

A New Model of Indo-European Subgrouping and Dispersal

For Prof. Murray Emeneau on his 95th birthday, 28 February 1999

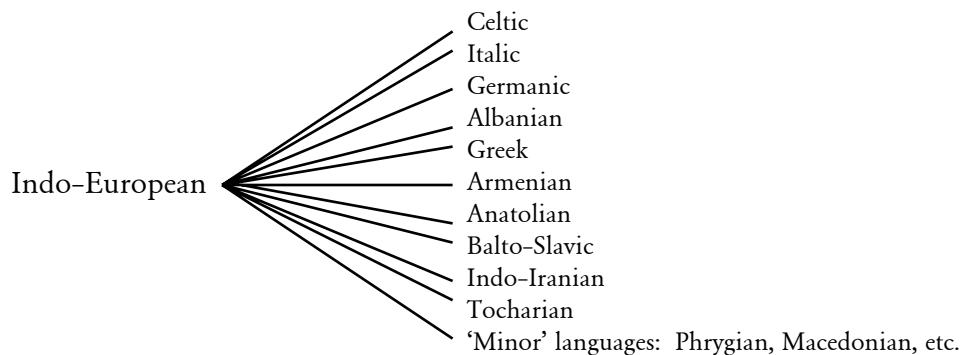
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1. Introduction

In this century two great discoveries have shaken our view of the Indo-European family tree and protolanguage. The first was the discovery of Hittite, which in turn revealed the existence of an Anatolian branch of Indo-European; the second was the discovery in Central Asia of languages belonging to the previously unknown Tocharian branch of the family. Yet as important as these are, they are not the only twentieth century archaeological finds with Indo-European ramifications. In this paper I will explore the implications of a less dramatic set of discoveries for Indo-European subgrouping.

I begin with a question posed in recent work by Johanna Nichols. Like many profound questions, this one is both shockingly obvious and disturbingly obscure: Why does Indo-European have so many branches? Ten are fully documented, and the count rises if you add the so-called ‘minor’ languages – Phrygian, Macedonian, Thracian, Venetic, and others known only through inscriptional remains. This is shown graphically in (1):

1



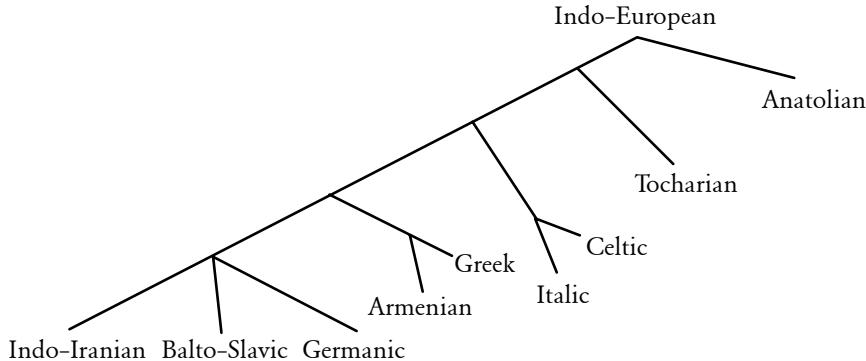
Typical subgrouping situations are of two distinct types. One is the family tree with binary or ternary branching. This corresponds to situations where a speech community is separated for some reason, such as population movement into or out of the area it occupies, and the newly separated communities evolve in relative linguistic isolation.

A second type of subgrouping situation is the dialect continuum, in which local dialects share features with their neighbors but are hard to arrange into a Stammabaum: any original subgrouping is obscured by the areal spread of innovations from dialect to dialect. The social reality behind a dialect continuum is presumably that speakers of adjacent dialects remain in contact and influence

each other linguistically. This local mutual influence eventually has the effect of erasing earlier patterns.

With ten or more branches, the Indo-European family tree is of neither type, yet it is hard to see what single event would split one speech community into ten. To be sure, the Indo-European tree can be turned into one with binary or ternary branching, and a recent proposal along these lines by Ringe et al. (1998) is adapted in (2)¹:

2



Yet attempts to construct a highly articulated Indo-European family tree beg one crucial question: Why are well-established subgroups like Greek and Indo-Iranian defined by many distinctive innovations, while higher-order groups are defined by only a few? Even if we can add structure to the Indo-European tree, the subgroups in (1) are still in some sense the ‘real’ ones, and the intermediate nodes in (2) are nameless precisely because we do not need to refer to them. This requires an explanation.

According to Nichols, the explanation has to do with the dispersal of Indo-European. She argues that ‘[m]ultiple branching at or near the root of a [family] tree points to abrupt dispersal of the protolanguage in a large spread’ (Nichols 1997b: 371). A ‘spread zone’ is defined as ‘an area of low [linguistic] density where a single language ... occupies a large range, and where diversity ... is reduced by language shift and language spreading. A conspicuous spread zone is the grasslands of central Eurasia, in which ... four different spreads have carried different language families across the entire steppe and desert as well as into central Europe and Anatolia’ (Nichols 1997b: 369). In reverse chronological order, these four spreading families are Mongolian, Turkic, Iranian, and Indo-European.

Nichols’ dynamic approach to linguistic geography is original and creative, and I have no quarrel with the general model she proposes, nor with the specific claim that central Eurasia has represented a linguistic spread zone in several cases. For the Indo-European case, this does conflict with the standard archaeological view of the so-called ‘homeland’ and the dispersal of the family. The standard view is that the language corresponding to Proto-Indo-European was spoken somewhere in the Pontic-Caspian steppe – in other words, just north of the Black Sea and Caspian Sea.² What Nichols calls the ‘locus’ of Proto-Indo-European was, on the standard

view, not far from the area where Proto-Indo-Iranian later emerged.

For Nichols it is important that Indo-European ‘has the greatest number of primary branches of any known genetic grouping of comparable age’, since this is ‘the hallmark of a language family that enters a spread zone as an undifferentiated single language and diversifies while spreading’ (Nichols 1997a: 138). What I will suggest here is that discoveries of the second half of this century call into question precisely this aspect of Indo-European subgrouping.

2. Greek

The most famous postwar discovery in Indo-European is the decipherment of Linear B and the subsequent identification of the Mycenaean dialect of Greek, attested between roughly 1400 and 1200 BCE, half a millennium earlier than any other Greek dialect.³ The facts of Mycenaean confuse the reconstruction of Proto-Greek. On the one hand, because Mycenaean Greek shows innovations that are found only in some Greek dialects, it cannot be viewed as Proto-Greek; it is just an early dialect. On the other hand, many innovations are found in every Greek dialect *except* Mycenaean. These used to be viewed as Proto-Greek changes, but because they are absent from Mycenaean they must now be seen as dialectal changes that have spread through all the post-Mycenaean dialects. As Morpurgo Davies (1988: 76) writes, ‘some of the features shared by all dialects are due to post-Mycenaean and a fortiori to post-Common Greek innovations which occurred independently in the various dialects’.

Some of these post-Mycenaean but pan-Greek innovations are sound changes: for instance, the change of labiovelar stops to labials, the loss of intervocalic *h* and *y*, and the loss of postconsonantal *y* with the development of distinctive consonant palatalization. In morphology, I will mention two changes Morpurgo Davies cites. The first involves noun inflection. All post-Mycenaean Greek dialects have a system of five cases in which the Indo-European locative, ablative, and instrumental cases have merged with the dative. Yet Mycenaean retains at least seven of the eight Indo-European nominal cases; the relevant syncretisms postdate Mycenaean.⁴

A second example involves nominal derivation. Proto-Indo-European had many nouns with stems ending in **n* and a few with stems ending in **m*. In forms with suffixes these stem-final nasals surfaced word-medially, and otherwise they were word-final. Greek word-final nasals merged as *n*, with the result that original **m*-stems showed *m* word medially but *n* word finally. Original **n*-stems, of course, showed no such alternation. For the **m*-stems, the alternation was then eliminated by generalizing the *n* from word-final to word-medial position. As an analogical response to alternations created by sound change, all *m*-stems became *n*-stems in Greek.⁵ This is shown in (3). What is interesting here is that this distinctive morphological change appears in all post-Mycenaean dialects but not Mycenaean itself, as shown by the dative *emei* ‘one’ (vs. e.g. later *en-i*).

3	<i>N</i> -STEMS	<i>M</i> -STEMS
	téko: n	*k ^h th ^ó : m > k ^h th ^ó : n
	tékt on -os ‘carpenter’	*k ^h th om -ós > *k ^h th om -ós → *k ^h th on -ós ‘earth’

The problem is not simply that our list of uniquely Proto-Greek innovations is smaller than it used to be; the list is in fact dangerously small. The facts I have just rehearsed complement an issue discussed long before the discovery of Mycenaean, in Meillet's 1913 history of the Greek language. In a number of areas of grammar, the Greek dialects agree in a structural innovation vis-à-vis Indo-European, but they disagree in its formal manifestation in such a way that the innovation cannot be reconstructed for the Greek protolanguage. The infinitive is one example: all Greek dialects have a present, an aorist, and a perfect infinitive; but from dialect to dialect these are formed so differently that the only reconstructible ancestor system is one like Vedic Sanskrit, in which various types of deverbal nouns could be used as ‘infinitives’. If there was any Proto-Greek system, it was not the kind of system found in the documented Greek dialects.

The first person plural verbal ending poses a similar problem. Greek dialects attest two variants of this suffix: *-mes* in West Greek dialects, and *-men* elsewhere. A standard view (e.g. Rix 1992: 251–52) is that *-mes* represents the original present (or ‘primary’) ending and *-men* contains the original past (‘secondary’) ending **-me*. As schematized in (4), *-men* is thus hypercharacterized, consisting of first-person plural **-me* plus a redundant marker *-n* taken over from the first-person singular:

4 CREATION OF 1 PL. *-MEN*

- a secondary 1 pl. **-me* + 1 sg. *-n* → hypercharacterized 1 pl. *-men*
- b secondary 1 pl. *-men* also generalized to primary function
- c West Greek: primary 1 pl. *-mes* generalized to secondary function

This analysis has two obvious problems if the Greek dialects are assumed to reflect a reconstructible ‘Proto-Greek’. First, it presupposes that the ending **-me* survived long enough to be the basis for the creation of *-men* in some dialects, but there is no other Greek evidence for this ending. Second, it fails to account for the Indo-European dialectology of first-person plural endings in **-s* and **-n*. A generalized present and past **-s* ending is also found in Italic (e.g. Latin *-mus* < **-mos*), across the Adriatic Sea from West Greek, while an **-n* ending is also found in Anatolian (e.g. Hittite *-wen*), across the Aegean Sea from the non-West-Greek dialects. It would be attractive to analyze the relevant events in context as innovations spreading between adjacent dialects in a continuum.

3. Celtic and Italic

At the western end of the Indo-European world we have no precise counterpart to Mycenaean Greek, but our knowledge of several peripheral and archaic Celtic and Italic languages has steadily improved as a result of archaeological work in Spain,

France, and Italy. For Celtic, three archaic languages are now documented outside Britain and Ireland: Gaulish in France and elsewhere, Celtiberian in Spain, and Lepontic in Italy. Analysis of the Continental Celtic languages, especially Lepontic and Celtiberian, has cast doubt on a number of putative Celtic innovations. Thus Eska (1998) argues that the diagnostic Celtic loss of Indo-European **p* may have been ‘in progress’ in the earliest documented Lepontic. This would mean that the change was not a ‘Proto-Celtic’ innovation after all, despite eventually affecting all Celtic languages. Current work on Celtiberian suggests a comparable view of some other distinctive innovations of the Celtic branch of Indo-European.⁶

For Italic, the question of linguistic unity is an old one. Certainly the standard view is that Latin on the one hand, with its close relative Faliscan, and on the other hand Oscan, Umbrian, and the other Sabellic languages are members of an Italic branch of Indo-European, and that this has a reconstructible protolanguage with a set of unique innovations vis-à-vis other branches. In this case there is also a respectable minority position, defended locally by Beeler, to the effect that Latin and the Sabellic languages ‘owe their similarities to relatively late linguistic convergence’ and ‘never constituted a subgroup intermediate between Proto-Indo-European and the attested languages’ (Beeler 1966: 51).

This is not the place, and I am not the person, to resolve the Italic controversy, but its contours can be described. Defenders of Italic unity are impressed by the overall structural similarity of the Latin and the Oscan and Umbrian verb systems. For example, Watkins (1966: 43) writes that Latin shows ‘a fundamental opposition of *infectum* and *perfectum*, a present, past, and future of each in the indicative, and a present-future and past in the subjunctive – ten functional structure points in all’, adding that ‘[t]his organization of the verbal system is Common Italic’.⁷ These ten categories are shown with plus (+) marks in (5).

5 ITALIC VERB STRUCTURE (Watkins 1966: 43)

	— Indicative —		— Subjunctive —	
	<i>infectum</i>	<i>perfectum</i>	<i>infectum</i>	<i>perfectum</i>
Future	+	+	—	—
Present	+	+	+	+
Past	+ (*- <i>b̥ā-</i>)	+	+ (*- <i>sē-</i>)	+

As evidence for Proto-Italic, Meiser (1986: 15 n. 1) likewise cites ‘the structural and material similarities’ of the verb systems of the Italic languages.

Opponents of Italic unity are struck by a lack of concord in the specific formal implementation of functionally equivalent categories like those in (5). A single Proto-Italic future formation cannot be reconstructed, and the Italic languages also disagree in the formation of the present subjunctive. Morphological disagreements in the *perfectum* are interpreted by several authorities, including recently Meiser himself, as evidence that the verb system ancestral to the attested Italic systems was unlike the one in (5), and was ‘similar to that of ... Greek’ (Meiser 1993: 171). Boldface plus marks in (5) show the only two categories for which it is clear that a

single innovation underlies the forms of both branches of Italic. In short, the question of Italic unity seems largely to have depended on the weight attached to formal correspondences, as opposed to functional or structural ones, in assessing the likelihood of historical identity as opposed to areal convergence.

4. Outliers

I have reviewed some aspects of Celtic, Italic, and Greek dialectology. For each language family there is evidence that its diagnostic innovations are not in fact shared by all its members. Thus, in the clearest case, the discovery of Mycenaean Greek has cast doubt on some important previously accepted Proto-Greek innovations. Similarly, reconstruction from the slowly expanding Italic dialect corpus suggests that the distinctive profile of the Italic verb system may reflect areal innovation, and new Continental Celtic discoveries are casting doubt on the Proto-Celtic status of previously accepted common features of the family.

Another similarity is that in each of these three cases there is at least one poorly documented outlier language whose generally accepted classification seems to depend on trivial diagnostics. In the Celtic case this outlier is Lusitanian, and a test of Celticity has been the loss of Proto-Indo-European **p*. Calling this change ‘circumstantial and systematically unmotivated’, McCone (1996: 8) concludes that it ‘is unlikely to have happened independently at two or more sub-Celtic nodes ... From this it follows that ... Lusitanian, which ... preserves PIE *p* unchanged in PORCOM “pig” ... cannot properly be considered a Celtic language.’ Here the assumption is that sound changes have systematic motivations, and that a change with no such cause is unmotivated; but this view of sound change is out of date.⁸ An alternative view may be better supported: sound change is caused by perceptual ambiguities, and [p] is less salient acoustically than its voiced counterpart [b] and than its voiceless counterparts at other places of articulation.

In the Italic case the outlier is Venetic, a somewhat better documented language, but one that has given rise to a similar debate. A recent comprehensive study notes that ‘[t]he classification of Venetic as “Italic” ... rests to a large extent on the treatment of the IE voiced aspirates’ (Stuart-Smith 1995: 313). Exactly the same issue looms large in the final case of Greek, where the outlier is Macedonian. In trying to resolve the exact status of this problematic ‘minor’ ancient language, the most recent analysis concludes that ‘some linguistic stratum ... had words inherited from PIE with deaspiration of the voiced aspirates’ (Weiss 1998). The common element in both cases is that the sound changes affecting the Indo-European murmured stops are viewed as crucial in deciding the subgrouping of a poorly documented outlier. Yet Armenian, the sole member of one branch of Indo-European, nonetheless shows among its diverse dialects a range of treatments of the Indo-European murmured stops. This is clearly shown in (6): Armenian dialects that retain murmur coexist with those where the murmured stops are devoiced or have undergone other changes.

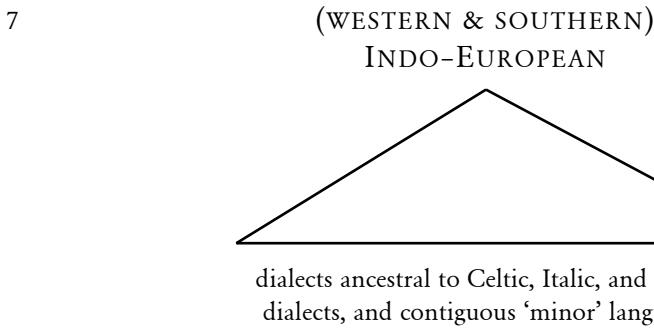
6 SEVEN ARMENIAN DIALECT TYPES (Garrett 1998)

PIE	1	2	3	4	5	6	7
*t	th	th	th	th	th	th	th
*d	d	t	d	d	d	t	t
*d ^h	d ^h	d ^h	d	t	t ^h	d	t

The point is that changes in this one area are very weak evidence indeed for deciding whether to call a language ‘Italic’. The difficulty disappears — the question becomes uninteresting — if we adopt a model that does not require us to impose a historical classification in which every language in the family either does or does not originally belong to a single ‘Italic’ daughter of Indo-European.

5. Conclusion

The picture that emerges differs from the usual model, according to which Celtic, Italic, and Greek correspond to three neighboring daughter nodes on the Indo-European family tree. For western and southern Indo-European, an alternative model is sketched in (7):



What is crucial in this model is that at some early date — say, at the beginning of the second millennium BCE — the dialects that were to become Celtic, or Italic, or Greek, shared no properties that distinguished them uniquely from the other dialects. The point is not simply that innovations could spread from one Indo-European branch to another: this is well known. The point is that while there was linguistic differentiation, the differentiation *among* dialects that were to become Celtic, for example, was no more or less than between *any* pair of dialects. At this time, there was no such thing as Celtic or Italic or Greek.

I am not rejecting the Stammbaum model in favor of the wave model, and I am not saying that Proto-Indo-European was a dialect continuum, or that Indo-European is the result of convergence, or anything along those lines. What I am saying is this: there is no clear evidence for a historical — that is to say, in the technical linguistic sense of the term, a genetic — Celtic or Italic or Greek subgroup of Indo-European. These do not correspond to nodes on an Indo-European Stammbaum. On the contrary, sometime in the third or second millennium BCE, the Indo-European dialects of western and southern Europe formed a continuum.

This contained the ancestors of Celtic, Italic, and Greek, as well as Venetic and the other ‘minor’ languages of the area, and no doubt other dialects that are now lost. But there is no reason to assume that the ancestors of the later Celtic or Italic languages, or of the Greek dialects, shared any exclusive set of innovations defining them as distinct subgroups of Indo-European.

In short, according to the view I am advocating, the formation of a Celtic subgroup of Indo-European, the formation of an Italic subgroup, and even the formation of ‘Greek’ itself may have been secondary Sprachbund phenomena: local responses to areal and cultural connections that could very well have arisen in Greece, on the Italian peninsula, and in western and central Europe. These would represent linguistic areas, not merely the final landing sites of three discrete Indo-European subgroups after some millennial peregrination from the steppes. If this view is right, it makes no sense to ask what route the speakers of ‘Proto-Greek’, ‘Proto-Italic’, or ‘Proto-Celtic’ followed from the Indo-European homeland: no such languages existed, and no such populations. It is an accident of history that these three families and apparent branches of Indo-European have arisen (or four, if we restore Albanian to its place among the living). This accident reveals nothing about Indo-European, its speakers, or the dispersal of Indo-European languages and their speakers.

At least two additional questions now arise. First, can the model I advocate be applied elsewhere in Indo-European? I have only discussed the southwest quadrant of the Indo-European area, not the area occupied by Germanic, Baltic, Slavic, Armenian, Indo-Iranian, and Tocharian. In the Indo-Iranian case, for example, it is easy to reconstruct a protolanguage with numerous distinctive innovations, and there is no evidence that these changes spread secondarily through an early dialect continuum. But if it is only Greek, Italic, and Celtic whose dialectology should be reconceived, then Indo-European still has an impressively large number of initial branches.

The southern and western branches I have discussed here differ in an important way, however, from those that remain. The line that separates them is precisely the line demarcating areas with relatively early writing. The inscriptional record alone gives evidence for the kind of secondary areal convergence I have suggested for the histories of Celtic, Italic, and Greek, because the early, peripheral, and nonstandard dialects whose testimony is crucial are precisely not transmitted through a literary tradition: they are dug out of the ground.

Written evidence is relatively late in the Indo-Iranian world, in Balto-Slavic, in Tocharian, etc., and so in principle the crucial kind of evidence is absent. There is no Indo-Iranian counterpart of Mycenaean Greek because we do not have a variety of Indo-Iranian dialects documented, say, in a corpus of Bactrian inscriptions of 2000 BCE. We will probably never have such evidence, but the fact that Indo-Iranian did not leave the same testimony as Greek does not mean that it was not formed by similar processes. The Indo-European branches whose protolanguages

can be least easily undermined are, in short, those where the diffusion and use of writing is such that we necessarily lack the relevant data. If we apply what we learn from cases where there is evidence to those where there is none, it follows that the Indo-European family tree with a dozen independent, highly distinctive branches is nothing more than a historical mirage.

If so, a second question immediately arises: Why does Indo-European give the impression of multiple initial branching? The answer is clear in each case. In the Greek case, for example, the answer is that a set of dialects were spoken in what can now be seen as a Greek linguistic area, where innovations spread from dialect to dialect, and that dialects that did not fully participate are now either lost or poorly known; Macedonian may be an example. But why is this pattern repeated in case after case?

It is a truism that the discovery of Indo-European and the foundation of the academic field of linguistics were substantially fuelled by nationalism. I suggest that the nationalist ideologies lurking behind our field refract the same sociological forces that shaped its object of study. Our conception of Indo-European emerged from the analysis of national literatures and cultural traditions, and the canonical branches of the family emerged through the creation of national identities. On the view of Indo-European dialectology I have sketched, the existence of Greek is a by-product of the formation of what we may call Greek national identity. Through a series of mutual linguistic influences, this facilitated the dialectological coalescence of the set of Indo-European dialects we now call Greek. The ironic point, if I am right, is that the supraregionalizing force underlying the study of Indo-European is at the same time responsible for obscuring the true nature of its family tree.

This approach to Indo-European dialectology should not change the practice of historical reconstruction and analysis, but it does have several virtues apart from any purely evidentiary justification. First, it makes it possible to interpret cases like the Greek first-person plural ending, where western dialects agree with the Indo-European branch to the west and eastern dialects agree with the branch to the east. Such cases can in principle reflect the earlier state of the dialect continuum, in this case before the diffusion of a set of unique innovations throughout the dialects we now call Greek.

Second, classificatory debates about peripheral or ‘minor’ languages become somewhat pointless on this approach. Whether Venetic is ‘Italic’, for instance, is a matter of its development and its historical sociolinguistic connections in Italy and not its essential status. It will be interesting to know how Venetic participated in the Italic linguistic area, and its treatment of the Indo-European murmured stops may cast light on this issue, but we need not expect to find any unitary answer.

Finally, inasmuch as sociolinguistic processes are social and should therefore be encompassed by social theories, it is worth noting that the view I have advocated is congruent with present-day archaeological models, which stress the dynamic aspect of ancient societies. The development of areal cultural homogeneity is sometimes

attributed to ‘peer polity interaction’ (Renfrew & Cherry 1986), or contacts within a group of smaller-scale communities, and these accounts seem to predict precisely the kind of linguistic evolution I have proposed. Here is a point of overlap with the views of Colin Renfrew, who ‘would prefer to see the development of the Celtic languages, in the sense that they are Celtic as distinct from generalized Indo-European, as taking place essentially in those areas where their speech is later attested’ (1987: 245). Renfrew’s view of Indo-European dispersal is rightly rejected by specialists as linguistically unsound, but his influential perspective on ancient social interactions is entirely at home here.

Let me add a historiographic coda. Nichols’ integration of historical and areal linguistics is brilliantly original, as I have said, but like everything it has a context. In this case it is reasonable to think of a Berkeley school of language change whose most striking feature is its emphasis on the dynamic interaction of languages and speakers in contact. The founding document of this school is Emeneau’s paper on ‘India as a linguistic area’ (1956), though it is also a school arising from the specific linguistic profile of California; and it is relevant that Emeneau’s paper appeared in the issue of *Language* honoring Alfred Kroeber on his 80th birthday. I agree with Nichols that a dynamic view of language change enriches historical study, but for Indo-European I prefer to situate the dynamic at the end of linguistic dispersal, in the historical formation of the branches that now comprise the family as a whole.

Notes

¹ I do not mean to imply (nor do Ringe and his colleagues) that the tree in (2) is original in every respect; it echoes subgroupings proposed by a number of scholars (e.g. the late Jochem Schindler) in recent years.

² For illustrations of this view see e.g. Mallory (1989, 1997).

³ Strictly speaking this is not true, but the corpus of Greek dialect inscriptions earlier than 800 BCE is insignificant for present purposes.

⁴ For the data see e.g. Meier-Brügger (1992, vol. 2, pp. 63–66) and Hajnal (1995: 16–32).

⁵ This change is discussed from a typological point of view by Schindler (1974).

⁶ For instance, the Celtic raising of long mid vowels (*ō > ū in final syllables, *ē > ī) may not be regular in Celtiberian; cf. the discussion of McCone (1996: 15–17) with references.

⁷ ‘Infectum’ and ‘perfectum’ are technical terms for the ‘present’ and ‘perfect’ verb systems.

⁸ McCone’s argument also ignores the possibility of areal diffusion: an innovation that originates in one dialect and spreads to others cannot be said to ‘happen independently at two or more nodes’. In fairness it should be noted that McCone also cites a sporadic metathesis (attested in Irish, British Celtic, and Gaulish, but not in Lusitanian) as a Celtic diagnostic.

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