once, that is, the divergence of the Celtic language family may have created multiple ‘refuges’, each of which would house its own ‘pupa’ stage followed by the ‘imago’ stage (\*Celtic-2), all of them genetically similar to each other yet not identical.

Is it possible to agree with Cunliffe in part and make a trade-off by locating a similar and contemporaneous ‘refuge’ on the south of the Iberian Peninsula? Meid’s objection that the Celts “had not maritime ideology” is hardly valid: while the Celts were doubtlessly not much of a sailor race, Cunliffe’s model only requires a series of cautious voyages along the Mediter-

<sup>12</sup> Cf. also [Falileyev 2014; Kazakevich 2015] on the East spreading of the Continental Celts. As we know, Celtic languages are characterized by the loss of IE \*p (\*p > \*f > \*h > 0); so, could we suppose to see a trace of the ‘caterpillar’ in the name *Hercynia* (*silva*) < \*perkunyā, with a reflex of IE initial \*p-?

ranean coastline. So some Celtic tribes could at least theoretically (as it seems to me) have reached the south of present day Portugal and settled there. But, if we accept this idea, this would automatically bring us to the conclusion that the Celtization of the British Isles began with the people who came from the Iberian Peninsula. This, however, goes at odds with the apparent similarity between Brittonic and Gaulish dialects. Or could there have been multiple waves of Celtic migration? Or at least Indo-European?

In any case, the obvious conclusion is that the actual homeland of the Celts still remains a mystery, and that the designation of Tartessian as ‘Celtic’ hardly provides a serious clue for understanding the contents of this ‘black box’.

The Tartessian question was tackled in a special section of Vol. 42 of *The Journal of Indo-European Studies* (No. 3 & 4, Fall/Winter 2014).

After a brief preface by the editor-in-chief, J. Mallory (“The Indo-European-Tartessian Debate”, pp. 332–334), who not only presents the issue (to which language family Tartessian belongs) to a novice reader, but also gives a summary of what this issue has resulted in (that is, the emergence of the Atlantic theory of Celtic ‘origin’), the floor is given to John Koch himself.

Koch’s text takes up nearly a hundred printed pages (“On the Debate over the Classification of the Language of South-Western (SW) Inscriptions, also known as Tartessian”, pp. 335–427). This time it seems to rather address an audience that is already acquainted, at least to a certain degree, both with Koch’s own theories and, importantly, with their critique. Thus, while the tone of his earlier publications on Tartessian ‘reconstructions’ used to be buoyant and sometimes even triumphant, here he is reduced to a mere defensive position. (Besides, I should also note that the journal publication lacks those fine coloured illustrations which must have contributed to *some* extent to the hypnotism of the earlier publications).

Koch begins by pointing out that his theory of Celtic Tartessian is not universally rejected. For instance, in [Hamp 2013], the Tartessian language is added to the branch ‘Northwest Indo-European’ and “the language name *Tartessian* is used” (p. 338). Indeed, recognition by one of the patriarchs of Celtic historical linguistics (born 1920!) is notable.

No less significant, as rightly noticed by Koch, is the fact that the term ‘Tartessian’, as a designation for the language of inscriptions from a certain region, has become widely accepted. Before his works, the very idea of the linguistic uniformity of these texts was doubted.

One cannot deny that Koch’s revisiting of the epitaphs from the southwest of the Iberian Peninsula, which had been thought to be unidentifiable by definition and only occasionally bearing a few isolated Celtic or Indo-European traits (such as personal names), evoked real interest on the part of many linguists. Moreover, if they choose to critique Koch, they automatically had to take their own stand on the matter — which meant, basically, that they either had to qualify Tartessian as a ‘non-Indo-European language containing a large number of Celtic onomastic forms’ (p. 343) or regard it as another possible Indo-European language, unattested elsewhere.

Interestingly, those who believe Tartessian to be non-Indo-European accept the presence of Indo-European and even Celtic names borrowed into it over the course of long-term contacts. The proponents of the ‘lost Indo-European language’ may see these names as either borrowed from Celtic or as native cognates, often close to Celtic or Italic. Thus — I would stress it once more — Koch’s idea that the language of the SW inscriptions is Indo-European and presumably Celtic is not totally unsubstantiated.

His JIES article presents an attempt at a systematic arrangement of his studies, dividing them into the areas of phonetics (which, however, he admits impossible to reconstruct), vocabulary, morphology and to some extent syntax. The available vocabulary of Tartessian, due to the limits of the genre (i. e. epitaphs), is, as he cannot help but notice, not too rich in appella-

tives, but its onomasticon is quite abundant, so this is where he suggests to look for Celtic traits. A large portion of his article is dedicated to the name of a Tartessian king, Argantho-nios, which Koch naturally assumes to be linked to the IE word for ‘silver’<sup>13</sup>. The bibliography on the subject is vast, and extensive citations given by him are sufficiently reliable. The only problem is that this very element never occurs in the Tartessian onomasticon listed by Koch... and, unfortunately, other possible Celtic names in the journal publication of his Tartessian theory are not mentioned at all.

Thus, the reconstructed sound sequence **b<sup>a</sup>ast<sup>e</sup>-** is interpreted as a personal name containing a stem with the meaning of ‘death’ (cf. Celtic *\*bāsto-m* < *\*g<sup>w</sup>ōsto-m*, IE *\*g<sup>w</sup>es-* ‘extinguish’) “showing the characteristically Celtic treatment of Indo-European *g<sup>w</sup>* and *ō*” (Koch 2009: 83). The cited reconstruction is one of the most plausible — it can at least be reasonably interpreted as a case of *g<sup>w</sup>* > *b* transition, present in all Celtic languages and presumably of a very early origin. However, the only actually attested case is Irish *bás* ‘death’, whose etymology is disputed (see [Matasović 2009: 59]). As for the second part of the reconstructed name, **eb<sup>u</sup>u<sup>o</sup>i** ‘the yew-wood’ (p. 84), this is an actual Celtic word, yet its specific meaning is highly variable (yew, alder, hog-weed), so it is very likely to be a *Wanderwort* whose etymology is unclear and whose meaning is at best vaguely botanical — that is, it may well have been an early borrowing into Celtic. The very bizarre semantic of the name ‘death-yew’ is hardly worth discussing. However, this time the JIES article treats it as a place-name (p. 408).

Other lexical units are also revised in the article. Thus, one of the most notable stems attested in the inscriptions is doubtlessly the letter sequence **na<sup>r</sup>k<sup>e</sup>ti-**, whose verbal nature is confirmed by the abundance of sub-forms (**na<sup>r</sup>k<sup>e</sup>nti**, **na<sup>r</sup>k<sup>e</sup>ni-**, **na<sup>r</sup>k<sup>e</sup>nti-**). As Koch writes referring to (Villar 2004), “what Villar identifies as an Indo-European verb is a variant of the most common word in the corpus and the most essential element of the Tartessian epigraphic formula” (p. 372). Yet, while in the first publication of the corpus this verb (it seems very likely to be a verb indeed) is linked by Koch to Old Greek *ναρκάω* ‘grow stiff, numb, dead’, this time he offers another etymology: from IE *\*ner-* ‘under, below, left’. Even so, he admits that “*\*ner-* does not occur in Celtic, but is probably related to the fossilized Old Irish preverb *ne* ‘down’ (on which see [McCone 2006: 181])” (p. 373). The transition from IE *\*CeC* > Celtic *CaC* is in fact possible (cf., e. g., *\*mer-* ~ OI *marb* ‘dead’), but what K. McCone writes in the cited passage is actually the following: “This brings us to the main exceptions to the principle of reducibility, beginning with *-ne-* (probably < *\*ni* ‘down’ by lowering before *-s(s)a-* in the first instance and then spread by analogy to forms containing the other two roots in question)”. Thus, the etymology of Tartessian **na<sup>r</sup>** can hardly be called substantiated. The second element **k<sup>e</sup>** is seen by Koch as the verbal form itself, derived from IE *\*k<sup>e</sup>i-* ‘lie down’. In Celtic, this verbal stem is only attested with the suffix *-m-* (OI *cóim* ‘dear, nice’, MW *cu*, OBret. *cum*, Gaul PN *Coemo*); of course, the supposed archaic nature of Tartessian may theoretically support the existence of an independent IE cognate that is not directly derived from the reconstructed Common Celtic *\*koymo-*, but still, this etymology does not seem fully reliable, and moreover, Koch never explains the preservation of original vocalism.

Generally, he himself has described the situation best: “To compare the unclassified language with principles and reconstructions that are not well established is a derailment of the method — the tail wagging the dog” (p. 361).

The article is concluded with a lengthy alphabetic list of Tartessian vocabulary reconstructed by Koch, each word given its semantics and etymology.

<sup>13</sup> However, the Celtiberian word for ‘silver’, *silabur* (Tovar 1975: 11), is never discussed by Koch in his Hispano-Celtic reconstructions.

What arguments are given by the opponents of the Tartessian-Celtic hypothesis to refute Koch's claims? In his short article "Comments on John Koch's Tartessian-as-Celtic Enterprise" (pp. 428–438), Joseph Eska revisits the data of the Italo-Celtic theory which are at odds with Koch's theory. But this time, he proposes another, quite intriguing and non-trivial, system of criticism: "As an experiment, let us propose that Tartessian is Iberoid" (p. 431). What are the consequences? It turns out that the multiple morphemic elements used as evidence by either Koch or other proponents of the Indo-European nature of Tartessian (including myself), also occur in Iberian languages in the same functions; for instance — **-a** (ā-stem nom.sg; 1 sg. perf.), **-ai** (ā-stem dat. sg.; loc.sg.), **-Bi** (instr.pl.), **-un** (gen.pl.) etc. As for the key stem **nařk\*ni-** and its numerous derivations (or variants), Eska draws attention to their high variability which apparently does not allow to reconstruct any verbal inflections at all, especially on the level of Indo-European.

The critique by Miguel Valério ("The Interpretative Limits of the Southwestern Script", pp. 439–467) concerns mostly the challenge of deciphering. In his view, the very basic data seen as 'Tartessian' need reconsideration, since any discussion of etymological matters where the very readings are doubtful can only make "results unsustainable".

The five points below are only a short summary of the problems: (a) some epigraphic and paleographical evidence is mishandled or discarded; (b) doubtful readings and fractures of the inscriptions are not seen as impediments to interpretation; (c) consensual and uncertain sign readings are used at the same level; (d) the orthographic rules are postulated with a suspicious degree of irregularity (one has yet to see what proper comparativists have to say about the posited sound laws); (e) in several cases, segmentation is based exclusively on etymological comparisons, without the support of any combinatorial or contextual evidence (p. 461).

Finally, there is Blanca María Prósper with her paper "Some Observations on the Classification of Tartessian as a Celtic Language" (pp. 468–486). She does not take such a harsh attitude towards Koch and is theoretically ready to accept the presence of Indo-European (and, moreover, even Celtic) traits in Tartessian inscriptions. Yet in her opinion these traits are mostly limited to onomasticon which may well have been borrowed (see, for instance, [Correa 1992]). Along with that, she notices the absence of the sound /p/ in the inscriptions ("ironically, the most convincing token of celticity that Koch has really come up with"), as well as certain traits which make Tartessian look similar to Lusitanian. Moreover, one might suspect that Prósper herself had already considered the idea of Tartessian as Celtic before and is now being somewhat vexed by the fact that Koch's uncritical reconstructions have impaired the belief in the possibility of any such affiliation at all. With a feeling of regret, she points out the inconsistency of Koch's alleged verbal forms and the inaccuracy of his readings. As she writes, "the success of Koch's theory in the long term will crucially depend on his ability to persuade the reader that the phonetics of the appellative vocabulary are Celtic *and* the morphology at least looks Indo-European *and* the chronology is consistent with what we know about other Celtic languages" (p.484). That is, in her opinion, the question is still open.

In a final response to his opponents, John Koch writes: "A question stands behind this: what do we expect in a newly discovered language, one more ancient than the rest of its family? No one reading this will remember at first hand the surprised reaction to the initial claim that Hittite was Indo-European, and few will remember the decipherment of Linear B" (p. 516).

This argument is indeed irrefutable. Yet it seems to me that the debate as such in this particular case is far from useless. Whatever we believe Tartessian to be, its probable dating (about the 8<sup>th</sup> century BC) leaves no possibility to see it as a sort of fossilized proto-Celtic, since the latter must have diverged already about 1250 BC. Therefore, the Atlantic hypothesis turns out to be no more convincing than the traditional Central European theory, and one has to look for the actual Celtic 'Urheimat' elsewhere.



*As an epilogue:* In June 2015 I was in Glasgow, where the 15<sup>th</sup> International Congress of Celtic Studies was being held. John Koch also took part in it, this time speaking on the emergence of the first Celtic inscriptions on the Iberian Peninsula. His paper did discuss the implications for the precocious (900 BC) Bronze-Iron transition on the Iberian Peninsula and for the primary linguistic split in Celtic dialectology — with Hispano-Celtic on one side and the commonality of Goidelic, Brittonic and Gaulish on the other. In other words, it seems to me that the Atlantic theory is presently being revised, given that (as I have written above) the data of insular Celtic (Goidelic and Brittonic) do not fit into it.

However, since I was not able to hear his presentation directly, there is one last thing on which I would like to comment instead. During a traditional trip, I visited the west of Scotland (Pictland), where in one of the local museums, I bought a small book by Benjamin Hudson, called *The Picts* (Hudson 2014), thematically a volume in the series “The People of Europe”, devoted to ancient European tribes and peoples from their origins in prehistory to the present day, with general editors listed as James Campbell and... Barry Cunliffe! The book on the Picts itself turned out to be full of errors, historical inaccuracies, and wild speculation, although this is not uncommon in publishing business. What actually attracted my attention was that the advertising list of forthcoming editions mentioned *The Celts*, by John Koch. I admit that I am not sure how to deal with this fact. Should I feel concerned that the Atlantic theory and the recognition of Tartessian as Celtic are moving from the field of academic debate to the field of firmly established facts, deserving popularization? Or, on the contrary, relief on seeing that the Cunliffe-Koch theories are being expelled from the field of mainstream academic discussion and reduced to the confines of marginal popular books?..

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Т. А. Михайлова. Происхождение кельтов: время и место? По поводу развернувшейся дискуссии о центральноевропейском или же атлантическом сценарии.

Статья представляет собой обзор новейших работ, посвященных проблеме определения кельтской прародины. Долгое время господствовавшая в археологии и лингвистике так называемая «центральноевропейская» модель, локализирующая прародину кельтов у истоков Дуная и опирающаяся в основном на данные археологии (Гальштат), в настоящее время пересматривается. Толчком к этому послужили открытия, сделанные в южной Португалии: Дж. Кук на базе прочтения старых и новых тартессийских надписей, написанных иберийским письмом, пришел к выводу, что тартессийский язык является кельтским. Эти же идеи были поддержаны археологом Барри Канлиффом. В результате возникла новая, «атлантическая» теория кельтской прародины, локализуемой на юге Пиренейского полуострова, вызвавшая резкую критику кельтологов. В статье также предлагается альтернативная обеим моделям гипотеза восточной локализации кельтов. В состав работы также входит текст доклада профессора Вольфганга Мейда «Кельты с Запада?», предоставленный автором специально для журнала «Вопросы языкового родства».

*Ключевые слова:* миграции, кельты, иберийцы, тартессийский язык, археология, генетика, прародина кельтов.

