

On the Debate over the Classification of the Language of the South-Western (SW) Inscriptions, also known as Tartessian

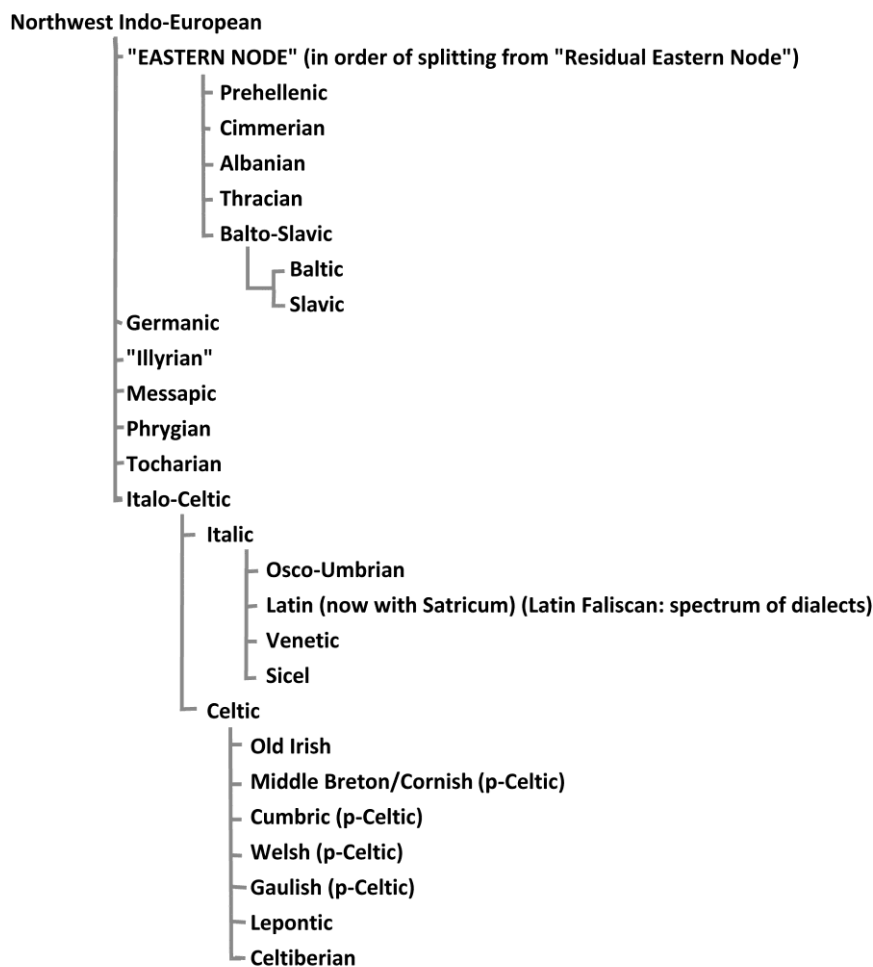
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I. Three and a half theories

§1. Many linguists will be interested in Eric Hamp's 2012 revision of his 1989 Indo-European family tree (both in Hamp 2013). For the present subject, the node called **Northwest Indo-European**¹ is most relevant. The older and newer versions of this branch are redrawn below.²

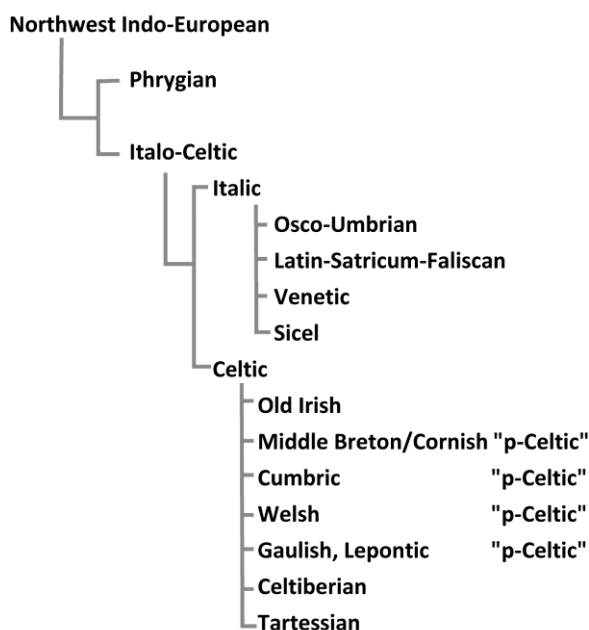
Hamp's **Northwest Indo-European** 1989



1 Hamp's names and descriptive terms for languages are indicated here with bold type rather than quotation marks, as quotation marks are used meaningfully on his trees.

2 Outside of the **Italo-Celtic** node I have simplified some of the details and omitted some of the bracketed annotations.

Hamp's Northwest Indo-European 2012



§2. The biggest change in this section of the tree is that what had been **Northwest Indo-European** in 1989 has become two sibling branches in 2012: (I) **Northwest Indo-European** and (II) **Northern Indo-European (“mixture with non-Indo-European”)**. The latter branch (not included above) comprises: (1) “**GERMANO-PREHELLENIC**” (whence the siblings **Germanic** and **Prehellenic (substrate geography)**³); (2) **Thracian, Dacian** (as a single node); (3) **Cimmerian**; (4) **Tocharian**; and (5) “**ADRIATIC-BALTO-SLAVIC**” (whence the siblings (a) **Balto-Slavic** and (b) “**ADRIATIC INDO-EUROPEAN**” (the latter being the parent of (i) **Albanian** and (ii) “**MESSAPO-ILLYRIAN**”).⁴ Thus, one way in which the 2012 tree is different is that **Phrygian** alone is now seen as forming a subgroup with **Italo-Celtic**, whereas the 1989 tree had **Italo-Celtic, Tocharian, Phrygian, Messapic, “Illyrian”,** and **Germanic**, all as first-generation descendants of **Northwest Indo-European**, together with an “**EASTERN NODE**” (as a sibling in the same generation as **Italo-Celtic** and so on).

§3. Within the **Celtic** node, one change from 1989 is that **Gaulish (p-Celtic)** and **Lepontic** are no longer represented as siblings, but now as **Gaulish, Lepontic “p-Celtic”** on a single line (thus more along the lines proposed in Eska 1998). The “**p-Celtic**” label of the **Gaulish, Lepontic** node also occurs with (1) **Middle Breton/Cornish**; (2) **Cumbric**; and (3) **Welsh** (each of these three on a separate line). Like Gaulish, these three were also labelled

3 The last term does not designate a form of Greek, but apparently an extinct Indo-European language that had influenced Greek.

4 I have added the numbers and letters, in the style outline levels, for clarity.

(p-Celtic) in the 1989 tree, but the term was then represented in parentheses rather than within quotation marks, which might be viewed as a kind of downgrading of meaningfulness of this category. In neither the 1989 tree nor the 2012 does **p-Celtic** form a parent node with descendants.⁵

§4. In the 2012 tree **Tartessian** has been added as a **Celtic** language.⁶ It is represented as a sibling of **Celtiberian** and **Gaulish, Lepontic “p-Celtic”**, rather than as a dialect, parent, or offspring of these Continental Celtic languages or of any of the other languages on Hamp’s **Celtic** branch.

The language name **Tartessian** in Hamp’s 2012 tree is used, as I have used it, following Correa’s *tartésio* and Untermann’s *Tartessisch*. Thus, it means specifically the language of the SW inscriptions, rather than referring to all the pre-Roman linguistic evidence from the south-west of the Iberian Peninsula.⁷ In its classification of the language of the SW inscriptions, a.k.a. ‘Tartessian’, Hamp’s 2012 tree is broadly in line with what I propose in recent publications (including Koch 2009a, 2009b/2013a; 2011).

§5. The theory that the language of the corpus, called *tartésio* ‘Tartessian’, was Celtic was first formulated by the philologist José Antonio Correa of Seville in the 1980s and early 1990s (cf. Correa 1989; 1992). The initial reaction, it seems, was largely incredulous, but this is not well documented (cf. Villar 2004, 265). By the mid 1990s, Correa had re-formulated his position, now seeing the corpus as embodying Celtic names within a different, probably non-Indo-European, matrix language; however, he offered no proposal about the matrix language as a particular known non-Indo-European language or the relative of one (Correa 1996). This newer

5 Nothing along the lines of Insular Celtic figures in either tree. Lusitanian is another noticeable absentee.

6 This conclusion is anticipated in Hamp 2012, 132 N 4.

7 The controversy over this language name is not directly relevant to the classification issue, but it is principled and not trivial. Many ancient historians and archaeologists have seen the probable core of the polity, usually referred to today by the Greek or Hellenized name *Ταρτησος*, as being in the Huelva area and along the lower Guadalquivir. Clustered in south Portugal and, secondarily, along the upper Guadiana the distribution of the inscriptions is not focused mainly in this core area. Rather, this large zone is a peripheral area for *Ταρτησος* and associated with another group name, variant attested forms of which include *Κυνητες*, *Κυνησιοι*, and *Κονιοι*. So arguably, the name for the script and language of the inscriptions should be ‘Cynetian’, or the like (cf. Koch 2013d). Furthermore, the geographical distribution of the *(-)ip(p)o-* place-names, which are not clearly Indo-European or related to the language of the inscriptions, is dense in the core zone of *Ταρτησος*. Therefore, it might be more accurate to reserve the name ‘Tartessian’ for the pre-Roman language of the place-names of this core. On the other hand, as Correa (2005) explains, three of the SW inscriptions do occur along the Guadalquivir or south of it: the stones of Villamanrique (J.52.1), Puente Genil (J.51.1), and the now-lost stela of Alcalá del Río (J.53.1). In the light of this evidence in the core zone, ‘Tartessian’ is defensible as the name of the script, language, and corpus of the SW inscriptions.

position—call it Correa II—received more favour and could now be considered the de facto standard view, in so far as it is not misleading to speak of such a thing for a subject with so few active researchers.

If Correa’s earlier position (‘Correa I’) had been fully expounded twenty-five years ago, beyond Palaeohispanic studies and within the full view of international Celtic studies, there can be little doubt that it would then have received greater attention from a range of specialists in various Celtic languages and in Indo-European, along with other fields.⁸ According to Correa’s earlier hypothesis, the language of the SW inscriptions would be the first attested Celtic language, with remains going back at least to the 7th century BC (a dating subsequently confirmed). Also, calling the language *tartessio* ‘Tartessian’ injected a further element of excitement into the theory, linking the corpus to the semi-mythical Ταρτεσσοῦς of Greek literature as well as the wondrous, luxury-bearing ‘ships of Tarshish’ of the Old Testament.⁹

But the theory that Tartessian is Celtic first reached international Celtic studies in a toned-down and semi-retracted form, effectively stillborn, as reflected in the abstract of the late Jürgen Untermann:

In the extreme southwest of the Iberian peninsula, a corpus of ca. 70 inscriptions engraved on stone plaques and stelae is attested which is characterised by a script which differs from all of the other native epigraphic scripts of the western Mediterranean; today, this corpus is conventionally designated as composed of ‘southwestern’ or ‘Tartessian’ inscriptions. Thus far, their archeological context has been inadequately investigated: it is assumed that they are to be dated to the period 700–500 B.C.E., and that in most instances the inscriptions are funeral in nature. The script has been deciphered with some degree of thoroughness and security in recent decades, once it had been established that it was a variant of the much better attested and interpreted Old Hispanic scripts attested in the south and east of the peninsula. The linguistic analysis of the texts, which are engraved in scriptio continua, began with the identification of a few formulaic words, and then, more or less securely, with personal names. Some of the latter seem to exhibit Celtic etyma and flexional elements; the formulaic segments do not completely exclude the possibility that the language of the corpus is Celtic. A conclusive decision can not be taken yet. (Untermann 1995, 244)

This thoughtful formulation is exemplary in being certain about its uncertainty: the matter is not resolved, but for the time being closed. Note that Untermann’s position was neither Correa I nor Correa II. Like Correa II, it can be seen as a compromise between ‘Tartessian is Celtic’ and ‘Tartessian

8 This is part of the reason that I chose the title *Tartessian: Celtic in the South-west at the Dawn of History*. Too many people had missed the theory the first time around. Even someone who saw only this title would at least know that there was such an idea.

9 For the ancient passages, see Freeman 2010. For a recent, and cautiously affirmative, re-evaluation of the old problem of the possible identity of Ταρτεσσοῦς and the Semitic Tarshish, see López-Ruiz 2009.

is not Celtic'. But it is not a blend. Untermann found no acceptable evidence for the language of the corpus being anything other than Celtic. And there was a case for it being Celtic, but that case was not conclusively strong or clear, yet.

Nevertheless, tentatively expressed versions of Correa I never completely went away: the language of the SW inscriptions was possibly Celtic, though there were some remaining problems or uncertainties about its classification (Lorrio & Ruiz -Zapatero 2005, 18); or it could be classified simply as 'Celtic family' and 'Indo-European macro-family' with question marks (Jordán 2005, 8; 2007, 751).

§6. With the classification question now reopened, recent reviews of my work by Joseph Eska (2013a; 2013b; 2013c) provide an opportunity to compare the theories, see how they differ and how their implications differ.¹⁰ These three reviews can be welcomed because the SW inscriptions and their language have still received relatively little attention despite their significance. If we think of the Italian Peninsula as southern central Europe, the SW corpus, which belongs to south Portugal and south-western Spain, represents the earliest writing of an indigenous language of western Europe. Despite the priority of the corpus, it remains common, outside the Iberian Peninsula, for experts in ancient history and later prehistory never to have heard of the SW inscriptions. Until recently, this also was the norm in the fields of historical linguistics and Celtic studies, certainly in Anglophone circles.

§7. Arguably the time is now ripe to give more room to this evidence in the linguistic and cultural history of Europe. The examples are nearing the milestone total of 100,¹¹ considerable consensus has been achieved regarding the phonetic transliteration of the most frequently occurring signs of the script (cf. de Hoz 2010, 623; Ferrer i Jané 2010¹²), and a good published collection (with photographs, drawings, and Romanized transliterations) of what is still the majority of the corpus has been available

10 Although the reviews raise some issues of substance, their value is lessened by lack of balance. The aim is apparently to disprove unequivocally rather than to consider thoughtfully the possibilities that the language of SW inscriptions might be Celtic and that Celtic might have evolved from Indo-European in Europe's Atlantic zone. Revised Indo-European explanations are never contemplated.

11 There have been a few discoveries since the census of 95 in Correia 2009. (The Portuguese researcher is not to be confused with J. A. Correa of Seville; emend Eska 2013b 68 and 72).

12 Ferrer i Jané 2010 is concerned with the SE Iberian script, also known as 'Meridional'. Ferrer's article is relevant to the SW script because the writing systems are closely similar. Canvassing the consensus concerning the phonetic values of the SE script, sign-by-sign, it is clear that there is now detailed agreement about the transcription of most signs in the two southern variants of Palaeohispanic script.

for seventeen years with the *Monumenta Linguarum Hispanicarum IV* of Untermann and Wodtko.

An unexpected leap forward was achieved with the discovery by Guerra in September 2008 of the longest extant SW text at the site of the Republican Period Roman town and earlier Iron Age hillfort at Mesas do Castelinho (MdC) near Almodôvar, south Portugal. Having lost only a section of 5–8 signs through wear and with 84 readable signs of continuous text (Guerra 2009; 2010), this find represents a breakthrough for the study of the corpus. In particular, by recombining stems, prefixes, and terminations attested elsewhere in the corpus, the MdC text confirms many word divisions which had previously been in doubt (Koch 2011).¹³ With few exceptions, the SW texts are written in *scriptio continua* with no obvious marking of word divisions between signs.

§8. Most of what follows here will be of interest primarily to historical linguists and of greatest interest to that subgroup dealing with the Indo-European background of the early Celtic languages. However, we shall begin by considering statements made in the concluding sections of Eska's second and third reviews that carry implications for archaeology and ancient history, likely also of interest to generalists seeking a better understanding of European cultural history.

[John] K[och] allows that there may have been Iberian phonetic or phonological influence upon putatively Celtic Tartessian ([Koch 2011]170), but, in view of the undoubted presence of Celtic onomastic forms in the language beside distinctly Iberoid appearance of its phonology, it seems to me much more likely that Tartessian was an Iberoid language which was being influenced by contact after the expansion of Celtic speakers into the area. (Eska 2013b, 72)

13 For my own work on the corpus, this discovery changed a great deal with bibliographical consequences. I first became aware of the inscription when I saw it in Lisbon on 26 February 2009 at the X Colóquio Internacional sobre Línguas e Culturas Paleo-Hispânicas. This was after the publication of the first edition of *Tartessian* (Koch 2009). I chose not to proceed immediately on the MdC text on the sole basis of my own reading. When I received the images and Romanization of Guerra for *Celtic from the West* (Guerra 2010), my own contribution to that book (Koch 2010) was already in proof and only a few mentions could be added. When I began to work on the inscription in the following months, my intention at first was to produce a work of article length. It took time for the full implications of the MdC text to sink in, leading eventually to several changes and, I think, advances in my understanding of the language of the SW corpus (its historical phonology, lexicon, and syntax). The draft article expanded to become *Tartessian 2* (Koch 2011). The first edition of *Tartessian* went out of print in 2010. An expanded and revised second edition (Koch 2013a), taking MdC fully into account, was prepared in 2011 and early 2012, then published in 2013. Although my thinking has continued to evolve, *Tartessian 2* and the second edition of *Tartessian* present a consistent viewpoint on most details. They cover all the subject matter included in Koch 2009a; 2010 and should be considered to supersede those earlier publications.

This thought is echoed in the final paragraph of Eska 2013c:

Jürgen Zeidler, in his [*Bryn Mawr Classical Review*] review of the predecessor of this volume [i.e. *Celtic from the West* (Cunliffe & Koch 2010), as predecessor of *Celtic from the West 2*], suggests that Tartessian may be a non-Indo-European language containing a large number of Celtic onomastic forms. Given that much of the corpus, unlike the undoubtedly Indo-European Celtiberian language, cannot be parsed and that Tartessian phonology appears to be distinctly Iberoid, I am entirely in agreement with such an opinion.

The position taken in these passages is similar to that of Correa 1996, what I have called ‘Correa II’, but here there is a more specific idea about the affiliation of the matrix language, as ‘Iberoid’. Hence my subtitle for this section ‘Three and half theories’: the theory of an ‘Iberoid’ matrix language is a significant subtype of the more general ‘probably non-Indo-European matrix language’ theory.

In the books reviewed,¹⁴ my position is, and still is, that the language of the SW inscriptions, the matrix language as well as the names, show several linguistic features and recognizable lexical items and morphemes that are Indo-European and that a significant subset of these fit the definition of Celtic to the exclusion of a classification of a more generalized Western Indo-European or a specifically non-Celtic Indo-European. Therefore, in comparing my conclusions to the passages above, we can see, against a background of general disagreement, a consensus regarding the classification of an important part of the SW corpus.

§9. As will be seen below (§§14–15), the consensus about the Celtic names in the corpus has numerous adherents. On the other hand, de Hoz expresses the view that many or all of the apparently Indo-European forms have been obtained by arbitrary segmentation of the usual *scriptio continua* of SW texts, for example:

Sí puede ser significativo, desde el punto de vista gramatical, el que la inscripción termine, como muchas de las que contienen la fórmula, en *-nii*, pero aquí entramos ya en el terreno de las especulaciones arriesgadas a las que puede dar lugar la falta una segmentación segura; es característico el que segmentando al comienzo de la inscripción [J.1.1 ‘Fonte Velha VI’] **...PUonii ara**... obtengamos una forma comparable con la final *-nii* mencionada, mientras que segmentando como propone Correa **lok(o)oPUo niiaraPUo** obtengamos una sintagma de claro aspecto indoeuropeo, lo que a mi modo de ver es esperable que ocurra en no pocas

14 *Tartessian* (Koch 2009; 2013), *Tartessian 2* (Koch 2011), and *Celtic from the West 2* (Koch & Cunliffe 2013).

ocasiones cuando intentamos segmentar secuencias de una lengua no indoeuropea sobre cuya segmentación auténtica carecemos de conocimientos. (de Hoz 2010, 398).¹⁵

As I argued in *Tartessian 2*, there is not in fact doubt about the segmentation of most forms in the longer and better-preserved texts in the SW corpus, due to the following factors: (1) the beginning and/or ending of the text preserved intact; (2) the agreement of ‘redundancy’ between the signs for the stop consonants and following vowels (see §§47–49 below); (3) the recurrent formula with elements of it sometimes rearranged; (4) other recurrent stems, word endings (some of these showing case agreement, such as **lok^oob^o niirab^o**), and prefixes, which retain their recognizable form when rearranged in the syntax. I repeat the example of inscription ‘Mealha Nova 1’ (J.18.1) (Koch 2011, 37). In the transcription below, pairs of stop consonant signs and vowels in agreement are underlined (there can be no word divisions breaking these pairs), words and phrases of the recurring Tartessian epigraphic formula are enclosed in square brackets, and probable grammatical endings in agreement are co-superscripted.

lo^o ti e a n a¹ ke r to r o ba^a1 [[t^e] [b^a r e]] [[b^a] [n a r^e n ti]]

II. Implications of the Celtic names

§10. As to how extensive this component of the corpus is, the list of Celtic names varies—though not entirely—among the publications of various researchers (see below §§14–15). In the catalogue in Koch 2013a, there are 88 SW inscriptions. Of these, 16 are short, badly damaged, or for various reasons offer little basis for segmenting the text into words and names that could be compared with the rest of the corpus or with other languages.¹⁶ The 72 remaining inscriptions contain 1752 graphemic signs. The sequences of signs that I have provisionally identified as names all have Indo-European or Palaeohispanic parallels, usually both. Most often these forms have specifically Celtic affinities, including case endings that are consistent with a classification as Celtic. This onomastic subset comprises 596 signs or 34% of the corpus.¹⁷ As discussed below, the lists of names differ in published

15 I follow the Correa/Untermann transliteration **lok^oob^o niirab^o**. A letter (the final **o** in the first sequence) is omitted in error in the transcription in Koch 2013a, 30. **lok^oob^o** on pp. 29, 31, 137, and 197 is the correct reading. Having examined the stone in Almodôvar and a plaster cast in Lisbon, I see no basis for the **a** between the double **ii** and following **r** in de Hoz’s reading.

16 The inscriptions excluded from the statistics are: J.1.6, J.4.2, J.6.2, J.6.3, J.7.3, J.7.4, J.7.7, J.8.1, J.12.2, J.12.5, J.16.4, J.18.3, J.28.1, Medellín T3 86H/1TP-1, Folha do Ranjão, Salacia.

17 In compiling these statistics, signs that could be read with relative certainty were all

research. Earlier writers have generally not attempted exhaustive lists. Therefore, my list is longer (see §14 below). But the figures offered here will serve to indicate an order of magnitude. There is therefore a sizeable subset of the corpus about which there is broad consensus.

From this consensus there follows an implicit agreement that ‘Celtic speakers’ were influentially present amongst the inhabitants of the extreme south-west of Europe by the period of the inscriptions. They were present in, or had previously expanded into, the Algarve, Alentejo, and western Andalucía by the Early Iron Age. What remains in dispute is the classification of the matrix language of the corpus, what is not names. That question will be discussed below. But, for those not so interested in the classification of the matrix language of the corpus, but mainly in the whereabouts of Celtic speakers in Early Iron Age Europe and the implications of this for locating the region of the emergence of Celtic from Proto-Indo-European at an earlier stage, what remains unresolved in this debate may matter relatively little if at all. There is wide agreement in current linguistic scholarship that Celtic was spoken in the extreme south-west of Europe as far back as at least the 7th century BC.

§11. Given that Eska’s three reviews are otherwise consistently against finding anything Celtic in the SW corpus, readers new to the subject will be unprepared to learn of ‘a large number of [undoubted] Celtic onomastic forms’ in the latter two of the three concluding summaries. It is remarkable that the first and longest of Eska’s reviews makes no mention of this crucial fact.¹⁸

The reviews also argue forcefully that it is unlikely that the Celtic language family first emerged from Proto-Indo-European in the Atlantic region, or specifically the Iberian Peninsula. Take, for example, the final sentence of Eska 2013c:

counted. Signs that could be read, but were damaged or the reading was for some other reason doubtful, were also counted. Signs the reading of which was totally uncertain and were represented in the transcriptions with * were not counted. Signs that were missing due to a break in the stone or were simply not carved, but their value can be inferred because of the usual orthography of the recurrent formula, are supplied within square brackets [] in the catalogue, but they were not counted in compiling these totals.

18 Anyone who attended the session of the XI Coloquio Internacional de Lenguas y Culturas Prerromanas de la Península Ibérica held in the Museo de Cultura Ibérica in Liria on 27 October 2012 might notice this disparity especially. In the public discussion after my paper (Koch 2013e), de Hoz said that he agreed with the view expressed by Eska at the Harvard Celtic Colloquium in 2010. (I was not present at that Colloquium.) As de Hoz is sceptical of the presence of most or all identifications of Indo-European names in the SW corpus, as well as Indo-European forms identified in the matrix language, his position would be consistent with Eska’s first review, but not the second or third.

Readers should approach the Celtic from the west enterprise, which must rely solely upon linguistic evidence, with the greatest of scepticism.

Readers are told how to view ‘the Celtic from the west enterprise’, but not what it is. If what is meant is the multidisciplinary project with which I am currently involved (together with co-workers in Oxford, London, Bangor, and elsewhere), this is an ‘enterprise’ consisting of a research team investigating the possibility that Celtic evolved from Proto-Indo-European (or Proto-Italo-Celtic, if we favour a model including this stage) in a region nearer to the Atlantic seaboard than to the upper Danube.

Why is Eska so sceptical about the worth of such an enterprise? The evidence cited by him in 2013b and 2013c does not lead logically to his conclusion quoted above, but to its opposite. If the earliest written texts of western Europe contain ‘a large number of [undoubted] Celtic onomastic forms’, most of these inscribed on not very portable stones found at no great distance from the Atlantic coast, though this fact will not by itself constitute conclusive proof that that is the region where Proto-Indo-European evolved into Celtic, it is certainly not evidence precluding that possibility. In fact, the implication of the onomastics of the corpus is a strong argument that relies ‘solely upon linguistic evidence’—as Eska’s third review insists—to spur further investigation into the possibility of a more westerly emergence for the Celtic sub-family of Indo-European. It is also a reason for readers to reconsider (or indeed ‘approach with greatest scepticism’) the established explanation according to which Celtic emerged in west-central Europe during the Early Iron Age and spread from there together with the Hallstatt and La Tène material cultures.

§12. The Celtic element in the SW corpus is the proverbial ‘tip of the iceberg’ for a much larger case for considering a new ‘Atlantic Celtic’ paradigm. If we do not completely ignore the archaeological evidence (as the third review seems to demand), it is hard to see any scenario to account for the arrival of Celtic in the Peninsula from west central Europe during the Iron Age. There is no Iron Age scenario that will account for the Celtic of the SW inscriptions, but neither is there one for Celtiberian in the eastern Meseta or to explain why Celtic proper names are found in abundance over wide regions to the west and north of Celtiberia in sources for the earlier Roman Period. The evidence for movement of people, or even substantial inter-regional contact, over or around the Pyrenees is too sparse. On the other hand, looking back to the Bronze Age, the western Peninsula was then integrated within the Atlantic Bronze Age phenomenon until about 900 BC (Burgess & O’Connor 2008).

§13. The alternative Atlantic paradigm for Celtic origins now being investigated (see §54 below) has long been incipient. Whenever the two passages in which Herodotus writes of Κελτοί at the source of the Danube are cited in full as founding documents of the central-European model, both of these will also include statements that the Κελτοί resided beyond the Pillars of Hercules as neighbours of ‘the westernmost people of Europe’ called Κυνητες (4, 49) or Κυνησιοι (2, 34) (see now Koch 2014). From other sources we can confirm that these Cynetes inhabited what is now south Portugal, the region of the densest distribution of the SW inscriptions. In Powell’s *The Celts*, reflecting standard doctrine on the subject a half century ago, the author pulls back from a Palaeo-Atlantic subplot as it verges on contradicting the central theme of Celtic expansion from Hallstatt and La Tène.

Tartessos, which probably lay near the mouth of the Guadalquivir, had been in friendly trade relations with the Greeks since the chance voyage, beyond the Pillars of Hercules, of Colaeus of Samos about 638 BC. The Massaliote Periplus reported that the Tartessians traded as far north as the Oestrymnides, which are taken to be the island and peninsula of Brittany. Furthermore, the Tartessians said the Oestrymnians traded with the inhabitants of two large islands, *Iernē* and *Albion*. This is the earliest reference to Ireland and Britain, and the words are Greek forms of names which survived amongst the natives speaking the Irish branch of Celtic. The Old Irish *Ériu*, and the modern *Éire*, are derived from an earlier form which gave *Iernē* in Greek, and the name *Albu* was used by the Irish for Britain down to the tenth century AD. It is another matter as to whether these two names should be considered as of genuine Celtic origin, or whether they are Celtic adaptations from an older language. On the whole it seems that a case can be made out for their being Celtic, but the nature of the evidence is too slight to press an absolute decision. (Powell 1958, 22)

§14. Eska’s second and third reviews end shortly after the sentences quoted in §8 above, thus raising two important questions and leaving them unanswered. If the SW inscriptions contain ‘a large number of [undoubted] Celtic onomastic forms’ as the aftermath of ‘the expansion of Celtic speakers into the area’: (1) where did these ‘Celtic speakers’ come from and when? and (2) which forms in the corpus are these undoubted Celtic names? Readers may also wish to know whether these names are inflected as Celtic or have been assimilated to the grammar of the non-Indo-European matrix language as might be expected as an implication of the two-language theory (Celtic-‘Iberoid’¹⁹).

19 Inverted commas are placed around ‘Iberoid’ because this is not a term or concept I use, and the term is not explained in the reviews. Reasonable concepts that this term could signify include a genetic relative of Iberian or a language neighbouring Iberian and sharing typological features with it. Certainly, it would be theoretically possible for

Over the past 25 years or so, the following forms from the SW corpus have been compared with Ancient Celtic proper names and/or indigenous proper names from the Indo-European zone of the Peninsula. The list of Correa 1992 (98–101) includes **ak^oosioś**, **ʃ^urka^aioʃ**, **b^ootⁱieana**, **t^aalainon**, **lok^oob^o**, **niirab^o**, **aib^uuris**, **tⁱirt^oos**.²⁰ Untermann 1997 (166–8) listed **liirnest^aak^uun**, **ʃ^aarne^uun**, **lok^oob^o**, **niirab^o**, **k^oob^eelib^o**, **rinoeb^o**, **lok^oon**, **ak^oosioś**, **aalaein**, **aib^uuris**, **alb^ooroī**; **ʃ^anba^ati^a**, **-uarb^uui**, **aark^uui**, **b^ootⁱieana**, **oo^ooir**, **sarune(ea)**, **t^aalainon**, **tⁱirt^oos**, **ʃ^urka^aioʃ**, **uursaar**; also seeing as Indo-European and not regarded as other than Celtic, **na^rk^eentⁱ** and **ba^arentⁱ** (probably being verbs) and **sab^ooi** (possibly a common noun, if not a name).²¹ The list of Villar 2004 (262–6) is **lok^oob^o**, **niirab^o**, **rinoeb^o**, **k^oob^eelib^o**, **aib^uuris**, **uarb^aanub^u**, **lok^oon**. Villar also mentions in the same section of his 2004 article the names of kings of Tartessos, known from Greek and Roman sources, *Ἀργανθωνιος*²² (on which see §§15–16 below) and *Gargoris*. The latter is recognized as probably a compound name with Celtic **-rīχs* ‘king, &c.’ The list of Ballester (2004, 119–20) is *Ἀργανθωνιος*, **anb^aati^a**, **ak^oosioś**, **aib^uuris**, **aark^uui**, **b^ootⁱieana**, **tⁱirt^oos**, **eert^aaune** (which is probably not a name), **lok^oob^o**, **mut^uuirea**, **sarune(ea)**, **liirnest^aak^uun**. Almagro-Gorbea, Lorrio, Mederos, and Torres (2008, 1050) list the following forms as evidence for Celtic character of the *Κυνητες*, also known as *Κοινιοι*, of south Portugal and the Guadiana basin: **ak^oosioś**, **aalaein**, **aark^uui**, **aib^uuris**, **alb^ooroī**; **anb^aati^a**, **b^ootⁱieana**, **sarune(ea)**, **t^aalainon**, **tⁱirt^oos**, **ʃ^urka^aioʃ**. They also mention *Ἀργανθωνιος* as Celtic (2008, 1051). In my publications, I have proposed as Celtic several further name forms in the ‘naming phrases’ of the SW corpus, including **alk^u**, **asune**, **eb^uu^ooi**, **k^aa^set^aana** (probably an occupational title

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- a language to be both of these. A significant distinction that is not explicit in the reviews is whether ‘Iberoid’ is meant to exclude the attested Iberian language itself or not. Even though we do not understand the Iberian language well, it is attested abundantly (for a pre-Roman language that did not outlive the Roman Period). We might expect, therefore, that if the matrix language of the SW corpus actually was Iberian itself, or a very closely related language, more Iberian words and exclusively Iberian names and name elements would be apparent in the SW corpus. By saying ‘exclusively Iberian’, I mean unlike name elements, such **urke** and **ulti**, which are found in both the Indo-European and non-Indo-European regions of the Peninsula.
- 20 **uarb^aan** and **uarb^aanub^u** are also interpreted as Celtic in this paper of Correa’s, but not seen as names. For the sake of ease of comparison, the lists of forms are all given using the conventions of Romanized transcription used in Untermann’s MLH IV (1997). Where the proposed word divisions vary between the writers, I have used my own (Koch 2013a). The segmentation of the forms is not the point at issue presently.
- 21 Several of the forms identified by Untermann as having Celtic and/or Palaeohispanic Indo-European comparanda are attributed by him to publications of Correa of the 1980s.
- 22 With foreign names recorded in Greek script, the accents are omitted here, as they are not likely to reflect the nature or position of the accent in the source language.

rather than a name), **meleśae**, **mut^uuirea**, **soloir**, **tⁱilek^uurk^u**, **t^uurea**, **t^uufek^uui**.²³

§15. 'Αργανθωνιος, Eska's first review says, 'is not so "transparently Celtic" as claimed by K[och]' (Eska 2013a, 63). 'Αργανθωνιος has not only been identified as Celtic by numerous scholars, but several have made the point with particular force, in some cases conveying limits of patience with the quality of objections that have been raised against it. Thus Villar writes:

... we should note that the name *Argantonius* ['Αργανθωνιος in Herodotus] is most probably Celtic, and in no case could it simply be the result of a Hellenization of a name suggesting the abundance of silver in the country governed by him. In Greek, the word for 'silver', though from the same root, has a different formation (ἄργυρος), very unlike *Argantonius*. If indeed an etymological interpretation of a native name changed the word to *Argantonius*, it was as a result of a Celticization and not a Hellenization. (2004, 264)

Also Ballester:

Así la forma podría relacionarse sin dificultad con los hispanocélticos **ARCaNTa**, ARGANTO (*-onis), ARGANTOQ(VM) y otros, pero sobre todo con un estupendo ARGANTONIVS MIROBRIGENSIS en Cáceres (Albertos 1979, 138 y 1983, 873s; Untermann y Wodtko, 1997, 589). (Ballester 2004, 119)

Similarly De Bernardo-Stempel:

It is in fact possible to find Celtic tribal, place, divine, and personal names beginning from the Catalan coast up to the extreme west with *Ligustinus lacus* near Tartessos (where the ancient authors place the ancient Celts): names like ... *Tarrakon*, *Segisa*, *Ebora* and ... *Belenos*, *Neito* or *Argantonios* and many others constitute a too often purposely disregarded evidence in order not to disrupt the so-called 'briga- vs. iltu-ilti-line', a supposedly clean-cut division between a Celticized Iberian Peninsula and its allegedly Celtic-free counterpart. (2006, 47)

And from the Medellín research team:

En efecto, raíces celtas son evidentes en el nombre del rey de Tartessos *Arganthonios* ... que parece de tipo proto-céltico a juzgar por la etimología de la raíz *argent-*, plata. ... estos elementos confirman un claro componente celta en Tartessos. . . (Almagro-Gorbea, Lorrio, Mederos, Torres 2008, 1051)

23 Cf. also Fortson (2009, 313), though with no list: 'On the southernmost tip of the Iberian Peninsula was spoken another poorly known language called Tartessian, which has some Celtic linguistic material in its personal names.'

§16. Ἀργανθωνίος is not recorded in the SW inscriptions, but the name has, understandably, often been brought into discussions of the language of the south-western Peninsula in the Early Iron Age. It was the name of the king of Ταρτησσοί mentioned in two accounts in the Histories of Herodotus (1.163–5). Ἀργανθωνίος was fabulously wealthy and long lived, his reign notionally spanning c. 625–c. 545 BC. The name is meaningful, as he gave a party of Phokaian Greeks 30 talents of silver. It also fits into patterns of pre-Roman names of Hispania and the Celtic world. Thus, the funerary inscription from Alconétar, Cáceres, which impresses Ballester, reads FLACCVS | ARGANTONI [FILIVS] | MAGILANICVM | MIROBRIG|ENSIS (Sánchez Moreno 1996, 127; Vallejo 2005, 186–7; cf. Luján 2007, 253). It does not seem plausible that *Argantonius* at Alconétar could be a borrowing from classical sources, as the man’s kindred and home town also had Celtic names. Alconétar is on the periphery of the historical Ταρτησσοί and the geographical distribution of the SW inscriptions. *Argantonius* at Alconétar is furthermore wholly consistent with Celtic naming patterns found in the Peninsula and beyond: for example, ARGANTO MEDVTICA MELMANIQ[VM] (Riba de Saelices [Vallejo 2005, 186–7]), the group name LVGGONI ARGANTICAENI (Villaviciosa, Oviedo [Búa 2000, 274]), the family name of [T]OVTONI ARGANTIOQ[VM] AMBATI F[ILIVS] (Palencia [González Rodríguez 1986, 123; Vallejo 2005, 186–7]); cf. Cisalpine Gaulish (Vercelli) ARKATOKO⟨K⟩MATEREKOS/ARGANTO-COMATERECVS. Cf. Celtiberian **arkanta**, *Arganta*, Old Irish *arggat*, *airget* ‘silver’, Middle Welsh *aryant* ‘silver’, Breton *arc’hant*, *argant* ‘silver’, Latin *argentum* < Proto-Indo-European **h₂erǵntom* ‘silver’ < **h₂erǵ-* ‘white, bright’ (cf. Falileyev 2010, 55–6); Gaulish ARGANTODANNOS/ARCANTO-DANNOS ‘moneyer, magistrate of silver and/or coinage’ on the coins of the Lexovii and Meldi (de Hoz 2007, 192–3).

§17. Regarding the implications of the Celtic onomastic evidence in the SW corpus, Eska’s second review says:

... it does not take K[och] long to move on and make the assumption that the presence of onomastic forms of Celtic origin in Tartessian ‘is hard evidence for Celtic as the pre-Roman language (at any rate *a* pre-Roman language) in the very place where the longest Tartessian text was unearthed’ (Koch 2011, 6–7). (2013b, 68)²⁴

24 The review then continues: “... subsequently to conclude “[**my present perspective is**] that the classification of Tartessian (‘macro-familia indoeuropea, familia celta’) is *well enough settled*” ([2011] 80; K.’s italics) to the extent that only “[**If**] a comprehensive, coherent, and fully detailed hypothesis for a non-Indo-European Tartessian [**could be devised, that would indeed be interesting and could surely succeed in actually explaining some evidence rather than merely ‘unexplaining’**”

Here the review misunderstands the passage that it quotes, which is clearly, in context, not about the Mesas do Castelinho inscription in SW script (which is to date ‘the longest Tartessian text’), but a second inscription unearthed at the same site written in Roman capitals and of probable Augustan date, reading MANTAIVS EBVRIAE, two forms for which there are abundant Celtic comparanda. Furthermore, the review is contradicting itself if it is concluding, on the one hand, that the language of the SW corpus ‘was being influenced by contact after the expansion of Celtic speakers into the area’ (Eska 2013b, 72) and implying, on the other, that it is an unwarranted assumption that Celtic was spoken there (2013b, 68).

§18. As to the question posed above in §14(1), when and from where did ‘the expansion of Celtic speakers into the area’ occur, I do not agree that archaeological evidence has nothing to contribute to locating linguistic communities in later prehistory. I also do not agree that we have a sound basis on which to know already that the Celtic languages of the region cannot have evolved from Proto-Indo-European *in situ*, to echo the words of Renfrew (1987, 245–9). It is very unlikely that the Celtic of the SW inscriptions or anywhere else in the Iberian Peninsula came about through movements of people or long-distance contacts during the Iron Age (after c. 900 BC). Against the dominant Mediterranean influences transforming the culture of the Peninsula at this time, there is little evidence for substantial Iron Age contacts with Hallstatt and later (indeed too late) La Tène west-central Europe or with western Gaul or the British Isles.

On the other hand, for the period c. 1250–c. 900 one finds abundant evidence for the intense sharing of objects indicative of high status, complex

Tartessian as Celtic]” ([2011] 81) would be sufficient to disprove the Celtic hypothesis.’ (2013b, 68) The review alters the meaning of the sentences by deleting the words restored here in bold type and inside square brackets and 19 lines of text between the two sentences. In writing that the classification of Tartessian as Celtic is settled *from my perspective*, these essential words mean that this is my view, in my thought processes, I myself am not in doubt. Given that what I wrote clearly has a different meaning, the interpretation put on it must be corrected: ‘K. would object that it is “pointless and tedious” to point out problems with his analysis — a position antithetical to the principle of scholarly review —, and states that only a full explanation of Tartessian as something other than Celtic will deter him ([2011,] 80–81)’ (Eska 2013b, 69 N3). It would be a pointless exercise *for me* to think up counter-arguments that I see as unsubstantial and then expect some progress to be achieved by demolishing a succession of such straw men. The sentence does not mean that I think all researchers now accept this classification or that it would be pointless for someone who actually holds a different view to argue it. This would *of course* be worthwhile and could lead to progress. The words ‘If a comprehensive, coherent, and fully detailed hypothesis for a non-Indo-European Tartessian could be devised, that would indeed be interesting’ are also exactly what I mean. If this could be done, we would learn something, and it could lead to progress, maybe even a major stride forward. Without this, the non-Indo-European hypothesis is relatively weak. But I make no claim that this weakness in and of itself must be taken final proof of the Celtic hypothesis, as the review implies.

communal practices, values, and specialist expertise linking the western Peninsula with Atlantic Gaul, Ireland, and Britain. For the (to my view) inevitable conclusion that it is more likely that the Celtic reflected in the SW inscriptions arrived in the Peninsula before the Bronze–Iron Transition and verging on the impossible that it arrived afterwards, it does not matter at all whether we are talking about Celtic names as well as matrix language or Celtic names only. Similarly, if we are debating whether the corpus has many Celtic names or is written entirely in Celtic, neither in itself can decide whether a descendant of Proto-Indo-European (or Italo-Celtic) first developed its defining Celtic features in west-central Europe and then spread to the Atlantic or vice versa. These may not be the total of possible scenarios for a Celtic homeland.

III. How old are the SW inscriptions?

§19. The possible answers to question §14(1) above (when and from where did ‘the expansion of Celtic speakers into the area’ occur) will also of course be limited by the date of the SW inscriptions. There has been recent work in this area. De Hoz (2010, 516) concludes that the invention of the SW script cannot be later than the first half of the 7th century BC. Especially noteworthy is the three-volume publication of the excavations of the Early Iron Age necropolis of Medellín, Badajoz, Spain, on the upper Guadiana (Almagro-Gorbea 2007; 2008; Almagro-Gorbea, Lorrio, Mederos, Torres 2008; see also Almagro-Gorbea 2004). Medellín is the find site of three graffitos in SW script scratched on ceramics (Medellín T2, 86H/13–1 c. 625–600 BC; T3, 86/TP–1 c. 550–500 BC) and one contextualized stela (J.57.1 = Medellín T1, 86H/EN12–1 c. 650–625 BC²⁵), all closely datable.

The stela not only uses SW script, but is clearly in the same language as the stelae of the less-urbanized Algarve 200+ kilometres to the west. J.57.1 reads:]**lok^oon k^eeloia na^rk^ee**[. . . The text thus shares items of vocabulary (**lok^oon** and **na^rk^ee**) with, for example, the ‘Fonte Velha 6’ (J.1.1) inscription from near the end of the mainland in Sagres.²⁶ Inscriptions from both sites show a mature and standardized form of the SW script, the a-be-ka-tu in

25 This date range corresponds to the second generation using the necropolis in the demographic scheme of Almagro-Gorbea 2010. Stela J.57.1 is an instance of re-use in this necropolis of the Early Iron Age, a practice attested elsewhere for both the stelae with SW inscriptions and the so-called ‘warrior stelae’ of the Late Bronze Age. The burial in which the Medellín stela was re-used dates to 525–500 BC, and the date for the stela was determined by the 7th-century fill in the burial and the detailed overall chronology worked out for the necropolis.

26 If Indo-European, **lok^oon** in the Medellín and Fonte Velha texts would formally suit an *o*-stem accusative singular or nominative/accusative neuter. In funerary contexts—and Fonte Velha like Medellín is a necropolis—we may compare Cisalpine Gaulish *lokan* ‘interment, funerary urn’, corresponding to VRNVM in the parallel Latin text at Todi (1st century BC), thus possibly as an item of Common Celtic funerary vocabulary.

right-to-left orientation, in use from the western Algarve to the upper Guadiana. As Almagro-Gorbea proposes, the Medellín stela probably does not represent the initial stage of SW literacy, but a subsequent period when standardized literacy had come into general use over a wide area (2004, 14–16; 2008, 766–71).

§20. If we accept the interpretation of the Medellín stela of 650–625 BC as reflecting a middle stage in SW literacy, when do we date its beginning? A key detail in determining when the SW inscriptions begin is the fact that the series overlap with that of the approximately 130 Late Bronze Age ‘warrior’ stelae (on which see the extensive collections of Celestino 2001; Harrison 2004; Díaz-Guardamino 2010; 2012). As well as geographical distributions that partly coincide, there are examples of stones with both images of human figures and elements of the warrior panoply (shields, spears, swords, helmets, chariots, brooches, mirrors, combs, lyres, &c.) together with SW writing. In the recent work of Díaz-Guardamino, the warrior stelae of Capote (J.54.1) and Cabeza del Buey IV (Majada Honda, J.110) are amongst the latest in her chronological scheme and both also have SW inscriptions. She proposes a date of the 8th or 7th century BC, with this date depending on the beginning of SW literacy (Díaz-Guardamino 2012, 408–9). Therefore, it seems to be only the presence of writing that suggests that stela could be as late as the 7th century.

In the case of the photogenic ‘guerreiro’ of Gomes Aires 1 (J.12.1), features of the representation of the warrior are unlike those of the other warrior stelae.²⁷ The writing and image are integral to a tightly organized overall design and so were almost surely executed at the same time. The warrior motifs of the stelae of Capote and Cabeza del Buey IV are more easily paralleled in the rest of the series without writing. The writing on these two has an orientation that is upside-down from the perspective of the images. However, as Ruiz-Gálvez 2013 emphasizes, the position of writing respects that of the warrior emblems and works together with them to form an overall design. Therefore, the relationship of the writing and pictures on these stones do not imply a prolonged time lag or complete discontinuity between the traditions.

§21. Ruiz-Gálvez makes this last point and has developed a case in publications over the years (most fully in 2013) for an early dating for the SW inscriptions, beginning as early as the Bronze-Iron Transition or even the

27 In representing what seems to be body armour and the position of the feet and legs, the main figure of the well-known warrior stela of Ategua (Córdoba) shows similarities to ‘o guerreiro’ (J.12.1). Díaz-Guardamino regards Ategua as ‘posiblemente uno de los más reciente de la serie’ (2010, 432) dating from the late 9th century or during the 8th (cf. Díaz-Guardamino 2012, 404, 405, 408).

last century or two of the Late Bronze Age, thus beginning in the 9th or 10th century BC. It is possible here only to note a few main points of her argument.

First, while it is widely agreed that the main inspiration of the SW writing system was a West Semitic alephat (vowel-less alphabet) similar to the Phoenician script, Ruiz-Gálvez argues that there were archaic features in the source script, both in specific letter forms and in the orientation of SW writing, which can be right-to-left, left-to-right, or boustrophedon, rather than the consistent right-to-left of the Phoenician inscriptions of the Iron Age. She concludes that the West Semitic source of the SW script was the Old Canaanite of the late 2nd millennium or 10th century BC, rather than its Phoenician derivative used at the time of the Phoenician colonization of the southern Peninsula in the 9th and 8th centuries. She focuses instead on the pre-colonial stage—after the collapse of Mycenae c. 1200 BC but before the Phoenician colonies in the west. During this interval entrepreneurial mariners and artisans built up regular contacts with indigenous clients in the Peninsula. Detailed archaeological evidence is used to support the idea that Cyprus was a key staging ground for these western ‘joint ventures’ at this period, a likelihood to which we shall return in another connection below.

§22. There are also details in the archaeological contexts of the SW inscribed stones which Ruiz-Gálvez thinks more consistent with Late Bronze Age dating. Frequently, the generalization is made that their context is funerary and belongs to the Orientalizing Period of the Iberian Early Iron Age. I do not dispute this. But she argues that they were probably not primarily funerary in their original function. Rather, like the category of warrior stelae that preceded them, they were meant to express the immortalization and divinization of heroes.²⁸ From a recent survey with a total of 86 SW inscribed stones, she notes that only 35 of these can be securely contextualized in necropolises. Of those, three are from necropolises of indeterminate date or later than the Early Iron Age. And another seven were reused as construction material in burials of the Orientalizing Period (7th–6th centuries BC). Only four can be securely confirmed as having a funerary setting as their primary context. She notes examples, and the list could be extended, in which stelae inscribed with writing (that had been clearly designed to stand upright with their lower third buried and concealed) were turned over face down as the roof for a burial chamber of the Early Iron Age. This practice implies not only reuse at that later time, but a meaningful ritual

28 This theory is, incidentally, consistent with what I have suggested is the meaning of the Tartessian epigraphic formula, which appears in its fullest form as NAMES OF DECEASED + **uarb^aan t^e-ro-b^aare b^aa nark^eenti** ‘... has carried away [the named deceased] to the highest place/being/state (feminine) so they now lie under [together]’ (Koch 2013e).

reuse.²⁹ All of which she sees as arguing for dates starting before the end of the 9th century BC (Ruiz-Gálvez 2013, 307–8).

§23. Another interpretive model indicating a possible starting point for the SW inscriptions in the 8th or 9th century BC is that of Brandherm 2013. Like Díaz-Guardamino and Ruiz-Gálvez, he recognizes the importance of the overlap with the series of Late Bronze Age warrior stelae. The warrior series also has a geographical distribution widely spread over the south-west of the Peninsula. Brandherm also sees special connections with the ‘Alentejo’ stelae of the Middle Bronze Age (17th to 13th centuries BC). The distribution of these Alentejanas and the SW inscriptions are especially similar: both have been found mainly in south Portugal. More strikingly, the associated burials are also similar and in fact hard to distinguish until excavated. Both the associated Middle Bronze Age and Early Iron Age types often have a circular or sub-circular pavement at the surface with the burial in a stone cist cut through the middle.

Brandherm’s focus heretofore has been on the metalwork of the Iberian Late Bronze Age and its European and Mediterranean connections (e.g. Brandherm 2007). Building on this, he investigates the types of weapons and other warriors’ accoutrements represented on the Alentejanas and LBA warrior stelae. He concludes that there is nothing represented in the latter series that must be later than the 10th century BC. In other words, there is nothing in the iconography to prove that the LBA stelae evolved beyond that. Therefore, on this basis, one looks for the beginning of the series of stelae with writing at some point within the long gap between 950/900 BC and the dated stela of Medellín J.57.1 = T1, 86H/EN12–1 c. 650–625 BC. For now, a safe interim conclusion is that SW writing was used by the mid 7th century BC, a beginning before the end of the 8th century is probable, and earlier than that is possible.

IV. Differing approaches and preliminary assumptions

§24. We return now to linguistic matters, first with a recapitulation. Theories under consideration concerning the language of the SW inscriptions fall into three categories: a non-Indo-European one-language theory, an Indo-European one-language theory (the names and matrix language are the same, Indo-European, specifically Celtic), and a two-language theory (Indo-European, specifically Celtic names and an

29 An example of a similar re-use of a warrior stela without writing in a necropolis of the Early Iron Age is that of Setefilla, Sevilla, where the secondary reburial is dated to Orientalizing II c. 725–c. 625 BC (Díaz-Guardamino 2010, 359).

unintelligible, ‘unparsible’ matrix language that is probably non-Indo-European and, per Eska, specifically ‘Iberoid’).

As well as different interpretations of specific forms occurring in the corpus and different general conclusions about the classification of the language, readers will note fundamental differences of method and theory and what might be considered overall mindset when contrasting Eska’s three reviews with my publications. The metaphor ‘to be on different wavelengths’ is apt here.

The reviewer does to some degree sense the disparity, but does not, I think, grasp it entirely or correctly. The reviews are written as though there is only one possible method for determining whether the language of the SW inscriptions is Celtic or not. Therefore, the different conclusions could only have been arrived at by my approaching the material with this one method incompetently handled, having already decided beforehand that I wanted the language to be Celtic:

... it appears that K[och] decided that he wanted Tartessian to be Celtic before conducting a truly empirical analysis. (Eska 2013b, 72; cf. 2013a, 65)³⁰

In fact, what I decided beforehand is that an approach like that of the reviewer was not valid and could not possibly determine whether or not the language of the inscriptions was Celtic. To sum up this disparity, the attitude of the reviews is that, in confronting forms in the SW corpus that remind us of early Celtic and other early Indo-European languages, we need to be rigorous and sceptical, so as to exclude coincidental lookalikes. With that I fully agree. On the other hand, the reviews do not, in my opinion, show nearly enough rigour and scepticism concerning what we think we know about Proto-Celtic and what we assume to be impossible in an Ancient Celtic language, attested or unattested. Repeatedly, assumptions are made that we know things about Ancient Celtic that we don’t know. Unproven theories (resting on a narrow base of ambiguous evidence) are treated as proven.

30 If we cannot avoid speculating about the motives of fellow researchers, the safest assumption might be that what we all ‘want’ to make sense of the evidence and that we sometimes reach different conclusions because we have internalized different evidence bases and explanatory models. This point leads to another one relevant here, namely when one ‘wants’ a language to be something as an effect of basic cognition. One hears or sees a stream of words, in which one recognizes some forms as being in a language one knows or something resembling a language one knows, say a Romance language or an Ancient Celtic language *mutatis mutandis*. One then ‘wants’ the other words in the stream to be in that same language, that is, one tries to make sense of them based on that initial impression. If one’s language skills are in balance, one will neither miss cues for the words that are actually in the language or mis-organize the signs into unintended messages; comprehension will be maximized. With the natural language instinct, there is no special virtue in tipping the balance towards one side and calling that rigour. It should go straight down the middle. First impressions need to be checked, but cannot be ignored or doubted to the point of compulsion. What we linguists do is not fundamentally different or separate from the natural language faculty, but an extension of it made conscious and intellectualized.

And it is consistently assumed that to be Celtic Tartessian must be either Proto-Celtic itself (adopting the preferred reconstruction of that) or could only have evolved from Proto-Celtic in accord with one particular unproven model for the evolution of Old Irish from Proto-Indo-European.

§25. To see whether a language is genetically related to a known family, the primary evidence of the attested languages must be compared for systematic relationships, just as philologists first established linguistic relationships many years ago. The only valid shortcut is to use reconstructed features of the lexicon and grammar which can be well supported by abundant unambiguous evidence and are widely accepted (cf. Koch 2011). 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. Unproven theories prematurely elevated to become part of the definition of Celtic are put beyond testing and become unfalsifiable, given veto-power over potential new evidence.

There is an implicit assumption that the Celtic proposals made in my publications are the only Celtic proposals possible for the forms in question (which is obviously absurd as soon as it is stated explicitly), so that any sort of objection that can be raised against a particular Celtic explanation constitutes proof—all the proof that is needed—that the matrix language is not Indo-European. With the two-language hypothesis, as it has been formulated with an unknown matrix language ('Iberoid', not Iberian)³¹, rigour is only possible and only on view on one side of the equation. On the less-examined side, there is the exact opposite of rigour.

I describe the method with which I initially approached the SW corpus as, 'essentially fishing for any further Celtic forms within a corpus of inscriptions in which a few promising examples had already been recognized' (Koch 2013a, 5). This is not a slip betraying a methodology inferior to that claimed. 'Fishing' is what needs to be done, casting the net as wide as the full range of possibilities for an Ancient Celtic language. The classification question cannot be approached encumbered by unexamined

31 Eska 2013a 65: 'So what is Tartessian? It surely is best to leave it as unclassified for now, but the apparent rarity of /m/ and /w/ as phonemes and of **e** outside in inflexional morphology (in which <e> is sometimes taken by K[och] to represent . . . the result of a monophthongisation) and the presence of two sibilants (represented by <s, ś> and two rhotics (represented <r, ř> looks distinctly un-Indo-European.' Celtiberian also has two sibilants, incidentally, and most of the other Celtic languages contrast strong and weak *r*.' With a two-language hypothesis, if the second language is not well understood, but widely attested, such as Iberian (not an otherwise unattested 'Iberoid'), there will be a sizable corpus that will give us an attested inventory of forms and known patterns for combining them. So, there will be (as with such well attested matrix languages as Latin, Greek, or Phoenician) a positive basis for assigning material to the second language. What is to some degree difficult to interpret as Celtic cannot simply be assumed to be Latin or Iberian on that negative basis alone, by default, as it were.

assumptions about what an Ancient Celtic language should look like or eagerness to settle unresolved questions in a particular way. Once a catch of Celtic-looking forms is hauled in, it is then time to ask whether they look Celtic in a grammatically and phonologically consistent way—consistent with each other and also consistent with what we actually know about Proto-Celtic and Proto-Indo-European. Consistent patterns found in the SW corpus include assimilation and loss of [w] before **i, e, o,** and **u**; simplification of diphthongs, lowering of **i* before [a(:)]; and **ro** prefixed to what appear to be verbs, but never prefixed to verbs with present-tense marking (Indo-European primary endings).

§26. We know a lot about the word stock common to the Ancient Celtic languages—especially the onomastic stock—and a fair amount about inflections. Also about sound systems: we know, for example, that Proto-Indo-European **g^w* > Proto-Celtic **b*; that PIE **b^h, *d^h, *g^h* then merged with **b, *d, *g* as **b, *d, *g*; that PIE **p* was weakened in all positions in Proto-Celtic and possibly already lost in some positions at the stage of shared development; that Late (post-laryngeal) PIE **ō* became PC **ā* in non-final syllables and **ū* in final syllables; that PIE **r* and **l* before stop consonants became PC **ri* and **li*. There is overwhelming and unambiguous evidence for all these changes in all the Celtic languages. And all of this has been known for many years. A language that does not show these changes is either not Celtic, or what it means to be a Celtic language must be redefined to include it.

§27. There is great gap between what is known about the Medieval Celtic languages and the less that is known about Ancient Celtic languages. Thus, we know enough about Old Irish and Middle Welsh that we can today not only understand texts written in these languages (in most instances), but can also compose prose or poetry in them. We know how you could, and could not, express most simple, pre-modern thoughts in them. But the Ancient Celtic languages, as known today, are not like this. Composition is not within our grasp for any of them: Gaulish, Lepontic, Celtiberian, Ancient Brythonic, or Primitive Irish. And we cannot *a fortiori* speak or compose literature in reconstructed Proto-Celtic. Linguistic reconstructions are of course an essential ingredient of scientific philology. And in Celtic studies considerable effort has gone into attempts to reconstruct sentence types, the ‘verbal complex’, and so on. These researches have surely taught us something about the prehistory of Goidelic and Brythonic and the relationship of their syntax to that of the other Indo-European languages. But they have been of limited utility for predicting and explaining the forms of Ancient Celtic coming out of the ground on the Continent.

Wodtko’s *Wörterbuch der keltiberischen Inschriften* (2000) and Delamarre’s *Dictionnaire de la langue gauloise* (2003) are two very different

books compiling the lexicons of two rather different Ancient Celtic languages. Gaulish is generally better understood and shows more similarities to the fully attested Insular Celtic languages. And Delamarre is more ready to endorse a preferred interpretation than is Wodtko. However, even though these factors favour more definite conclusions with Delamarre's Gaulish, there remains a high proportion of entries in both books in which more than one possible interpretation must be allowed.

There is every reason to expect that any Celtic in the SW inscriptions would be more difficult to interpret than Gaulish, at least as difficult as Celtiberian, and probably more difficult still. If then, we were to adopt a testing procedure in which we would have to arrive at the one and only widely accepted Celtic interpretation for a form in the matrix language of the SW corpus or conclude that that form was not Indo-European at all—and this seems to be what the reviewer is doing—then the end result of rejecting the Celtic classification would be a foregone conclusion, a 100% certainty, as it would likewise for Celtiberian and possibly even Gaulish.

V. Tartessian evidence and the Celtic verb and sentence

§28. The disparities between Eska's approach and my own work are illustrated by our interpretations of the first 8 signs of the (now lost) inscription of Alcalá del Río, Seville. The second review states:

I note that K[och]'s analysis of **k^ot^uuarat^e** (MLH J.53.1) as continuing **kon-to-~~uo~~-[rāte]* is not possible, for **to* continues a clause-initial sentence connective (cf. OHitt. *ta*) . . . and cannot be preceded by another preverb unless in a very late construction. (Eska 2013b, 71)

None of this is proven or part of the accepted definition of Celtic. The Anatolian part of Eska's doctrine on **to* is examined by Melchert:

All available evidence thus points to the Hittite system of clause-linking conjunctions as an innovation that is not even Proto-Anatolian . . . The attempt of Eska (2007) to refute Rieken's account and revive the connection with Celtic comparanda reflecting an alleged bare stem **to* is not remotely convincing. *None* of the uses he cites for the Gaulish conjunction *to* are actually attested for Hittite *ta* or any other Hittite clause-linking conjunction. (Melchert forthcoming, 32 & N 21)

The recent discussion by McCone of the Indo-European background of Old Irish verbal complex again strongly argues that Celtic **to* cannot be the cognate of the Hittite sentence connective *ta*.³² That explanation goes back to

32 The Continental Celtic evidence underpinning Eska's views about *to* are not extensive.

Myles Dillon and a period when it was thought that Celtic was an especially archaic branch of Indo-European and therefore was expected to share features with Anatolian lost in all other branches. If **to* were in origin a sentence connective, it ‘should have been invariably clause-initial and pretonic like OIr. *no*.’ And, ‘[w]hatever its origin’, there is no basis for interpreting *to* as something other than a preverb in the Insular Celtic languages (McCone 2006, 183).

For Hispano-Celtic, most writers have not sided with Eska in interpreting TO in the ‘long’ inscription from Peñalba de Villastar (K.3.3) as a sentence connective. Rather, it is usually understood as a preposition governing the dative case in the phrase TO LVGVEI (thus Villar, 1991; Prósper 2002, 215: ‘una secuencia de preposición + núcleo nominal’; Jordán 2004, 383–9: *to* ‘preposición de dativo’; Jordán 2006; *a fortiori* De Bernardo Stempel 2008; cf. the discussions of Ködderitzsch 1996, 153–5; and Wodtko 2000, 402–4).

In McCone’s new theory of the Indo-European background of the Insular Celtic verbal complex, which is supported by a body of thousands of items of evidence, the fairly rigid ordering of preverbs, with *to* usually seeking first position, is not seen as a syntactic feature of great antiquity, which would go back to Indo-European or even to Proto-Celtic. Rather, strings of several preverbs—so characteristic of Old Irish and Early Welsh—can be understood as a relatively recent development. The growth has been gradual, spanning a great length of time. It was driven by semantics, so that a first preverb with an originally separate adverbial sense came over time to develop a fused meaning with verb, at which point a second preverb could be added to further modify the meaning and so on. The distinction found in Old Irish and Early Welsh between preverbs having semantic content, modifying a compound verb throughout its paradigm, and those with functions in the tense and mood systems, such as Old Irish *ro*, is secondary. Originally, the preverbs all functioned semantically. So, for example, the meanings of *ro* < **(p)ro* lent themselves to convey or emphasize perfectivity, a resulting state, or previously completed action (McCone 2006, 177–224).

§29. The Celtic compound verb **u(p)o-ret-* ‘rescue, deliver, help’ < ‘run under’ has reflexes in Gaulish, Goidelic, and Brythonic and thus arguably existed already at high node within the branching Celtic family tree. Reflexes of **to+u(p)o-ret-* occur in Goidelic and Brythonic. Therefore, this next compound might also have been achieved as a common development at an early stage. The semantic modification deriving from the second preverb was perhaps ‘(safely) deliver’ > ‘(safely) deliver to’ taking an accusative of destination. The semantics of *kom* added to that might either have

None of the examples demand that *to* was derived from, or continued in use as, a sentence connective rather than a prepositional preverb. The Celticity of the Voltino inscription is in doubt (cf. Eska & Wallace 2011).

emphasized that the one rescued (in the accusative) had been taken to safety ‘together with’ the one rescuing (in the nominative) and/or emphasizing the completed state of the action |*rāte*| ‘has run’.

Against the principle stated in Eska’s review, McCone (2006, 179) argues that composition in which preverbs, including *con* < **kom*, preceded *to* was not necessarily confined to late calques or that the rule about the placement of *to* was necessarily old. And that is just the line of evolution leading to Old Irish. To extrapolate such rules to all Ancient and Early Medieval Celtic languages would require a solid Indo-European basis, which has not been demonstrated. Furthermore, even if it had been, why could adding *kom* to an existing compound verb beginning with *to-* only have happened ‘late’ as the reviewer said? Even if *con* never preceded *to* in Irish until the Milan glosses (2013b, 71)—and as McCone argues, that’s probably not so—that does not constrain all Celtic languages to the same absolute chronology. It is a single inherited productive preverb added to an inherited compound verb, an unremarkable one-step innovation that could have occurred at any time.

§30. Like TO LVGVEI at Peñalba de Villastar, Tartessian **t^o** appears to be a preposition governing a dative noun in the sequence in inscription J.1.1 **t^o a^raiaⁱ**. I have proposed that the opening section of J.1.1 **lok^oob^o niirab^o t^o a^raiaⁱ** shares traditional Hispano-Celtic diction with Peñalba de Villastar’s TO LVGVEI ARAIANOM. The reviews appear to see these similarities as coincidences (Eska 2013b 70–1), regarding as proven the theory of Ködderitzsch (1985; 1996) about ARAIANOM:

...K[och] compares **a^raiaⁱ** (MLH J.1.1) to Celtib. ARAIANOM (MLH K.3.3), but as Ködderitzsch 1985 : 214 has shown, an epenthetic vowel that copied the colour of the preceding vowel was inserted into liquid + consonant groups in the language of this inscription. ARAIANOM, in fact, continues **ar-ĭ-*. (Eska 2013b, 70).

Eska has written about this theory before, at which time he acknowledged its lack of universal approval.

The forms Ködderitzsch identifies as containing such epenthetic vowels are listed in (15).

- (15) a. ENIOROSEI < **or-s-*
 b. ERECAIAS < **perk-*
 c. ARAIANOM < **ar-ĭ-*
 d. OLOGAS < **polĝ^(h)-* < **polk-*

I leave open the question as to whether Ködderitzsch’s etymologies are correct. (Eska 1996, 78)

None of the treatments of the text since the publication of Ködderitzsch’s analysis, Olmsted (1988: 356–361 & 374–378) & (1991: 287–290), Villar

(1991), and Meid (1994a ≡ (1994b: 31–37), discuss his epenthesis proposal at all. (Eska 1996, 84 N. 14)

Wodtko (2000, 30) regards the epenthetic vowel (Sproßvokal) in Ködderitzsch's explanation of ARAIANOM as unnecessary. We should avoid an explanation that would also make a coincidence of the similarity of ARAIANOM and the Celtiberian genitive plural group name **araiokum**, which occurs twice in Botorrita 3 (K.1.3) (Wodtko 2000, 30; cf. Jordán 2005, 375–90).

§31. I think the unique **k^o-t^u-ua-rat^e** of Alcalá del Río is probably functionally equivalent to, or takes the place of, the formulaic **t^ee-b^aar[e]** (J.16.1), **t^ee-b^aare** (J.1.3, Vale de Águia), **t^ee-b^aare** (J.18.1), **t^ee-b^aere** (J.7.8), **t^aae-b^aare** (J.14.1); **ro-b^aare** (J.1.1, J.12.1, J.18.2, MdC), **ro-b^aare** (J.16.3); **t^eee-ro-b^aare** (J.18.2), **t^ee-ro-b^aare** (J.1.1, J.12.1); **ar-b^aarie** (J.10.1), **ar-b^eieñit^u** (J.12.3); &c. The form of both can be seen as derived from an Indo-European perfect formation, with **a** (probably |*ā*| < **ō*, the usual development in Celtic) taking the place of **e** in the present stem. In both cases, I see these as verbs of motion taking a feminine singular superlative as an accusative of destination: thus, **k^o-t^u-ua-rat^e t^uunbⁱitesb^aan** |*kon-tu-ua-rāte tumites_aman*| meaning something like 'has delivered safely to the greatest tumulus' as a variant on the formula **uar(n)b^aan t^ee-ro-b^aare** |*uar_aman de-ro-bāre*| 'has carried away to the highest destination'. There are two objections to this in the reviews: first,

In Insular Celtic, the descendants of proto-Celt. **pro* are employed to perfectivise preterite verbs so one wonders what the function of **ro** would be when used with verbs in the perfect tense. Surely we would not expect to find pluperfects in funeral inscriptions?' (2013b, 71)

This is not a valid point. Where reflexes of Proto-Celtic *(*p*)*ro* are found with reflexes of PIE perfect formations, the sense is perfect not pluperfect: e.g. Old Irish *ro-cechain* 'has sung' (not 'had sung'), Middle Welsh *ry-gigleu* 'has heard' (not 'had heard'). The correct generalization is that Insular Celtic perfectivized verbs have a no more perfect and no less perfect meaning regardless of whether they derive from Indo-European perfects, aorists, or Narten imperfects.

The usage of **ro** in the SW inscriptions, where it always occurs with forms that resemble Indo-European perfect formations, suggests the interesting possibility that this pattern reflects a stage that led to what can be observed in the earlier medieval Insular languages. Thus, **ro** possibly began to be used to reinforce the perfect meaning of inherited perfect forms to which it was prefixed. That usage, observable in Tartessian, could have led to a situation in which the inherited perfect forms came to have only simple past meaning when *ro* was absent, leading to a functional convergence of the perfect and

other past tenses and the inverse possibility of adding *ro* to other past tense formations to give them perfect sense.

§32. The second objection to **t^ee-ro-b^aare** ‘has carried away’ is:

K[och] argues that **b^aare** (e.g., MLH J.1.1) continues the perfect of the Indo-European **b^her-* ‘carry’ **(bhe-)bhōre*, citing Ved. *jabhāra* ‘has brought’, *babhāra* ‘has carried’, and Goth *-bar* ‘carried’ as comparanda ([2011,] 64–65). This root, however, did not make a perfect in Indo-European: the forms that he cites are inner-Indic and Germanic innovations. Elsewhere in Celtic, this root makes a *t*-preterite and continues narrative imperfect **b^hēr-t* as now convincingly demonstrated by Jasanoff (2012, 131–133). (2013b, 71)

This is written as though there was a dispute over **b^her-* not forming a perfect in Proto-Indo-European, but there isn’t. The point is that there was an inherited gap and inherited raw material that resulted in forms resembling **b^aare** being created in more than one Indo-European branch. The review is also written to imply that there is a disagreement over Jasanoff’s explanation of Old Irish *birt*, Middle Welsh *kymmyrth*, &c. The Narten imperfect **b^hēr-t* works better than Watkins’s (1962) sigmatic aorist **b^her-s-t-i*. An Early Welsh example which Jasanoff’s explanation suits is *ken-y vaccet am byrth am-borth* ‘because he was nurtured, he wielded [weapons] around the stronghold’s gate’ (< **ambi-birt*) from the B text of *Y Gododdin*.

The key point is that we do not know when the convergence occurred of the Indo-European perfect with the other past tenses that contributed to the preterite in Goidelic and Brythonic. This convergence could be subsequent to Proto-Celtic (Villar 2005, 352). There are some details of Brythonic that would be more easily explained if this convergence had come relatively late in prehistory. For example, Early Welsh and Middle Breton, *guoreu/gueureu* from perfect **uēurāge* survives alongside *t*-preterite *gwnaeth*³³/*greaz*³⁴ as preterite forms of the same verb with the same meaning ‘made, did’. Similarly, in Early Welsh *amuc* ‘fought for’ < perfect **ambi-woike*, alongside *t*-preterite *amwyth*, meaning the same thing. A significant degree of flux and variation in the system is also indicated by innovative Gaulish past-tense verbs such as ΔEΔE and KARNITU that have no cognates in Goidelic or Brythonic. It follows that it is not certain that at the Proto-Celtic stage **(p)ro* could be added to preterite (< imperfect) **birt* to supply the meaning ‘has carried’. Furthermore, it is unwarranted to conclude that such an innovative form would be the only possible way that any Ancient Celtic language could have created a perfect for **b^her-* in any of its meanings.

In devising a tradition of memorial epigraphy, a way to express the

33 First singular *gwreith* < **u_raxtū* in the ‘Peis Dinogat’ interpolation in Llyfr Aneirin.

34 From the full preterite paradigm and comparanda it is obvious that Modern Breton *greaz* is in origin a *t*-preterite, though formally it agrees with regular *s*-preterite in *-az*.

concept 'has carried away', as completed action, might have been felt more acutely than before. With *|rāte|* 'has run' (present *|ret-|*) already in the language, as indicated by comparative reconstruction as well as the form attested in the corpus, *|bāre|* (present *|ber-|*) might easily have been formed on this specific analogy.³⁵ The creation of *|ro bāre|* would have been especially favoured in a language in which it was not possible to create a verb with perfect meaning by prefixing **(p)ro* to forms derived from PIE aorists or Narten imperfects.

§33. An initial reason to favour the one-language hypothesis and see some deficiency in the explanatory value of the two-language hypothesis is that the Indo-European features in the corpus are not confined to the names, as has been recognized repeatedly (e.g. De Hoz 1989, 534; Untermann 1997, 165; Koch 2013d). This point has been recognized even in the context of arguments leading to conclusions favouring a two-language explanation. For example, Villar has written:

A syntagm like **ak^oosioś nařk^eeti** (Untermann 1997, J.56.1) seems undoubtedly to be a funerary formula from an Indo-European language with a thematic nominative singular anthroponym followed by a third person singular verb, also with thematic inflexion. (2004, 264)

§34. This observation implicates a great deal of the matrix language, because 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 (Villar 2004, 264): **nařk^eenii** (J.2.1, J.21.1), **n[ar^ke]enii** (J.6.1), **nařk^eenti** (J.12.1, J.16.1, J.17.2, J.18.1), **[n]ar^kenti** (J.1.5), **na]řk^eenti[** (J.4.3), **n(a)řk^eenii** (J.11.1), **n]ar^kenii** (J.11.3), **na]řk^eeni** (Corte Pinheiro) **nař]k^eenii** (J.19.1), **nařk^ee:n:** (J.23.1), **n[a]řk^een** (Cabeza del Buey IV), **na[ř]k^een** (Monte Gordo), **nařk^eenai** (J.7.1, J.55.1), **]nařk^eeuu[** (Corte do Freixo 2). **nařk^eenti** and these variants make up 276 of the 1752 signs of the 72 most readable SW inscriptions mentioned above (§10). These forms thus constitute 15.8% of the corpus.

35 Note that Villar explains Celtiberian **kombalkez** (Botorrita K.1.1) similarly as a perfect form without reduplication and with the preverb **kom-** and $\bar{a} < *o$ in the root, replacing *e n* the present stem **belg-* (Villar 2005, 344). In this explanation the final **-z** represents an older **-t*, the marker of the 3rd person singular secondary ending, innovatively taken over from the present/imperfect system or aorist to the perfect. With the SW writing system, it would be impossible to tell whether Tartessian had shared this innovation. **ua-rat^e** could represent *|ua rātet|* and **t^e-ro-b^aare** could represent *|de ro bāret|*.

§35. Villar’s identification of **nařk^eeti** as a verb with thematic inflexion might lead us to expect a corresponding 3rd plural ****nařk^oonti**. There is more than one possible explanation as to why ****nařk^oonti** does not occur. By the era of the SW inscriptions, many of the Indo-European daughter languages were no longer maintaining the original distribution *-e-* and *-o-* of the old thematic paradigms. For example, also within the Italo-Celtic domain, the Sabellian (Osco-Umbrian) languages of Italy extended the athematic ending **-enti* beyond its original range (as suggested to me by Peter Schrijver).³⁶ A tendency like the Sabellian is possibly reflected in the remains (exceedingly meager for verbs) of the pre-Roman languages of the western Iberian Peninsula in Lusitanian DOENTI, usually translated ‘they give’. Another possibility is that 3rd sg. **nařk^eeti**, 3rd pl. **nařk^eenti** reflects an Indo-European *-e_i^e/o-* present inflection that has developed like Latin *monet, monent* (as suggested to me by Dagmar Wodtko).³⁷

§36. Leaving aside a possible etymological connection with Greek *ναρκάω* ‘grow stiff, numb, dead’ < Proto-Indo-European **(s)ner-* ‘bind, fasten with thread or cord’ (Koch 2013a, 202), a different derivation of **nařk^eenti**, &c., follows suggestions made independently by Christopher Gwinn and Octavià Alexandre. This etymology begins with Indo-European **ner-* ‘under, below, left’ (Pokorny 2002, 765–6; Mallory and Adams 2006, 290, 293, 305), the source of Germanic ‘north’, Umbrian *nertru* ‘left’, Greek *νείπος* ‘lowest’, Homeric *νέρθεν* ‘below, under’ and *ἔνεποι* ‘those below the earth, shades, nether gods’, thus an apt semantic domain for a funerary formula. **ner-* does not occur as Celtic, but is probably related to the fossilized Old Irish preverb *ne* ‘down’ (on which see McCone 2006, 181; cf. Old Welsh NITANAM ‘underneath me [this inscribed stone]’; cf. Pokorny 2002, 766).

Isolating a directional element **ner-*, perhaps in its zero-grade **nr-*,³⁸ leaves **-k^e-** as the verb. Indo-European **k^éi-* ‘lie down’ (Pokorny 2002, 539; Rix 2001, 320) again fits semantically. That root is the source of Hittite *kittari* ‘lies’, Vedic *śáye* ‘lies’, Greek *κεῖμαι* ‘lie, be placed, rest’. So it clearly belonged to Proto-Indo-European and has dropped out of the branches where it is not attested. The synonymous root **leg^h-* could take the place of **k^éi-*.³⁹ The Greek verb is common in epitaphs and, as Mallory

36 This is the reverse of the pervasive trend in Latin, where, for example, *sunt* reflects a notional **s-onti* replacing Proto-Indo-European **s-enti*.

37 The last possibility would be consistent with the reading]**nařk^eeuu**[(Corte do Freixo 2) as the 1st person singular of the same paradigm, but it is not impossible that this brief fragment should be segmented as two words]**nařk^e euu**[.

38 On the development of PIE **nr* > Celtic *nar*, cf. the Early Welsh byforms *nâr¹* ‘lord, chief, leader’ and *nêr* ‘lord, chief (frequently of God)’, both derived from PIE **h₁n^{ér}* ‘man, leader, hero, &c.’ by GPC. *nâr¹* probably continues one of the zero-grade forms of the inherited ablauting paradigm. Eska’s (2013a 63 N) expectation that **nr-k-* should give PC ****drik-** is not supported by comparative evidence. In any event, ****nri-** is impossible in the attested Celtic languages.

39 Note that **lak^eenti** occurs in the long (57 signs) and complete text of the inscription

and Adams remind us, occurs in Homer for fallen heroes: κεῖται Πάτροκλος ‘[here] lies Patroklos’ (2006, 296; *Iliad* 23.210). **kéi-* has not been found in the Celtic verbal systems, but the root occurs in the standard etymologies of Old Irish *coem*, Old Welsh *cum* ‘dear’ and Old Irish *céile*, Middle Welsh *cilyd* ‘fellow, companion’ (Vendryes, *Lexique* s.nn. *cáem*; *céile*). Forms related to the latter pair possibly occur in **kⁱielaoe**: (J.11.1), **k^eeloia** (J.57.1), and **k^eeilau** (Cabeza del Buey IV) in the SW corpus (Koch 2013a, 185).

As well as working semantically, the interpretation **na^rk^e(-)** < PIE **ner-* + **kéi-* (‘below’+‘lie down’) would explain why the vowel **e** occurs in both 3rd sg. **na^rk^eeti** and 3rd pl. **na^rk^eenti**. In origin it would have been neither the thematic vowel nor part of the athematic plural ending, but belong to the root, which was always accented and *e*-grade in Proto-Indo-European (Rix 2001, 320). In Vedic and Greek, the simplex verb is inflected as a middle without active forms. Two of the forms in the corpus, **na^rk^eenai** (J.7.1; J.55.1), resemble the Homeric middle κεῖνται ‘they lie down, they rest, they are placed’ (cf. Koch 2013a, 245). But it would be surprising for a reflex of the Late Proto-Indo-European middle to look like this in Celtic or Italo-Celtic; most Indo-Europeanists would expect a form with an *-r(-)*.⁴⁰

Further evidence that could be explained by deriving **na^rk^eenti**, &c., < **kéi-* is that one of the most common of the attested forms is **na^rk^e** (J.1.1, J.7.8, J.27.1, J.57.1, S. Martinho, J.17.1, J.26.1). **na^rk^e** might be seen as an abbreviation. But systematic truncation of the formula words is not otherwise a feature of SW epigraphy. On the other hand, with the present etymology, it is possible that the seven instances of **na^rk^e** represent the complete verb form. Unlike the Greek present 3rd singular κεῖται and the later Vedic form *śéte*, which look like cognates although they’re probably parallel innovations, the earlier Vedic 3rd singular present tense form *śáye* lacks *-t-*. This must be an archaism. It implies a Late Proto-Indo-European **kéio-i*. Using internal reconstruction, Jasanoff (1994) proposes a succession of earlier Proto-Indo-European forms going back to the common ancestor of the middle and perfect: **kéi-o-i* < **kéi-o-r* < **kéi-e-r* < **kéi-e*. With this background in mind, **na^rk^e** can be understood as a fully represented 3rd person past-tense form: either with the Indo-European secondary ending, **|nar-kē-t|* ‘lay down below [this stone]’, or perfect **|nar-kē-e|*⁴¹ ‘has lain down below’. **na^rk^e** would thus contrast with present active (with stative

from Alcalá del Río (J.53.1). No form of **na^rk^eenti** occurs in that text. So arguably **lak^eenti** is taking its place as its semantic equivalent in an innovative variation on the formula.

40 Of course, a primary 3rd plural middle ending **-nto-i* could have been formed from secondary **-nto* independently of Greek and Indo-Iranian.

41 The model here is that the Proto-Indo-European vocalism of the accented *e*-grade has been preserved and that PIE **e_j* has given PC **ē* by the monophthongization common to all the Celtic languages. However, this is not necessarily the only possibility. In a long *ā*-preterite from perfect of the well-attested Celtic type, **kāi-e* could have given ***-k^ee-e** (written **-k^ee**) by the Tartessian monophthongization (see Koch 2011, 134–6).

meaning) **naṛk^eeti** ‘lies down below’, plural **naṛk^eenti**. **naṛk^e:n**, **n[a]ṛk^een**, and **na[ṛ]k^een** can thus be identified as past-tense 3rd person plural forms with the secondary ending, **|nar-kē-nt|* or **|nar-kē-ant|* < **-kēi-nt*.

§37. If the very common **naṛk^eenti** and its variants are recognized to be, like **naṛk^eeti**, Indo-European verbs, the same analysis follows for the forms **lak^eenti** (J.53.1), **t^ee-b^aanti** (MdC), and **t^{ee}-b^aarenti** (J.23.1). These same stems that are seen inflected as Indo-European verbs are found with repeated prefixes **t^ee**, **ro**, and **ar** which correspond to preverbs that are amongst the most common of Old Irish (*de, ro, ar*). Two of those three also show the diagnostically Celtic loss of Proto-Indo-European **p*. The relevant examples include **t^ee-b^aar[e]** (J.16.1), **t^ee-b^aare** (J.1.3, Vale de Águia), **t^ee-b^aare** (J.18.1), **t^ee-b^aere** (J.7.8), **t^aae-b^aare** (J.14.1); **ro-b^aare** (J.1.1, J.12.1, J.18.2, MdC), **ro-b^aare** (J.16.3); **t^{ee}-ro-b^aare** (J.18.2), **t^ee-ro-b^aare** (J.1.1, J.12.1); &c. The segmentation of preverb from verb, indicated with hyphens here, is clearly implied by the patterns of syntactic recombination of these elements within the corpus.

§38. These forms are construable as Indo-European verbs within the syntax of the SW texts: **naṛk^eenti** is commonly in statement-final position. After **naṛk^eenti**, &c., **t^ee-ro-b^aare** and its variants represent the most common of the formula words of the corpus. The full form of the epigraphic formula, giving each element in its most usual form and position, is **uarb^aan t^ee-ro-b^aare b^aa-naṛk^eenti**. The varying versions of this formula are most commonly placed after the naming phrase. Within the 72 inscriptions examined statistically here, variants of the formula account for 565 signs of the total of 1752 or 32.2% of the corpus and 49% of the matrix language, i.e. excluding the names (counted as 596 signs or 34% of the corpus; see §10 above). Therefore, particularly in the light of the syntactic patterns discussed here, it is misleading to state flatly that the matrix language does not look Indo-European. When taken together with the evidence of the short words resembling Celtic preverbs repeatedly prefixed to stem formations otherwise occurring inflected as IE verbs in the corpus, and especially the nine instances of **ro**, that this might be a Celtic language is at least a somewhat obvious possibility.

VI. Proper names and the Ancient Celtic languages

§39. Greater difficulties with the matrix language—in contrast to the names—can be anticipated, even if both are Celtic. Overwhelmingly what

we know most about Continental Celtic and the Insular Celtic languages in their ancient manifestations is proper names as occur in Greek and Latin texts and short inscriptions. So, for example, the long list of names making up the greater part of Botorrita K.1.3 presents fewer difficulties in analysis compared with the sentences of Botorrita K.1.1. Even with short inscriptions written continuously in the better-known Gaulish, there are fewer analogues for the parts of texts that are not taken up by names. For example, the three words of the formula ΔΕΔΕ ΒΡΑΤΟΥ ΔΕΚΑΝΤΕΜ have no exact analogues in fully attested medieval Goidelic or Brythonic, though found together with names that are undoubtedly Celtic. It took many decades of incorrect Celtic explanations for this formula before a more widely acceptable interpretation came forward (Szemerényi 1974).

It is unusual to find a verb in Continental Celtic like *uediiumi*, the second word of the inscription of Chamalières, which has an exact cognate in Old Irish *guidiu* and can also be understood as a direct inheritance from Proto-Indo-European. Even in this case, it remains uncertain whether Gaulish *-mi* is an affixed subject pronoun (like Welsh *-(f)i*) or the Indo-European athematic ending added to the thematic (as in Old Persian *jadyāmiy*) and several completely different etymologies have been proposed for *uediiumi* (Delamarre 2003, 309–10). We have no sentences written in Ogamitic Primitive Irish, probably only a very few in pre-apocope Brythonic (Mullen 2007). For the Continental Celtic languages, we have no sentences in the Galatian of Asia Minor.

§40. The Gaulish verbs that are found in recurring formulas—such as *karnitu*, ΔΕΔΕ, *ieuru*, and *auot*—cannot be so easily paired up with Insular Celtic cognates, on the one hand, or traced back directly to Proto-Indo-European, on the other. What we can see of the verbal systems of Continental Celtic inevitably raises the thorny question of how to reconstruct a Proto-Celtic verbal system that was the source of both the attested Continental forms and the system observable in Old Irish. Deriving individual verb forms from Indo-European is less often problematic. But, unlike common developments in phonology and in the lexicon as reflected in onomastics, it is a serious challenge to find a core in the domain of the verbal system of shared innovations defining the Proto-Celtic stage. The possibility must be considered that the Celtic branch was not a homogeneous and dialect-free linguistic unity, but a continuum of mutually intelligible pre-Celtic Indo-European dialects that became Celtic through sharing innovations (cf. the approach of Garrett 1999; 2006).

§41. Of the personal names cited above (§14), analogues can be found for several of them in Celtiberian and other Ancient Celtic languages. But the corresponding forms are to be found most frequently amongst the pre-

Roman names of the western Peninsula. This generalization includes **aark^uui, aib^uuris, alb^ooroi; anb^aatⁱia, b^ootⁱieana, eb^uu^oroi, t^aalainon, tⁱirt^oos, soloir, lok^oob^o, niirab^o, tⁱilek^uurk^uu, t^uurea**, and Ἀργανθωνιος. Particularly rich sources for onomastic comparanda are the collections of Albertos 1985 and Vallejo 2005. On the basis of its geographical distribution, this name stock can be seen as reflecting an earlier stage of the ‘Western Hispano-Celtic’ attested in the mixed language epigraphy of the earlier Roman Period.

Because this is also the region of the Lusitanian language (which is Indo-European, but most often regarded as not Celtic) and because most of the pre-Roman names do not include either clear-cut Celtic or clear-cut non-Celtic phonological features, it is often necessary to employ some such unsatisfying provisional label as ‘Western Hispano-Celtic or Lusitanian (HC/Lus)’ or ‘Hispanic Indo-European (HIE)’.⁴² However, in many instances, even when exclusively Celtic affinities cannot be demonstrated by sound law, Celtic affiliation is suggested because identical or similar forms are found in known Celtic-speaking regions of the ancient world outside the territory of the Lusitanian language.

§42. From the earlier Roman Period, when these comparanda are attested, we have no continuous sentences written in the languages of those groups called Celtici in the north-west of the Peninsula or those groups also called by the same name in the south-west.⁴³ The territory of the latter overlapped with the geographical distribution of the SW inscriptions. The matrix language of the inscriptions of these western regions at this period was Latin. We would not expect there to be much to compare between the pre-Roman names of the western Peninsula and the matrix language of the SW inscriptions, even if the latter was an earlier form of the same language. However, in a few cases, such as the personal name ROTAMVS (a superlative and/or ordinal based on *ro* < **(p)ro*, from São Martinho de Mouros, Viseu, north Portugal; see Vallejo 2005, 466)⁴⁴ and the place-name Ὀυαμα ‘highest’ (another Celtic superlative based on a preposition), there are nevertheless close analogues in the SW corpus—namely the recurring preverb **ro** < **(p)ro* (which is probably also the base of **rob^aa** |*roamā*|

42 Even the five longer inscriptions that can be thought of as the core of the evidence for the Lusitanian language contain names (for example, AMBATVS, CAELOBRIGOI) that must be understood either as Celtic loanwords or, less probably, as evidence that Celtic and Lusitanian were so similar as to share a common inherited name stock and to be often indistinguishable. See Wodtko 2010.

43 The Κελτοί that Herodotus wrote of in the mid 5th century BC, as living beyond the Pillars of Hercules and neighbouring the Κυνητες (Histories 4, 49) or Κυνησιοι (2, 34) who were the westernmost people of Europe, would be close in time and space to the SW inscriptions themselves.

44 Cf. Vedic *prathamā* ‘first’, *pratamā* ‘foremost’. I interpret **rob^aa** (J.128.1) as |*roamā*| ‘foremost woman, eldest female relative’, that is a more archaic feminine form equivalent to ROTAMVS.

‘foremost woman’ =? ‘eldest daughter’), **uab^aan** (J.16.5) |*uaman*|, and the formula word **uarb^aan** |*uara_aman*|.

§43. It follows from the preceding sections that, if there was a corpus of inscriptions comprising proper names within complete sentences—both written in the Western Hispano-Celtic of the Early Iron Age—, that the names and name elements could be recognized relatively easily on the basis of the names attested in the region and the wider Celtic world in subsequent centuries. It also follows that it would be more difficult to recognize the forms that were not names. The challenge would be formidable, taking into account features of the writing system such as the usual *scriptio continua* and a script that does not distinguish voiced from voiceless stops. It would be unrealistic to expect many of the verbs to resemble closely mechanical reconstructions of old Irish verbs, except for occasional fortuitous matches like *uediiumi* ~ *guidiu*. As I have proposed, it is likely that Tartessian **uarat^e**, corresponding to Early Welsh *gvaravt* ‘helped, rescued, delivered’ and Old Irish *fu-rráith* < Proto-Celtic **u(p)o-rāte*, is such a match. I think that **oret^o** (J.4.1) is possibly the genitive singular of the corresponding verbal noun. So few Continental Celtic verbs are known that the odds would be against finding the exact cognate of one already attested. We do not, to my knowledge, yet have full cognates (root, stem form, inflexion) of a verbal form attested in both Celtiberian and Gaulish. Nonetheless, this is possibly what we have in Tartessian **eert^aaune** alongside Celtiberian **uertaunei**. There is therefore a foreseeable pitfall of misinterpreting a corpus written in an Ancient Celtic language as comprising familiar Celtic names embedded in an otherwise unknown language. From recognizing this risk, it follows that sufficient effort should be put into exploring the one-language hypothesis before rushing to embrace the alternative on first running into difficulties.

VII. What do the inscriptions say?

§44. As explained previously, the translations of the SW inscriptions that have been offered in my publications are tentative and provisional. Their chief purpose is to allow non-linguists to follow the argument to some degree even if they are not familiar with the specialist terminology of the proposed identifications of verb forms, noun declension, &c. It was also hoped that offering explicit translations might stimulate advances in interpretation more effectively than if the meanings of the texts had been merely implicit in dense linguistic notes. A reaction along the lines of ‘this translation is unconvincing therefore Tartessian is a non-Indo-European language’ is a non sequitur, but at least reveals assumptions about what an

inscription in an ancient Celtic language should say. Thus, the first review states:

In the Celtic world, funeral inscriptions tend to be fairly straightforward statements. (Eska 2013a, 63)

Similarly in the second:

It is worth noting that although the funeral statements of the accepted Continental Celtic languages are straightforward and focus upon identifying the deceased, sometimes the dedicator and funerary operations performed . . . , many of K[och]'s translations are quite fantastic. (Eska 2013b 71)

Although some researchers do not accept the concept of a 'Celtic world' at all, it is arguably valid in some domains, for example, where common inherited vocabulary is used for parallel ideas and institutions. However, until we find Celtic speakers using Roman letters and Latin words during the Roman period, there is no unified meaningful category 'funeral inscriptions in the Celtic world'. The Celtic epigraphic traditions in Palaeohispanic script, Greek script, and derivatives of Etruscan script in northern Italy originated independently. They result from three episodes of contact in different places and times between Celtic speakers and three different literate civilizations, speaking three completely unrelated languages. Even within the Palaeohispanic category, the SW inscriptions and Celtiberian inscriptions are not in a direct parent-child relationship. The Celtiberian semisyllabary is derived from the 'Levantine' script used to write the non-Indo-European Iberian language.

§45. The first review gives examples of funerary inscriptions from 'the Celtic world', including the Celtiberian inscription from Ibiza, which is all names, and the double Cisalpine Gaulish inscription of Todi, which has names together with the verb KARNITU (twice) and two nouns (one in each version of the repeated sentence), which probably refer to parts of the burial: LOKAN and ARTUAŚ. LOKAN, incidentally, resembles the Tartessian form **lok^oon**, which occurs twice in the corpus—Fonte Velha VI (J.1.1) and Medellín (J.57.1 = T1, 86H/EN12-1).⁴⁵ Both Peninsular sites are necropolises of the Early Iron Age. Therefore, this might be an item of Common Celtic funerary vocabulary. Even so, it must be remembered that Celtic speech is a common inheritance as are concepts of burying the dead, but writing about them is not a shared inheritance.

Contrasting such examples as Ibiza and Todi with the SW corpus, we turn first to epigraphic formula, discussed above (§38): **uarb^aan t^e-ro-b^aare b^aa**

45 I also think it likely that Tartessian **k^aárner** (J.7.2) is related to KARNITU 'built a funerary monument'.

naṛk^eenti. This formula is not a name, nor, most probably, is any part of it a name. By looking through the corpus, we find elements that recur as words in different combinations, and parts of these words that change—suffixes, prefixes, and inflexions. All told, there are about 11 morphemes in the formula. It follows that the idea expressed is something more complex than *hic iacet* or KARNITU LOKAN. My working hypothesis for this SW epigraphic formula is that, putting it into a more scrutable transcription, |*u^{ar}aman de-ro-bāre ma narkenti*|, means ‘... has carried away to the ultimate (place/state/being) so they now lie down below’. In other words, this grave and/or death and/or immortalization has carried away (perfect tense) the named deceased to ‘the highest’ (feminine singular), so now (present tense), they lie down under [this stone that you’re reading] (see further Koch, 2013f).

§46. As discussed above (§§20–23), the tradition of the SW stelae with writing develops from, and overlaps with, that of the warrior stelae of the Late Bronze Age. The iconography of those preliterate stelae includes shields, spears, swords, helmets, chariots and horses, brooches, mirrors, combs, and lyres. As many writers have recognized, they represent the heroic ideal. And as Ruiz-Gálvez has argued they belong to the cult of immortalization and divinization of heroes. With their stick-figure heroes, weapons, horned helmets, chariots, and musical instruments, they bear an obvious resemblance to the Bronze Age rock art of Scandinavia, which are often seen as having narrative content related to myth. With the interpretation of **naṛk^eenti** as ‘they now lie down below’, the SW formula **uar^aan t^e-ro-b^aare ba^a naṛk^eenti** moves closer to the prosaic message expected by Eska. However, when the SW inscriptions are recognized as the successors of Late Bronze Age warrior stelae and the Middle Bronze Age *alentejanas*, a range of possibilities should be considered.

VIII. On the writing system and the sound system

§47. The SW script is the earliest writing system used for an indigenous language in western Europe and the most primitive form of the Palaeohispanic semisyllabaries or something close to their original prototype. It is therefore hardly surprising that there remain challenges and uncertainties concerning the origin of the script and its phonetic basis, as well as the phonology of the language or languages that it represents.

Eska’s first and second reviews take an interest in a remarkable characteristic of the script, known as ‘redundancy’, according to which stop consonants are usually written with a different sign depending on which of

the five vowels followed. As voiced and voiceless articulations for the consonants are not distinguished in the script and there is no labiovelar series, there are therefore a total of 15 signs for the stops, conventionally represented with superscript vowels in Romanized transliteration: **b^a b^e bⁱ b^o b^u, k^a k^e kⁱ k^o k^u, t^a t^e tⁱ t^o t^u**. The resulting system is an alphabet (one sign : one phoneme), but of a peculiar redundant kind that might be called a ‘pseudo-semisyllabary’.

In Koch 2013a and Koch 2011, I suggested that there might have been phonetic basis for this orthographic ‘vowel quality’, which could have become phonemic, at least in some contexts. Eska objects that it is unlikely or impossible for a language to have full fivefold contrast of secondary articulations.

What, for instance would have been different in secondary articulation between the phonemes represented by <t^e> and <tⁱ>. Those represented by <t^a> and <t^u> might be different from each other, but surely the phoneme represented by <t^o> would pattern with one or the other. And since <t^a>, for example, only appears before <a>, why should we think of separate phonemes anyway? (2013a, 59; cf. 2013b, 69)

For readers picking up the debate here, I should clarify that the idea of a five-way phonemic contrast, i.e. /C^a C^e Cⁱ C^o C^u/, running through the Tartessian consonant system is not a strongly held view of mine, was never an essential pillar in the interpretation of the language as Celtic, nor is it the only possible explanation for the redundant agreement between signs for the stop consonants and those for the following vowels. At present, I see three possibilities. The first two will be elaborated in following sections, and the third should be self-explanatory.

- (1) The SW signary, including the redundancy feature, can be understood as an outgrowth of known writing systems in use in the eastern Mediterranean at the period of intensifying East–West contacts up to and including the early stage of the Phoenician colonization, c. 1150–700 BC. The a-be-ka-tu does not reflect features of an indigenous language or languages of the Peninsula.
- (2) The signary was devised specifically to write an indigenous language or languages of the Peninsula. The redundancy feature can thus be seen as related to the strong observable tendency in the SW inscriptions for segments to assimilate to following vowels: e.g. simplification of diphthongs, *i lowered to **e** before *a(:), and *u assimilating to a following **e, i, o, or u**.⁴⁶

46 Because these assimilations are such a pervasive pattern in the observable phonology of the corpus, the criticism is rejected that the derivation **eert^aaune** (J.55.1) < ***u**ertaunai (cf. Celtiberian [**u**]ertaunai) is ‘unacceptably ad hoc’ (Eska 2013a, 60). Note also **oofoire** (J.19.1) ‘female relative of **oofoir**’ < *U(p)er-*uiriāi*, **leb^ooiiire** (MdC) <

(3) Some combination of (1) and (2).

§48. Further to possibility §47(1), it must be remembered that the SW writing was not invented ‘from scratch’, so to speak, in order to represent an indigenous language, whether Iberian or an Indo-European dialect. Its main inspiration as reflected in the form of the signs and their order, as seen in the Espanca signary, was a West Semitic alephat. If the Greek alphabet was then known in the west—and if it even existed yet—there are no features of the SW signary that require the Greek alphabet as their source (cf. de Hoz 2010, 495–500).

In the a-be-ka-tu, vowel signs have been added to the alephat that do not look like the vowels of the Greek alphabet, with the exception of **a**. The sign for **a** resembles forms of Greek α and Semitic aleph equally. The other vowels have been added in ways that not only do not implicate the Greek alphabet by their form, but also do not implicate it by structural principles. This double representation of the vowels—both with five vowel letters (**a e i o u**) and 15 stop+vowel (pseudo-)syllabograms (**b^a b^e bⁱ b^o b^u, k^a k^e kⁱ k^o k^u, t^a t^e tⁱ t^o t^u**)—was the basis of the distinctive hybrid character of all the Palaeohispanic (pseudo-)semisyllabaries: SW, South-eastern Iberian (Meridional), North-eastern Iberian (Levantine), and the Celtiberian semisyllabary.

As a structural feature (though not as a matter of the form of signs, which can be derived from the alephat to which some simple geometric shapes were added) the hybrid character of the Palaeohispanic scripts raises the possibility of influence from the syllabaries of the Aegean or Cyprus. Linear B is an unlikely source for the idea of syllabograms for several reasons. First, it went completely out of use in the 12th century BC, so was probably too early to have contributed to formation of Palaeohispanic script. Furthermore, there is not enough evidence for direct and intense Aegean contact with the Iberian Peninsula in the age of Minoan and Mycenaean literacy. And the structure of the series of stop+vowel syllabograms in Linear B, of which there were 25, is not that close to that of the Palaeohispanic series of 15.

On the other hand, Cypriot syllabic writing continued from the mid 2nd millennium BC until the 3rd century BC. And that system represented the same 15 stop+vowel combinations as the Palaeohispanic scripts—no more, no less. Cyprus was also heavily involved in the growth of eastern Mediterranean contacts in Iberia during the Late Bronze Age and Early Iron Age. Much of the Phoenician commercial and colonial activities in the west came by way of Tyre’s colony at Kition in Cyprus. The Cypriot syllabic script and alephat were used in Cyprus side by side for centuries as badges of

*Lemo-ııroi.

distinct indigenous Cypriot and colonial Semitic identities (Sherratt 2003; cf. Olivier 2008).

Therefore, it is likely that entrepreneurial mariners coming from Cyprus during the Late Bronze Age to Early Iron Age, and possibly their partners in the Peninsula, would have known both writing systems. A literate Cypriot⁴⁷ would have been conditioned to the idea that indigenous languages should have their own script. A thoughtful literate Cypriot could also have developed ideas about the best and worst features of the two scripts. But a new-fangled hybrid would have had no chance of catching on in Cyprus because the two scripts were well entrenched in their opposed sociocultural domains. For the alephat, a lack of vowels as a drawback was balanced against the advantages of 22 signs that were easily learned and recognized and easily written on various media. The syllabary had about 55 signs that were not so easily written or distinguished, represented final consonants and consonant clusters inaccurately, and did not distinguish voiced, from voiceless, from voiceless aspirate stops (e.g. δο το θο all written with one sign now transliterated *to*).

In the Aegean Iron Age, Linear A and Linear B had been completely forgotten. Therefore, when the Phoenician alephat arrived, the alphabetic principle (one phoneme : one letter) was extended to include vowels. But pre-Phoenician indigenous Cypriots held on to their syllabic script. Conditioned in this way, a Cypriot might well have failed to grasp that the alphabetic principle was compatible with vowels. In other words, a Cypriot ignorant of the Greek alphabet might think that it was necessary not only to add vowel signs but also structural features of a syllabic writing system.

If what is suggested here was the background of the inventor of the first Palaeohispanic (pseudo-)semisyllabary, the script's failure to distinguish voiced from voiceless stops need not reflect the phonological system of an unknown Palaeohispanic language. A user of the Cypriot syllabary would be conditioned to accept this as natural, essential to preventing the syllabary becoming even more unwieldy, and possibly even as a feature appropriate to writing a pre-Phoenician indigenous language. The failure to reflect voicing was no worse for writing Hispanic Indo-European languages or Iberian than for writing Greek.

That leaves the question of why it was only the stops that were represented with pseudo-syllabograms (cf. Eska 2013a, 59 N 5). The chief reason was, I think, that the alephat was seen as the better model script for the number of signs and their form. The Palaeohispanic script's inventor might thus have stumbled upon an alphabetic principle having already allocated so many signs to the most prominent subgroup of consonants. There is of course no certainty that the inventor would either have been taught or worked out for himself or herself the opposed categories of vowels

47 Or a studious native of the Peninsula who had absorbed both writing and ideas about writing from Cypriots.

and consonants as we now arrange them. Therefore, the stops might understandably have formed a distinct category of sounds on their own, standing outside any larger more inclusive category that did not also include vowels. In other words, the concept of the consonant might have been absent from the linguistic doctrines and original thinking of the inventor of the SW script.

§49. Further to possibility §47(2), the combined evidence for Tartessian phonology canvassed in *Tartessian 2* (and not recapitulated here) shows that most of the post-Proto-Celtic innovations fit the general characterization of segments assimilating to the articulation of the following vowels, either directly contiguous or across an intervening consonant. Therefore, that the distinctive redundancy feature reflects a vowel quality of consonants would seem to be a possibility in line with observable phonological tendencies of the language of the SW inscriptions. Eska not only objects to the lack of parallels for a system with five phonemicized variant articulations, but also asks, reasonably, why vowel quality of consonants would be noted if **t^a**, &c., was almost always followed by **a**, &c.? Indeed, if there were no exceptions to this distribution, why would the vowel quality be phonemicized?

A point to consider here is that literacy usually arrives with bilingualism, which can result in an altered perception of sounds that had been allophonic variants in one language (the preliterate one) but contrasted phonemically in the second language bearing the writing system. Such contrasting perceptions would be especially likely where one language had heavily palatalized and/or labialized allophones of consonants. But in the case of the SW corpus, such an explanation may be unnecessary, as there are traces of phonological innovations that could have resulted in phonemicized vowel quality of consonants independently of bilingualism and literacy.

Where the lowering of **i* or **ĭ* to **e** before *a(:)* occurred in Tartessian with an intervening consonant other than a stop, there can be no trace of whether the articulation of the consonant induced by the earlier vowel had become phonemic in its changed environment or not. So, for example, with **meleśae** (J.15.1) < **melit'sāi*, **(?ϕ)at^aaneat^e** (J.12.1) < **(p)atanĭatei*, **k^aaśet^aana** (J.53.1) < **kassidannā*, and **t^uurea** (J.7.8) < **Turĭā*, we cannot tell whether **l**, **n**, **ś**, and **r** acquired phonemicized *i*-quality as a result of the lowering of the **i* or **ĭ*. However, in the case of **mut^uuirea** (J.1.5), the orthography suggests phonemic palatalization of the **r** before an **e** derived from an earlier **ĭ* by lowering before **a**, i.e. */mutur'eā/* < **Muturĭā*.

With stops, the redundancy offers more clues: for example, **b^ootⁱieana** (J.18.1) = */bōd'eanā/* < **Boudĭanā*, **b^ooara** (J.2.1) = */bōāra/* < Pre-Celtic **bōra* 'I have carried'. In the case of **t^eeaiona** [(J.4.3), I think that **ai** represents a monophthongized long vowel after the reflexes of **ai* and **ei* had fallen together or nearly so. Thus, the orthography shows that the initial dental of the reflex of **Deiwonā* had *e*-quality or, at any rate, a phonemic quality different from what it would have been had the original form been ***Daiwonā*.

Eska is probably right that it is unlikely that a fivefold phonemic distinction could run through the entire sound system. However, there may have been a significant proportion of phonetic environments in which consonants could contrast phonemically owing to differing vowel quality alone. Once the principle was grasped that distinctive consonant qualities agreed with vowels, it would have been easier to represent it—and formulate a teaching doctrine about it—as a fivefold distinction within a writing system with five vowels.

Returning to possibility §47(3) above, the fivefold redundancy could have been an especially natural result if the inventor of the Palaeohispanic (pseudo-)semisyllabary had the model of Cypriot syllabary in mind and had also recognized that the SW language had instances of phonemic vowel quality.

IX. The loss of **p* and the Celtic homeland

§50. The question of where Proto-Celtic emerged from Proto-Indo-European (or Proto-Italo-Celtic) must first be approached theoretically before the evidence favouring one time and place or another can be evaluated. The following passage from Eska's first review reveals some common concepts.

... under K[och]'s scenario, whereby Celtic originated in southwestern Iberia and spread thence, there should be no trace of pre- or intervocalic **/p/* anywhere in Celtic. . . . If proto-IE **/p/* was lost before the Celts left Iberia how could it still be continued in [**uvamoKozis** and *Helvētiī*]? Its complete loss initially and intervocalically in the Celtic of Iberia instead suggests a migration into Iberia. (2013a, 61)

Arguably, too much emphasis has been laid on the weakening and loss of Proto-Indo-European **p* as the innovation that defines the emergence of Celtic, but for present purposes, it will do. Eska's formulation above assumes migration, probably mass migration. Indo-Europeans, living in what was to become the Celtic homeland, lose PIE **p*. We then call their language Celtic and call them Celts. These Celts then spread from this homeland. In taking up new lands, they either replace indigenous peoples who had spoken other languages or somehow impose their language on non-Celts, making them Celts too.

If we take into account the archaeological record and the dialectology of modern languages, a different model appears more likely (see Koch 2013c; cf. Garrett 1999; 2006). We start with communities in contact across a region in later prehistory (cf. the model of Wodtko 2013). Many of the groups within this interconnected sociocultural area speak dialects of Indo-

European, what may still be called Proto-Indo-European at this stage, rather than one of the defined branches as presently known. These dialects form a continuum with shallow differentiation and a high degree of mutual intelligibility. Thus the sociocultural area is also a linguistic area. Within part of that area—possibly a part of special economic and cultural importance—weakening and loss of **p* and some of the other changes common to the Celtic languages begin and spread. Individuals with high status and/or specialist ‘know how’ are on the move, acting as what may be thought of as a mobile élite. Larger groups—warbands and extended kindreds—are possibly moving as well. But we do not need to think on the scale of ‘the Celts leaving the Iberian Peninsula’ or ‘the Celts entering the Iberian Peninsula.’ The main process is not primarily culture change by migration, but linguistic innovations spreading across a continuum of mutually intelligible dialects through continuing and intensifying contact. The Atlantic Bronze Age of about 1250–900 BC would be one period when a well-defined sociocultural area included Ireland, Britain, Atlantic Gaul, and the western Iberian Peninsula.

§51. The principle that incomplete sound changes and variant phonological reflexes should be found in the homeland of a language is valid. This is why there is more dialect variation in the English of England than that of Australia. However, the point quoted in the previous section about ‘the complete loss [of PIE **p*] initially and intervocalically in the Celtic of Iberia’, in contrast to **uvamoKozis** and *Helvétīī* in central Europe, is misleading and invalid.

We have clear evidence in the place-names of the Iberian Peninsula for an even earlier stage. It is because it is an earlier stage, than that reflected in **uvamoKozis** and *Helvétīī*, that it is not conventionally called Celtic. So, we find side by side with Celtic, where **p* is lost as expected, evidence for other dialects that have otherwise developed from Proto-Indo-European in the same way as Celtic, but where *p* is either retained or has been softened to the voiced stop *b*.⁴⁸ For example, the Hispanic place-name element *paramo-* ‘exposed high ground’, etymologically is a superlative adjective derived from a preposition, precisely the same formation as in Celtiberian VERAMOS, VORAMOS⁴⁹ < **u(p)eramo-* and broadly synonymous with that word. But

48 Wodtko (2010, 359–60) raises the possibility that some Indo-European Palaeohispanic languages had [φ] from PIE **p* written as Roman *p* in our sources.

49 Following Correa, I think that the formula word **uarb^aan** (J1.2, J.3.1, J.4.1, J.21.1), also **uarnb^aan** (J.20.1), is cognate with Celtiberian VORAMOS, &c. It is consistent with a feminine accusative singular case form, [ɥar_aaman] < **u(p)er_amām*. Despite Eska (2013a, 60), I think the vocalism of the first syllable has arisen unproblematically by dissimilation as in Gaulish *uassus* alongside *uoθθos* and numerous other Gaulish and Brythonic examples (Koch 2011, 117–20). In the particular case of **uar(n)b^aan** [ɥar_aaman] a further factor favouring the vowel **a** in the first syllable is the synonymous and closely related form found in the South-western Hispano-Celtic place-name Ούαμα

paramo- has not lost *p*; VORAMOS has. The double place-name Σεγοντία Παραμικά in the Ptolemaic Geography (2, 6, 49) is situated north and west of Celtiberia. The first name Σεγοντία is well attested in Celtic. Παραμικά shows an adjectival velar suffix very common in the Celtic languages added to superlative *paramo-* (Ballester 2004b). Gaulish *uertamica* ‘excellent, superior’ (Lezoux) shows this same formation, but with loss of *p* in **u(p)er-tamikā*. Similarly, the ancient place-name *Bletisama* in the north-central Peninsula is today *Ledesma* and clearly cognate with Celtiberian *Letaisama*, all from **pletisamā* ‘broadest’. Here, **p* is lost or weakened then lost in Hispanic languages that agree with Celtic in the formation of the superlative (see further Villar 2004; Búa 2007, 20, 33; Koch 2013a, 261–2). In other words it appears that Ancient Celtic and its closest kin were in close quarters and interacting with each other in the Iberian Peninsula, as one would expect if the epicentre of the Celtic innovations had been in this region.

§52. Eska’s third review makes some distinctions in linguistic theory which are not well founded in my view, for example:

John T. Koch . . . embraces the idea that proto-Celtic did not descend directly from proto-Indo-European, but arose from the coalescence of a variety of Indo-European dialects in the Iberian Peninsula during the Bronze Age. Once formed, it [=?Celtic] came into contact with non-Indo-European Iberian,⁵⁰ which leads Koch to conclude that Celtic is Iberianised Indo-European ([Koch 2013c,] 137). (Eska 2013c)

I certainly do not embrace the idea that Proto-Celtic did not descend directly from Proto-Indo-European. This formulation implies that any Indo-European dialect that has acquired a linguistic innovation originating in another mutually intelligible Indo-European dialect is not a direct descendant of Proto-Indo-European. As a historical linguist, I am daily aware that the

Uama < **u(p)amo-* and the Latinized group name VAMENSI, applied to a Roman-period settlement (Salvatierra de los Barros, Badajoz; Falileyev 2010, 228) in the territory of the south-western Celtici, situated high on the massif within the bend of the Anas/Guadiana (Koch 2013a, 232–4). Another contributing factor would be the fate of *paramo-* in the dialects with weakened prevocalic **p*, where the form would have gone through a phonetic stage, prior total loss, as a pronunciation **[h^waramo-]* or something very similar.

50 The sequencing is confused here. If ‘it’ was already in the Iberian Peninsula why did it have to ‘form’ or ‘coalesce’ before coming into contact with Iberian? The sequence in my proposal is that Proto-Indo-European dialects extended to south-west Europe and were thus already in contact with *p*-less non-Indo-European, resulting in weakened **p* amongst bilinguals and their children in the contact area. Subsequently, weakened **p* spread through a continuum of IE dialects within a coalescing sociocultural area in Bronze Age western Europe. We can then start talking about ‘Celtic’ using the conventional definition, which hinges on this sound change.

English and Welsh I use are the result of a coalescence of dialects, but both languages are Proto-Indo-European's direct descendants, as the concept is usually understood.

The third review also quotes Ringe as follows and implying that what I have written is somehow at odds with this doctrine and therefore 'does not hold water':

'[E]xtensive structural convergence of languages, as opposed to mere word-borrowing or the adoption of a few superficial traits, turns out to be rare. . . . By contrast, dialects that are mutually intelligible can and do merge.' (Ringe *Language Log*, quoted by Eska, 2013c)

In fact I agree with Ringe on this. I do not think that Celtic is the result of Proto-Indo-European converging with Iberian and/or Aquitanian/Palaeo-Basque. Rather, I think a less drastic discontinuity is implied by the Celtic evidence (cf. Ringe et al. 2002, esp. 63–5): as Proto-Indo-European expanded in western Europe, some Indo-Europeans changed dialect and some non-Indo-Europeans changed language (cf. the scenario of Wodtko 2013). Amongst the latter, first-language speakers of Iberian and probably also Aquitanian learned Proto-Indo-European (or Proto-Italo-Celtic) as a second language. Some of these bilinguals then passed this second language on to the next generation as a first language carrying some substratum effects. Some of these effects, such as unstable articulations of **p*, then spread through mutually intelligible dialects of the sociocultural area in western Europe, including communities largely descended from first-language Indo-European speakers. Archaeological periods of observable dynamic interaction at the contact zone of the Atlantic and Mediterranean littorals in Iberia seem likely episodes during which the linguistic macro-families might interact in this way and then transmit substratum effects back to a monoglot hinterland along with other cultural innovations.

X. Clarifications

§53. There are further instances in which the reviews are not a reliable guide to what my publications said and other factual matters. An exhaustive list would be too much here. Interested readers should check the publications. The following are representative:

. . . John T. Koch states that, [1] at the beginning of recorded history, all linguistic records are consistent with a Celtic analysis and asserts . . . that [2] Tartessian is Celtic is an 'inescapable' observation ([Koch 2013b] p. 5), and that [sic] that [3] movement of key sword types from west to east

supports the notion that Celtic spread from the Iberian Peninsula ([Koch 2013b] p. 6) (Eska 2013c; numbers in square brackets added)

Point [1] in the passage above is nonsense. No one did say this or could say this. In point [2] my words ‘the Hispano-Celtic affinity of Tartessian’ have been replaced by ‘Tartessian is Celtic’ changing the meaning of the passage. The inescapable Hispano-Celtic affinity is largely a matter of the names that have been observed over and over again, as affirmed in Eska 2013b and 2013c. For point [3], the swords in question are the Ewart Park-derived Gündlingen type, the origin of which was in south-eastern Britain or the *transmanche* zone and thus have no bearing, at least not directly, on any idea about Celtic spreading from the Iberian Peninsula.

§54. The second review shows interest in the current research project, ‘Atlantic Europe in the Metal Ages (AEMA): questions of shared language’, concluding:

I note, in closing, that an announcement on the website of the British Broadcasting Corporation on 25 March 2013 states that K[och] has been awarded a grant of £689,167 by the United Kingdom’s Arts and Humanities Research Council to continue work on Tartessian, evidently on the grounds that it has already been proved Celtic. (Eska 2013b, 72)

As this sentence closely follows the mention of ‘the undoubted presence of Celtic onomastic forms in [Tartessian]’, one must take it that the reviewer does not consider that this by itself—even though it would amount to the oldest Celtic linguistic forms attested anywhere— would be worth investigating at a centre of Celtic research located in a country where a Celtic language is spoken. So long as doubts can be cast on the Celticity of the matrix language, the whole subject should be deemed sufficiently uninteresting not to merit further attention. Just Celtic names in south Portugal in the 7th century BC, so what?

The reviewer has found a meaning in the BBC report outside its wording. The relevant section of it reads as follows:

CAWCS [the Centre for Advanced Welsh and Celtic Studies] said many still believed the Celts spread from Iron Age central Europe bringing Celtic speech with them, so earlier eras further west were non-Celtic by definition.

But a previous AHRC-funded project at CAWCS showed there was more than one Celtic language in pre-Roman Iberia—present day Spain and Portugal—but it remained an enigma as to how and when the Celts arrived there.

Prof John T Koch at CAWCS, who is the project leader, said: “Pre-historians and historical linguists have a responsibility to Wales and the other Celtic countries, especially to people who speak and learn Welsh and the other Celtic languages.

“They want to know how, where, and when these languages emerged,

what the experts know and don't know.

“What are the viable models and the evidence favouring them? Otherwise, we are supporting cultural heritage with yesterday's theory.”

The ‘previous AHRC funded project at CAWCS’ mentioned in the BBC report had almost nothing to do with the SW inscriptions, as that grant period pre-dated my work on them (see further below). Therefore, the ‘more than one Celtic language in present-day Spain in Portugal’ identified in that earlier research were reflected primarily in place- and group names north and west of Celtiberia in the eastern Meseta. The premise of the current project does not assume that the matrix language of the SW inscriptions has been proven to be Celtic, but rather, as follows (from the documents sent in applying to the AHRC):

The proposal involves only the widely recognized Celtic names of the corpus, and these recur in later sources. The motivation for the project would therefore remain in force even noting this subset as controversial (or if it were left out altogether). It is Hispano-Celtic in general, not just Tartessian in particular, that demands the re-assessment—as impossible to square with Celtic expansion from central Europe in Hallstatt times.

§55. In reaching the judgement that I had ‘wanted Tartessian to be Celtic’ (see §24 above) the reviews ignore the history of my work on the SW corpus as summarized in *Tartessian 2* (pp. 2–6), which will not be repeated here. The reviews also disregard my earlier publications, which had consistently envisioned the emergence of Proto-Celtic in central Europe. None of these publications looked for, or thought it likely that there would be found, evidence for a Celtic language in the extreme south-west of Europe as early as the Early Iron Age.

My primary research activities during the ten years immediately before I turned to the SW corpus were as principal investigator for a project on ‘Culture and Celtic speech’ (2000–2004), also funded by the AHRC, and as general editor of the five-volume *Celtic Culture: A Historical Encyclopedia* (2006). The intellectual background motivating both of these projects was the then keen dispute over the link between the Celtic languages, culture, and cultural identity. In other words, they were motivated—to a significant degree at least—by the so-called ‘Celtosceptic’ controversy (on which see Sims-Williams 1998; James 2000; Collis 2003; Koch 2003).

Evidence from the Iberian Peninsula was included in these works, but it was not the central focus in either project. One of the main themes was the correlation of Ancient Celtic linguistic evidence with material of ‘Celtic’—i.e. Hallstatt and La Tène—typology. As this correlation breaks down in the Iberian Peninsula, that region remained marginal and somewhat anomalous within the main themes of our work. The ‘Tartessian’ language is mentioned only once in the 1,500,000 words of *Celtic Culture*, in the article on

'Continental Celtic' by Joseph Eska (2006).

In large part, it was the persistent marginality and anomalousness of the Hispano-Celtic evidence as considered within these earlier research projects (which had built on the traditional 'Celtic world' model of the late 20th century) that led to my re-examination of the SW corpus beginning in 2007. To cut a long story short, I had not expected what was found in the earlier AHRC project. If that project had been approached, not as research, but as an exercise in finding material to support a pre-conceived conclusion, it would not have resulted in me ceasing to believe in central Europe as the Celtic homeland. Neither did I expect what I found in the SW corpus. New evidence upset some of my long-held views, but I got over it. This led to a departure from the earlier direction of my research and put several of my earlier publications (including the most recent and substantial ones for which I had led teams of researchers) in immediate need of revision.⁵¹

In light of this background, it should be possible to accept the following words:

No one has taken the possibility of Celtic coming from Hispania to the other Celtic countries seriously since we stopped taking *Lebar Gabála Éirenn* (the 11th-century Irish 'Book of Invasions') seriously, but it is now at least worth pausing to review what it is that we think we know that makes that impossible. (Koch 2013a, 265)

The established, even entrenched, model of Celtic origins in Iron Age west-central Europe has come up short in explanatory power. An alternative possibility is being investigated. As previously, the present research project has not been set up to ward off unforeseen conclusions.

XI. Conclusions

§56. The different theories concerning the classification of the language of the SW inscriptions reflect different expectations. With the approach of De Hoz discussed above (§9), there is an expectation that a corpus in a non-Indo-European language written in *scriptio continua* will contain forms that look Indo-European if one uses arbitrary and incorrect segmentation. This is reasonable and, in principle, undoubtedly true. However, the principle only holds so far as there are no other clues to word division and when there is a relatively large corpus and a much smaller proportion of apparently Indo-

51 A few months went by in 2007 between reaching the conclusion that the language of the SW inscriptions was probably Celtic and abandoning the established idea that Celtic most probably emerged from Proto-Indo-European (or Italo-Celtic) in central Europe. The weight of the evidence led to the conclusion that the zone of the Atlantic Bronze Age more certainly included Celtic languages than did the cultures of the central European Urnfields.

European forms leaving uninterpreted material on both sides.

De Hoz's methodological approach is also reasonable. In this, recurrent syntactic patterns are sought in the sequences of signs without presuppositions about the classification of the language. But most of what we get this way looks Indo-European. Several of the epigraphic texts clearly end with the formula word **naṛk^eenti** or one of its variants. This form, as many have seen, looks like an Indo-European verb with a 3rd person plural primary ending. **naṛk^eenti** and its variants are often preceded, immediately or only with the sequence **b^aa** intervening, by **t^ee-ro-b^aare** and its numerous variants. Like **naṛk^eenti** &c., **t^ee-ro-b^aare** &c. is clearly a syntactic unit, correctly segmented in this way. In the light of **t^eee-b^aarenti** (J.23.1), **t^ee-ro-b^aare** also looks like an Indo-European verb, specifically a compound verb with preverbs **t^ee** and **ro**. Ahead of that sequence **uarb^aan** clearly forms a unit or word, a block of six signs moveable within the syntax. Formally, **uarb^aan** suits an Indo-European accusative singular, thus most naturally to be construed as an accusative governed by the nearer verb. Therefore, the claim that the formula cannot be parsed is hollow. As explained above (§38), the formula is not a tiny proportion of the SW corpus, but 32.2% of it, 49% of the matrix language, if we allow names to be identified at the proportion proposed. In addition to this, there are further numerous clues as to how to segment the texts: the principle of redundancy, recurrent stems, prefixes, and terminations, the last including agreement markers that can be parsed as Indo-European case endings. After recognizing such indications of word divisions, these reveal, time and again, series of signs resembling pre-Roman names and name elements from the Indo-European zone of the Peninsula and/or with comparanda from the wider Celtic world. In other words, we are very far in the SW corpus from a situation in which a statistically negligible proportion of arbitrarily segmented sequences of signs looks Indo-European, leaving a statistically high proportion of inscrutable material on either side. In the 33% or so that is neither a version of the formula nor thus far identified as names, the same sort of word-formation and syntactic patterns can often be observed.

§57. The theory of Celtic names in a non-Indo-European matrix language (specifically 'Iberoid' in Eska 2013b and 2013c) reflects an expectation that, if the language of the SW corpus were Indo-European, it should be easier to see that and interpret it than it has been. The disparity of expectations is illuminated when a sentence of mine is used to support the two-language theory:

Koch, himself, admits that the language contains '[m]any sequences [that] remain profoundly uncertain, lacking any identifiable stem prefix or termination' [Koch 2011, 87] (cited by Eska 2013c)

Obviously the reviewer does not think that this should be the case if the SW corpus was written in an Indo-European language. My expectations are different. I expect that a written Celtic language that is an extreme chronological and geographic outlier and is written in a difficult writing system that is primarily derived from that of a non-Indo-European language (with *scriptio continua*, not distinguishing voiced from voiceless stops, and the rest) would be every bit as challenging as Tartessian is. I also expect that the names in such a Celtic language will be more easily recognizable as Celtic than the matrix language.

A writing system not derived from the Greek alphabet (or from a descendant of the Greek alphabet) cannot be expected to permit the phonological structure of an Indo-European language to appear Indo-European. To expect this is to forget the lessons of the decipherments of Linear B and the Cypriot syllabary. If one wished to do so, Greek could be written with more phonetic accuracy in SW script than with either of the syllabaries that were used to write it. The so-called 'Iberoid' features of the phonology could be illusory, artefacts of a writing system that was never influenced by a language related to Iberian. If they are not illusory, they still would be expected in a tradition of literacy that first made landfall on Spain's non-Indo-European Mediterranean litoral and/or in the sound system of a Celtic language spoken on both sides of the *brigā/ilti*-line.

The expectation behind the two-language theories is that, if its language was Celtic, the whole corpus should look more like the other Ancient Celtic languages—as attested and/or as reconstructed from Old Irish—than it has done. The reason that I do not share this expectation is not because I think that the work of my fellow linguists on Continental Celtic and historical reconstruction has been wrong (although in many cases, where there is no agreement, somebody must be wrong). Rather, it is because I think that what has been achieved falls far short of recovering the totality of the grammar and lexicon of Proto-Celtic and, even more so, every possible grammar and every possible lexicon of every Celtic language spoken across Iron Age Europe. When and if we are lucky enough to get more written Celtic evidence dating from the Early Iron Age, I expect that we will find known Celtic names and name elements and more occasionally other kinds of words that look like forms we know already from Gaulish or Celtiberian or reconstructions of Old Irish and Brythonic. But we will also find fossils from Proto-Indo-European that survived in no other attested Celtic language and dead-end innovations that occurred in no other attested Celtic language. Some of the latter are likely to be the results of local conditions of bilingualism. The influence of a foreign language and of foreign ideas about language will be especially potent where bilingualism brings literacy and comes with other major cultural and social changes. In other words, because Proto-Celtic was not a written language spoken by literate societies, wherever we find written Celtic there will have been innovations from Proto-Celtic.

§58. Thus far, the theory of Celtic names in a non-Indo-European matrix language has been seriously unbalanced. The supposition of a second language has been a negative reflection of objections raised to the one-language (Indo-European/Celtic) theory. So, to a large extent, it has been something conjured from nothing: ‘we can’t read it’ or ‘we object to how you read it’, therefore there is a non-Indo-European matrix language.

The two-language theory struggles at the basic level of locating the supposed non-Indo-European matrix language within the corpus. Some of the strongly Indo-European-looking features, such as **naṛk^eenti**, are in the formula, and **eert^aaune** is not a name. It may seem unfair to ask for a more specific descriptive case to be built for the non-Indo-European matrix language. How could such a task be achieved if that language was not related closely or—truly an isolate—not related at all to Iberian, Basque, Semitic, Berber, Etruscan, or any other known language? De Hoz’s approach could be extended. The corpus is now large enough that one could seek a grammar and lexicon from patterns of signs and recurrent sequences, with no hypothesis of any particular language in mind. One could even intentionally exclude patterns that look Indo-European. I don’t expect that would lead anywhere. But don’t take my word for it.

The concluding paragraph of Eska’s first review begins:

The overall impression with which this monograph has left me is that K[och] decided what the desirable result would be prior to conducting a rigorous linguistic analysis. And for that reason, I think, it is bound to be regarded in the future as a Paradebeispiel for how not to establish the genetic affiliation of a language. (2013a, 65)

I do not know what direction future discoveries and scholarship will take. In the long run, the voices now being heard will fall silent and the publications now discussed be forgotten. But the evidence will still be there. It is not hard to see that the SW corpus contains Celtic names. It is not hard to see that the matrix language contains forms that look like Indo-European verbs and preverbs **t^e**, **ro**, and **ar**. Combined, these categories make up more than half the corpus and are consistent with a particular classification. Working independently, many researchers have noticed that Ἀργανθωνιος looks Celtic, and so on. Therefore, the theory that the language of the SW inscriptions is Celtic will probably have a future whatever turn the current debate takes. Given what capable and knowledgeable philologists have achieved so far with the non-Indo-European and two-language theories, building a satisfactory explanation from either of these might be impossible.⁵²

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