

# *LINGUISTICS*

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**MARCANTONIO, Angela**
**The Indo-European Language Family: Questions About Its Status**
**— Introduction —**

This paper is the introduction to the volume *The Indo-European Language Family: Questions About Its Status*, edited by Angela Marcantonio and published by the Institute for the Study of Man in 2009, Washington D.C., as the Journal for Indo-European Studies Monograph Series, Monograph 55. ([www.jies.org](http://www.jies.org)). Republished with permission. – Ed. Journal of Eurasian Studies

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**1. 1.** The purpose of the present volume is to survey the current state of the Indo-European ('IE') theory in the light of modern linguistic knowledge. Included in the survey is also extra-linguistic evidence, such as recent archaeological, genetic and palaeo-anthropological findings. Its ultimate purpose is to revisit the validity of the various tenets of the theory. In fact, when scholars refer to the "IE theory", they may be referring to one of a number of competing, and sometimes contradictory, models. For example, some regard IE as purely a linguistic classification, whilst others regard it as an attempt to reconstruct a 'real' pre-historical language. To take another example, some scholars hold that the original IE protolanguage was a morphologically complex language, similar to Sanskrit, whilst others argue it was morphologically simple.

The volume intends to achieve its purpose by approaching the IE theory from various perspectives and areas of expertise, thanks to a collection of articles by scholars specialized in IE and historical linguistics, and also by Sanskritists, Dravidianists and archaeologists, whose field of study have been heavily affected by the IE linguistic classification. It is only by bringing together, in a single volume, these various approaches and views about IE that it clearly and explicitly emerges how surprisingly different and, often, even deeply contradictory, these views and approaches may be. Thus, the IE theory is widely accepted despite the fact that opinions differ enormously on what the theory actually comprises. Opinions may clash even as to the very nature and validity of many of the underlying tenets of the theory – whether explicitly stated or quietly assumed. For example, as mentioned, some scholars regard the subject a 'pure theory', which helps to describe correlations between languages, whilst others regard it as a valid means to reconstruct pre-historical facts. Although there seems to be a widely shared assumption that at least a 'hard core' of the correlations among the IE languages are 'compelling' – that is, too striking to be the result of chance – there may be deep divergences on how to interpret these correlations, as well as on other, less central but equally important aspects of the theory. In other words, contrary to what one would expect, the wide acceptance of the IE linguistic classification does not appear to be accompanied by a parallel

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acceptance of a coherent and equally agreed set of tenets, a coherent common denominator, consisting of what one could call the 'hard linguistic evidence' and the 'fundamental principles' upon which the theory, supposedly, is based. This being the case, it is appropriate to dedicate a volume to the precise task of addressing the fundamental question of why this should be the case. This, ultimately, amounts to the task of re-assessing the founding principles of the theory. In particular, one might reasonably ask the following questions:

1. How is it possible that such widely differing and often contradictory views are still unresolved today? Is this a consequence of the fact that the adopted methods of analysis are not rigorous enough, or alternatively is it the linguistic and extra-linguistic evidence that is problematic – for example, is it ambiguous, insufficient, or simply highly 'malleable'?

2. Is the IE theory a 'scientific' theory, that is, has it been established through "methods of analysis which produce results that are subject to invalidation"<sup>1</sup>? If so, what precisely is the evidence counter to the model? This evidence does exist, although it is usually minimized, in various ways, but: is it really so insignificant?

3. As an alternative, some scholars believe that the theory cannot be invalidated and therefore is not a 'scientific' theory; nevertheless its validity is simply 'compelling'. What is the basis for this claim?

The points of views and perspectives presented in this volume, although might have surfaced in professional publications before, have hardly been dealt with and confronted with one another within a single volume, despite being inextricably interdependent. As a consequence, the questions raised in the points (1)-(3) above have hardly been addressed in a targeted and systematic way. For example, there are plenty of publications dealing with the issue of the strengths and weaknesses of the methods of historical linguistics (see Birnbaum (1977); Jones (1993); Fox (1995); Aikhenvald & Dixon (eds, 2001); Nichols (1992); Lass (1997), etc., just to pick up some names at random). However, these publications hardly ever ponder on whether the acknowledged weaknesses may have a negative impact on the IE theory and, if yes, to what extent. Similarly, there are many publications revolving around "how real(ist) are reconstructions" (Lass 1993), but their scope hardly ever extends to encompass the consequent issue of how to best interpret the IE reconstructed, comparative corpus. On the other hand, there are plenty of publications which (ignoring the debates mentioned above), revolve only around the question of the whereabouts of the (assumed) IE proto-community. Finally, textbooks of IE linguistics (and, often, specialist publications too), hardly ever mention any of these ongoing debates. It is true to say that the purpose and scope of a textbook is not that of reporting controversies, but just that of presenting and illustrating the received wisdom. Nevertheless, the fact remains that textbooks typically present a highly idealized, monolithic picture of IE: a paradigmatic, problem-free language family, where everything works (especially sound laws, lexical and morphological correspondences), where there are hardly any contradictions or ambiguities in the linguistic or extra-linguistic evidence, or even any significant divergence of views among scholars. This is even more surprising since this idealized picture is in stark contrast with the messy reality – in terms of

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<sup>1</sup> I have used here the words by H. Andersen, personal communication; 2006.

variation, high level of exception, contradictory evidence, etc. – found in practically all the other language families of the world, as well, admittedly, within branches of IE (such as the Romance and Germanic languages, or the Balto-Slavic continuum, etc.). This in turn has led several scholars (such as Grace 1990 & 1996) to come up with the rather ‘aberrant’ idea that there must exist in the world two basic types of language families: the ‘the exemplary ones’ (IE and just few others) and the ‘aberrant’ ones (all the rest).

There are, of course, textbooks that present a more realistic picture of the state of the IE family, by pointing out the many exceptions to the stated laws, the difficulties encountered in the process of phonological or morphological reconstruction, or the poor quality and /or quantity of the evidence in support of otherwise widely accepted theories. One may compare, for example, Szemerényi’s (1973 & 1996:122 ff.) and Gusmani’s (1979) account of the slippery evidence on which the laryngeal theory is typically based on the Hittite side, or Sihler’s (1995:144 ff.) account of the factual and methodological obscurities found in Verner’s Law, one of the most revered IE sound laws. However, in most cases, the dissident opinions or the problematic data and analyses – if mentioned at all – are typically minimized through various means (for example, by being reported in footnotes and / or by the absence of the due references), so that it is difficult to evaluate their potential impact on the validity of the theory, or simply just to acknowledge them. It could be objected that no theory can be or ever has been invalidated by the discovery of one or more pieces of evidence counter to the model. This is true; however, if, at some stage in the history of a theory the amount of evidence counter to the model reaches what is usually called a ‘point of critical mass’, then a revision of the theory might be in order, whether to modify or update its tenets, or, if necessary, to reject it altogether. In other words, minimizing, re-interpreting or adjusting any evidence found to be inconsistent with the model might be misleading, and therefore not desirable. In fact, scholars identifying problems in their area of research may wrongly assume that the matter has been settled beyond doubt in other areas of study, and may therefore in turn decide (understandably) to minimize or even ignore his / her own item(s) of counter-evidence, in this way, unwillingly, and maybe wrongly, contributing to reinforce the validity of the theory in question.

1. 2. The scholars contributing to this volume will argue in favor or against some of the major tenets embedded in the variegated IE theory on the basis of both specific corpora of data and / or methodological considerations. We believe that a fair and healthy debate may be of help in attempting to sort out, if necessary, what appear to be proper ‘facts’ and valid analyses from what appear to be instead (questionable) interpretations, (unfounded) speculations, or even sheer myths. If it turns out from this debate that the different and at times contradictory views about IE can, after all, be reconciled, then the IE theory will gain in rigor and therefore in credibility. If, on the contrary, it turns out that these views cannot be reconciled, then, maybe, some or all of the tenets and assumptions that lie at the foundation of the theory will have to be revisited, or called into question.

This volume sets out to bring together points of view and analyses that deal with issues within the following, main pillars of IE studies: a) the sound laws and the reconstructed phonology and lexicon; b) the reconstructed grammar; c) the traditional family tree model and related concept of ‘proto-language’ *vs* other models of language formation and diffusion; c) the thesis that the Sanskrit language /culture did not originate in India, but represents an intrusion from the west; d) the thesis that Sanskrit is (arguably) the oldest language within IE, and therefore one of the most, if not the most important one for the purpose of

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reconstruction. In addition, some attention will also be dedicated to the state of the archaeological, palaeo-anthropological and (to a lesser extent) genetic research, to verify whether or not the 'extra-linguistic evidence' lends support to the traditional model (although this is a linguistic volume and its focus remains the linguistic analysis). As to the specific areas of debate, we will concentrate on the following: 1) the strengths and weaknesses of the comparative method, and, therefore, the reliability of the conventionally established sound laws and reconstructions; 2) the role of morphology in assessing and reconstructing language families in general and the IE family, with particular attention being paid to the IE verbal system; 3) the debate of whether it is possible to tell apart genetic from areal correlations; 4A) the 'conventionalist' vs 'realist' approach to reconstruction; 4B) the issues of the IE homeland and related migrations of the (assumed) IE proto-community.

## 2. The comparative method and the sound laws

### 2.1. The strengths and weaknesses of the comparative method

The debate revolving around the issue of the strengths and weaknesses of the comparative method is an intense, long standing debate. In fact, establishing regular sound correspondences is considered by several (many / most?) scholars to be a crucial part of the process of establishing language families (see for example Campbell (1998:315)). However, as mentioned, this debate has hardly ever been associated with a targeted, extensive investigation of the possible impact the weaknesses of the comparative method may have on the validity of IE as a linguistic classification. In particular, the long standing "*Lautgesetz* controversy" (for which see Wilbur 1977) subsided without resolution, and despite its recent resurfacing in publications dealing with several linguistic areas / families in the world (see for example Ross & Durie (eds, 1996); Blust (1996)<sup>2</sup>, Aikhenvald & Dixon (eds, 2001), etc.), it is rarely referred to in textbooks of IE. In fact, these usually assume, whether explicitly or implicitly, that the 'regularity principle' and the related family tree model have been established in IE beyond doubt, through the support of an extensive amount of data derived from the various IE languages. However, this is not necessarily the case (as illustrated in this volume in the chapter by Andersen and Marcantonio). As a consequence, the reader, including general linguists or even experts in historical linguistics who are not acquainted with the details of IE, may be excused if they are confused as to the actual 'quality' and 'quantity' of the phonological / lexical correspondences conventionally established for IE. Indeed, within IE too irregularity and exceptions do occur – it would be odd otherwise. However, the vital questions are:

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<sup>2</sup> See the following quote by Blust (1996:137) with regard to the Austronesian languages of island Southeast Asia: "Pandemic irregularity resist a plausible explanation through borrowing, analogy or other mechanisms of secondary change and hence are particularly difficult to reconcile with the Neo-grammarians position that all apparent diachronic rule violations are due to secondary factors which interfere with the perfect regularity of primary change".

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A) How pervasive is this irregularity?

B) Is it really true that the encountered irregularities can, in most cases, be justified through 'genuine' linguistic processes, that is, without stretching the explanatory system up to the point of dangerous 'circularity', by 'appending' *ad-hoc* justifications?

The reader who would expect an answer to these questions which is coherent, unanimous, and, most importantly, decisive – one way or the other – might be disappointed. To show that this is the case, here is a list of the most common justifications put forward to answer question (A):

1. Only the ordinary nouns in IE, particularly those referring to objects and concepts of everyday life, display a high degree of irregularity (much higher in any case than verbal roots), but this is only because they belong to the lower level of speech, the lower level stratum of the IE population (Meillet 1934: 396 ff.; Benveniste 1935:175 ff.).

2. The irregularities are only, in most cases apparent, in the sense that linguists have not yet found the appropriate explanation to account for them.

3. There are some / many / plenty of irregularities (according to interpretations), but there is nothing to be surprised about. We know that sound changes do not proceed so regularly after all, but this does not have a negative impact on the validity of IE, whose establishment, in fact, has *not* been based (only) on the phonological / lexical correlations, but on the morphological ones. Such a view is embraced, among others, by Harrison (2003:214 ff), Greenberg (1987:18; see also Croft 2005) and, in this volume, by Kazanas. For example, Harrison (2003: 217) states that: "If one can *prove* that even one single cognate pair holds over two languages, one has proven those languages genetically related". The (implicit) claim here appears to be that proper lexical 'correspondences' are generally hard, if not impossible to attain. Therefore, linguists must come to terms with the fact that also within IE (like within other language families or across macro-families) the correlations among the assumed cognates are not, in the main, as 'regular and systematic' as generally claimed.

4. Sound changes do, in the main, proceed regularly, but the encountered irregularities are a natural effect of the great antiquity of the IE family, although its precise degree of antiquity is impossible to assess (see in this regard the chapters by Kazanas, Bryant and Schmitt in this volume).

One may further observe that sound changes (such as phonemic mergers) may not be necessarily that useful for the purpose of identifying innovations, and therefore the internal sub-grouping of families. Most of these sound changes are in fact so natural that they can be easily repeated in different lines of descent<sup>3</sup>. Besides, these natural sound changes may independently occur in totally different language families (see

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<sup>3</sup> According to Ringe et al. (2002), the same would apply to morphology, where, for example, often it is impossible to discover which inflectional markers are ancestral and which may represent innovation.

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for example Ringe et al. (2002:63 ff.); see also Blenvins (2004) for a detailed account of word-wild frequent sound patterns and sound changes<sup>4</sup>).

As to the answer to question (B), we are not aware of any research carried out with the specific purpose of ascertaining whether or not the circularity issue has had a negative impact on the soundness of IE. In the year 1998 Morpurgo Davies (1998: 254) wrote:

A final agreement about the nature and validity of sound-laws was never reached. It was generally accepted (by the neogrammarians and everyone else) that testing any sound law against the data was bound to reveal a number of exceptions; in other words, there could not be an immediate empirical demonstration of the regularity principle [...]. The neogrammarians did of course maintain that all the exceptions could be explained away by re-defining the law, or by identifying a different starting-point, or by recognizing the interference with analogical process, but they were immediately accused of circularity [...]. We can say that the sound laws have no exceptions only because when we find an exception we eliminate it saying that there has been analogical interference. On the other hand, we also say that the only way in which we can prove that a form [...] is analogical is by pointing out that otherwise it would be an exception to the sound laws. There seemed no obvious counter-objection and the problem remained open [...]. Meanwhile, however, the practicing historical or comparative linguist continued to, or began to, operate in terms of sound laws not too remote from those pleaded by the neo-grammarians.

On the issue of the reliability (or otherwise) of sound laws, compare also the following statement made by Clackson (2007: 60-61) with specific reference to the laryngeal theory: "The comparative method does not rely on absolute regularity, and the PIE laryngeals may provide an example of where reconstruction is possible without the assumption of rigid sound-laws". This statement begs the question of why, when and where, and on the basis of which criteria, scholars may – or may not – assume the existence of "rigid sound-laws".

## 2. 2. The circularity issue

If – as it seems – the circularity issue has not been solved (yet?), scholars could attempt to set up some sort of qualitative and / or quantitative constraint to the number of the defining parameters a given law may consist of. In practice, however, as far as we are aware, there has never been any such attempt. On the contrary, in the every-day, painstaking practice of establishing sound laws and correspondences, any mismatch in the evidence (ambiguity or absence of the expected outcome, exceptions, etc.) can always "be explained away" through a range of procedures, a range of 'adjustable parameters', to be added to the original definition of the law. In other words, instead of casting doubts on the validity of an assumed law (and, if necessary, dropping it) when faced with exceptions and difficulties, typically the practitioner tries

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<sup>4</sup> As pointed out by Hock (1986:633) and Belardi (2002:307), the neo-grammarians worked on sound changes only *ex post facto*; therefore they were unable to observe particular changes in progress; see Kazanas in this volume.

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to 'rescue' the stated law, even at a cost of making recourse to a (virtually unlimited) number of (often *ad-hoc*) adjustments, such as:

1. re-defining the law
2. identifying a different starting-point of the law
3. assuming borrowing, from other languages, or 'transitional' dialects, or even from unknown, extinct languages /dialects<sup>5</sup>
4. assuming analogical processes
5. re-arranging the stated sequence of rules in a different order
6. postulating a (/another) laryngeal segment
7. stating that the mismatches in the expected outcomes of the law are not significant for calling into question a theory as well established as IE.

The obvious consequence of this circularity deeply embedded in the comparative method is that the explanatory system runs into the risk of becoming so powerful, so flexible, that it can be stretched to match almost any data, in this way making it impossible to compare the results it yields against the predictions of the model. In other words, although each single 'adjustable parameter' as listed above may in itself reflect a plausible, genuine linguistic process, the overall cumulative effect of many adjustable parameters added to the definition of a given law may endanger the 'cumulative effect', the 'statistical significance' any established 'law', or even 'tendency', should display to deserve these names. This is an issue that has hardly ever been properly and systematically addressed (as far as we know). Although the supposedly rigorous, 'scientific' nature of the comparative method has often been called into question, and more objective quantitative methods of analysis have been at times adopted within historical linguistics, the statistical significance of the IE comparative corpus itself (both the phonological (/lexical) corpus and the morphological one) has never been tested. For example, Collinge (1985) raises the general issue of the lack of 'cumulative effect' in most of the conventionally established 'Laws of Indo-European', but does not provide any detailed, systematic (qualitative and / or quantitative) analysis of the IE comparative corpus on the whole. Ringe (1992, 1995, 1998a, 1999a) and Ringe et al. (2002) apply their statistical analyses basically to the correlations between IE and other, supposedly related language families, in order to prove that macro-families lack statistical significance. Other authors, such as McMahon & McMahon (2003 & 2005), or McMahon (2005), apply quantitative analyses to estimate the 'correct phylogeny' of the IE family, whilst also emphasizing the need of integrating the traditional methods of historical linguistics with quantitative methods (at this regard see the chapter by Drinka, who also discusses the pros and cons of the latest cladistic models). Thus, these Authors have used the IE family, whose validity is taken for granted, basically as a 'control case' for various kinds of statistical tests within historical linguistics, but

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<sup>5</sup> For example, Sihler (1995:157) says: "Much effort has been devoted to trying to discover the conditions under which  $*k^w > f$  might be a regular Germanic development, but without success. The probable explanation is that these forms are dialect borrowings from an otherwise un-attested *P*-dialects of Germanic".

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have not tested the statistical significance of the IE comparative corpus itself. See in this regard Brady & Marcantonio (2003) and Marcantonio (2002/5). See also Marcantonio in this volume, who argues that the great majority of the conventionally stated IE sound laws lack statistical significance and that, therefore, most of the conventionally established correspondences (within the LIV corpus) are simply similarities, most probably in the given sense of ‘chance resemblances’.

### 2. 3. Borrowing *vs* inheritance

In addition to the issues listed above, there is also to consider the possibility that the established ‘cognates’ – be they ‘similarities’ or proper ‘correspondences’ – may be due to the common processes of borrowing, diffusion, convergence, or even chance resemblances. As is known, borrowed words tend to integrate into the sound pattern of the receiving language, as well as undergoing the same (more or less regular) changes that inherited words would undergo. Thus, the identification of borrowed elements on the basis of internal, linguistic clues only might not always be easy. Therefore, sound correspondences, whilst fundamental to most approaches in assessing language families, “can be misused” (to use Campbell’s (1998: 315) words). The difficulties (and related long standing debate) of telling apart genetic *vs* areal correlations are clearly illustrated in this volume in the chapter by Andersen, who closely examines certain pathways of development of the Balto-Slavic accentual system.

It is also worth mentioning in this context the well known fact that several semantic fields within the IE basic lexicon (including the terms designating ‘extended family’ and ‘the family of the woman / wife’), in addition to being mainly irregular, typically lack a wide distribution across the IE area, being often confined to just two or three contiguous languages (as pointed out by Schmidt (1872) and Meillet [1934]). In contrast, the cognate terms for ‘mother’, ‘father’, ‘brother’, ‘daughter’, etc., display a much wider distribution, and a higher degree of regularity. This factor has correctly raised the suspicion that processes of loss (and consequent replacement) of original words, or even processes of chance resemblance, may have been involved in this area. The issue of the wide *vs* restricted distribution and the (degree of) irregularity of many basic cognates within IE is interpreted differently by different scholars (see for example discussion at point (1) in paragraph 2.1. above). On this topic the reader may compare the contrasting views held by Kazanas and Marcantonio in this volume. Kazanas<sup>6</sup> argues that a great deal of fundamental cognate terms have actually been individuated across the IE languages, more than enough to warrant their inherited nature – even if they are not, in the main, regular and systematic correspondences. Marcantonio, on the other hand, argues that there is a great deal of chance resemblances involved. Finally, one could also assess these data and related interpretation in the light of recent research in the field of historical, contact linguistics, for which see Matras (2002) and Matras & Bakker (eds, 2003). The results of this research appear to support the ‘loss’ interpretation. In fact, Matras (2002) investigates two typical mixed languages, Romani and Domari (descendent of Central Indo-Aryan languages) and shows how patterns of original words are typically retained, and are therefore easily identifiable even within a context of extensive borrowing. For example, the system of kinship terms of Romani generally retains original, Indo-Aryan terms that refer to “the first-level kin of the same generation” (such as the

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<sup>6</sup> Kazanas reports a detailed and exhaustive list of widely diffused cognates, which include, for example, the term for ‘blood’, ‘daughter’, ‘arm’, ‘daughter-in-law’, ‘house’, ‘metal’, ‘widow’, etc.

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terms for 'sister' and 'brother'), or the "first-level and lateral kin one generation older" (such as the terms for 'father', 'mother', 'uncle', etc.), or the "first level kin one generation younger" (such as the terms for 'son' and 'daughter'). In contrast, the terms for less close relatives tend to be more easily borrowed (such as the terms designating 'cousins', 'nephews', 'grandchildren', etc.). This is, by and large, the situation encountered within IE. Thus, one could argue that, over time, the original IE vocabulary dwindled, whereby 'more basic kinship terms' – so to speak – have been retained, whilst the 'less basic' ones (and others) have been replaced by borrowed terms (compare discussion below and Matras in this volume for the results of historical, contact linguistics applied to morphology).

#### **2. 4. Is the phonological evidence malleable?**

Judging from the debates outlined above, one could certainly argue that the conventional, phonological / lexical evidence on which the IE theory is based, to a closer scrutiny, appears to be rather 'malleable' – it is certainly not as decisive (one way or the other) as generally claimed. As a matter of fact, it is always possible to find a plausible, although not always testable, justification to any intervening piece of evidence counter to a stated rule or tendency. Similarly, it is also always possible to provide at least two equally plausible, equally well founded explanations for any given linguistic phenomenon. This is also the case within non-controversial areas of IE linguistics, such as the postulation of the so-called 'Indo-Iranian branch / unity'. One can in fact compare the different interpretations given to this unity – (also) on the basis of phonological /lexical evidence – in the relevant literature, for which see Lazzeroni (1998), Sims-Williams (1998) and Schmitt (1987). Compare also Schmitt in this volume, who argues that the data from both Vedic and Sanskrit are not with absolute necessity genuinely antique and that, therefore, Old Indo-Aryan is not as close to PIE as still believed by some scholars (see below).

At this point one could object that all these methodological and factual difficulties do not, after all, matter, even if they *did* impact negatively on the validity of the conventionally established sound laws (and related correspondences and reconstructions). This is because, as mentioned above, the lexicon is often considered to be the level of language less (or not at all?) relevant for the purpose of assessing genetic relationships. This would be sound and acceptable if there were indeed a consensus among Indo-Europeanists as well as comparativists in general on the principle that it is morphology the level of language (mostly?) relevant in this context. But, do we find that consensus? This brings us to the second, specific area of debate dealt with in this volume.

### **3. The role of morphology**

#### **3.1. Is morphology the most reliable indicator of genetic inheritance?**

Since the very times of the establishment of IE the prevailing opinion appears to have been that grammar can offer the most reliable evidence for assessing genetic relatedness. Grammar has typically been considered to be a rather stable level of language and, often, totally resistant to borrowing – in contrast to the volatility of the lexicon. These properties have made morphological correlations quite popular among many historical linguists, even if it has always been (more or less openly) recognized that

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it is not clear what the criteria are, if any, on the basis of which to identify and evaluate morphological similarities. In fact, not even the regularity principle is expected to consistently operate at this level, due to the overwhelming interference of the analogical principle. In addition, there has never been (to our knowledge) any systematic attempt to define a possible measure, a 'unit of similarity' (in form and / or function) – as it were – to be applied in the practice of comparing morphemes. This measure of similarity would work as a common denominator against which to evaluate the at times vaguely similar, at times very similar and at times identical morphemes occurring among (most / some) IE languages. Thus, the problem is that the morphological correlations are typically observed and established by intuitive, visual inspection (often by single scholars), whereby considerable latitude may be allowed when it comes to phonological forms as well as to similarity of functions. For the morphological (and morpho-phonological) correlations to be rigorous, to be proper 'correspondences', one should certainly require regular phonological correspondences between morphs which also indicate similar (but how similar?) meanings and /or functions –condition which is hardly ever met. Indeed, one often reads in the literature that the grammatical correlations within IE are (still nowadays) simply and purely 'obvious' to the 'naked eyes' of the trained philologist (see Nichols 1996a), exactly as they appeared to the first scholars who dealt with them a couple of centuries ago.

### 3. 2. The degree of 'borrowability' of grammar

In recent years a mounting body of evidence has been accumulating according to which not only grammar is found to be 'borrowable', but, given the appropriate historical and social context, it may rate quite high on the scale of borrowability. It could therefore be difficult to determine whether shared grammatical innovations are the result of genetic inheritance or of areal convergence. For example, in connection with some Australian linguistic areas the following question has often been posed: is grammar borrowable to such an extent that historical reconstruction becomes impossible? (See Dench (2001); Austin (1989); Austin (ed. 1990) & Dixon [2002]).

The issue of the (degree of) borrowability of grammar is addressed in this volume by Matras, through his detailed investigation of the grammatical borrowing that has taken place in the 'mixed' languages: Romani and Domari (see also discussion above (par. 2.3.) with regard to their lexical borrowing). In particular, the Author shows that in these languages grammatical borrowing has been extensive, occurring even in the domain of bound morphology. Nevertheless, genetically inherited morphological elements and patterns *can* be identified and kept distinct from the borrowed ones. In other words, even in the context of extreme contact and mixing, borrowing does not appear to be random but "tends to be structured in a hierarchical manner, and so it is at least to some extent predictable". This in turn means that "some components of grammar offer more reliable indicators than others about shared historical-genetic inheritance. For example, Romani shows "unique morphological conservatism in its nominal and verbal inflection", as illustrated by the fact that it preserves to a considerable degree the original endings of the Old /Middle Indo-Aryan present tense conjugation<sup>7</sup>. On the other hand, Matras also presents evidence that nominal case endings and verbal inflections, if borrowed at all, tend to be borrowed "wholesale" (see also

<sup>7</sup> Compare for the singular: OIA *-āmi* > Romani *-av*; OIA *-asi* > Romani *-es*; OIA *-ati* > Romani *-el*. For the plural: 1<sup>st</sup> OIA *-āmas* > possibly Romani *-as*; for the 3<sup>rd</sup> person OIA *-anti* > Romani *-en*, spreading also to the 2<sup>nd</sup> person by analogy (Matras 2002: 43).

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Levin (1995), who presents evidence of this sort in the borrowing of verbal and nominal paradigms from IE into Semitic). These findings can certainly assist linguists in the delicate task of identifying original morphemes / morphological paradigms and reconstructing the morphological structure of proto-languages in general, and of the IE proto-language in particular. As a matter of fact, within IE studies there is still an open question regarding the nature of the original morphological structure of PIE: was PIE rich in morphology (as is the case, mainly, of Greek and Indo-Iranian), which has then been ‘reduced’ or ‘lost’ in the other languages, or was it rather poor in morphology, in which case the complex morphology of Indo-Iranian and Greek is the result of parallel, shared innovations, rather than of genetic inheritance? This issue is dealt with in this volume by Drinka, Kazanas and Di Giovine, who all hold different, at times contradictory views on the topic, as discussed in more details below.

This already intricate debate is further complicated by the connected issue of the ‘Indo-Hittite theory’ as proposed by Sturtevant (1933) – although Sturtevant’s position as such has now been abandoned. In fact, although scholars appear to agree on the fact that the Anatolian languages have a special status among the IE languages, they disagree on how to interpret and justify this special status, that is, the numerous acknowledged differences existing between the Anatolian languages and all the rest of the IE languages, (particularly) at the morphological level<sup>8</sup>. Regarding this issue, see Carruba (1997 & 2001a<sup>9</sup>), Luraghi (1998); Zeilfelder (1994/2001); Marazzi et al. (1990), Drews (ed. 2001)<sup>10</sup>, Clackson (2007: 129 ff.), etc.). For a possible interpretation of the status of Hittite within the family compare the chapter by Di Giovine, who addresses the issue of the verbal morphology, and Carruba, who addresses the peculiarities of the Hittite *Ablaut*.

Still on the issue of the ‘borrowability’ of grammar, it has been claimed at times that the IE morphological correlations are, on the whole, similar enough to be considered valid correlations but different enough so as not to raise the suspicion that borrowing might have been involved. However, certain IE grammatical forms typically reported in textbooks as ‘obvious’ examples of genetic inheritance – such as the paradigm of the verb ‘to be’, or ‘to bear, carry’ – are so similar, if not in some forms identical across the area, that the suspicion of borrowing may indeed arise. In fact, one would normally expect much more divergence from a long process of inheritance and development. These observations have been made, for example, by Croft (2005: XIX) and Greenberg (2005), and are discussed in more details in the next paragraph. In addition to this, one should take into consideration the numerous morphological correlations which supposedly connect the IE family with other contiguous, but different language families, as argued for by the supporters of the so-called macro-families (see for example Greenberg (1987

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<sup>8</sup> there are basically two contrasting theories: a) the ‘Schwundhypothese’, according to which the features and categories present in most/all IE languages but missing in Anatolian (feminine gender, aorist, dual, etc.), have been lost; b) the “Herkunftshypothese”, according to which those features and categories absent from Anatolian have been formed only after Anatolian split off from the rest of the family, in which case Anatolian would be particularly archaic. Either of these analyses is void of problems; see Zeilfelder (1994 / 2001: 9-20).

<sup>9</sup> Carruba (2001a:6) states that there is still one knot to untangle: the Indo-Hittite knot, although this should take place in ‘a more modern, less polemical manner’, than in the twenties or thirties.

<sup>10</sup> Among the various articles contained in this volume see the one by Lehrman, who observes the following (Lehrman 2001:107): “What I find in 2000 is that many scholars have accepted the Indo-Hittite theory in some form – either in name as well as in essence, like Don Ringe (e.g., Ringe 1998b) and Anna Morpurgo Davies (private communication, January 28, 2000, Berkeley, Calif.), or in essence if not in name, as Craig Melchert has done [(2001)]”.

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& 2005) and Comrie [1998]). Furthermore, it has recently been observed that several grammatical endings (case and personal endings), at first believed to be exclusive to IE or the Eurasiatic macro-family, actually occur also in non-contiguous areas, such the Americas, so that both the assumption of 'long distance' genetic inheritance and that of 'borrowing' have to be excluded. In other words, in addition to the traditional dichotomy: 'inherited *vs* borrowed', there is now evidence that there may also be a third, (a priori) equally possible explanation for the correlations individuated among languages at the morphological level (as well as at the phonological / lexical level): 'chance similarities', as clearly illustrated in Campbell (1995 and 2003a)<sup>11</sup>.

Finally, it is worth reflecting on the following remarks made by Harrison (2003:223 ff.), who sums up some of the criticisms raised against the privileged role often attributed to the grammatical correlations (see also Koch (1996: 218-20) & (2001) for similar concerns):

Grammatical objects fare poorly as evidence for genetic relatedness [...]. There can be no regularity assumption for grammatical objects to provide a measure of similarity, because grammatical objects are unique. [...]. Cases like those of morphological person-number paradigms are of particular interest because, although not universal in any absolute sense [...] linguists are surprised neither by their occurrence nor by their non-occurrence, in the verb or noun morphology, not even in closely related languages, or by the occurrence of similar paradigms in non-related languages, or *vice versa*

### 3. 3. Is the morphological evidence malleable?

At this point one could object that the risk of reconstructing false matches within IE grammar is rather low, since one can rely on a wealth of shared (inflectional and derivational) morphology in a great variety of areas. Furthermore, although much of the grammatical evidence put forward by traditional IE studies is certainly rather intuitive and subjective, as it happens, this evidence turns out to fall within the range of what Nichols (1996a: 49 & 64) calls "diagnostic evidence":

Traditionally linguistic kinship was identified on the basis of diagnostic evidence which is grammatical and combines structural paradigmaticity [...] and syntagmaticity with concrete morphological forms. The Indo-Europeanists' *intuitive feel* for what was diagnostic evidence of relatedness corresponds to a computable threshold of probability of occurrence [...]. A grouping can be regarded as established by the comparative method if and only if it rests on individual-identifying evidence

The idea is that one can compute the probability of occurrence in the languages of the world of certain features, certain patterns, such as consonant sequences (in the dimension of syntagmaticity), or the co-occurrence of some grammatical categories and their morphological indicators (in the dimension of

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<sup>11</sup> Campbell (2003a:11) states that: "Highly recommended though such grammatical evidence is, caution in its interpretation is necessary. There are impressive cases of apparent, idiosyncratic grammatical correspondences which, in fact, have non-genetic explanation (accident or borrowing)".

paradigmaticity), etc., whereby the Author assumes that a probability of occurrence of a given phenomenon of 1 in 100.000 is “individual-identifying”. Within IE one finds several, arguably a statistically significant number of instances of individual-identifying diagnostic evidence. The following data, as proposed by Nichols herself (1996a: 47), are among the most quoted ones for the purpose of this discussion (notice that to the Latin and Greek endings reported in this table at least the corresponding Sanskrit endings should be added to complete and strengthen the picture: M. *-as, -am*; F. *-ā, -ām*; N. *-am, -a*):

**Table (I)**

	<i>Masculine</i>	<i>Feminine</i>	<i>Neuter</i>
<b><u>Latin</u></b>			
Nominative	<i>-us</i>	<i>-a</i>	<i>-um</i>
Accusative	<i>-um</i>	<i>-am</i>	<i>-um</i>
<b><u>Greek</u></b>			
Nominative	<i>-os</i>	<i>(*)-ā</i>	<i>-on</i>
Accusative	<i>-on</i>	<i>(*)-ān</i>	<i>-on</i>

Here there are two dimensions of paradigmaticity: case and gender on the one hand, with ‘cognates’ endings combined together with identical distribution on the other. These dimensions of paradigmaticity are also quite productive: there are for example numerous items in the masculine declension: Skt. *-as vs* Gk. *-os vs* Lat. *-us* (and not just nouns but also adjectives and participles: compare Lat. *amatus*, etc.). The same holds true, for example, for the feminine declension, although other classes of nouns may be less numerous, coming down to 5-6 items only. In addition, in many cases these morphological paradigms also occur in connection with lexical items (verbs and nouns) that are typically quite similar, and are in fact considered to be cognates (see for example the various IE terms and related paradigms for ‘name’: Skt. *nāma-*, Lat. *nōmen*<sup>12</sup> etc., as well as the various cognates and paradigms of the verb ‘to be’ and ‘to bear, carry’, etc.). Several, striking examples of diagnostic evidence of this type are reported and discussed in this volume by Kazanas, who, in line with mainstream doctrine, argues that these highly complex, and at times unique patterns of lexical and morphological correspondences cannot be explained either through chance or through borrowing. This is certainly a plausible model of explanation; however, alternative explanations are also possible. First of all (as mentioned in the previous paragraph) there is plenty of evidence that wholesale (nominal and verbal) paradigms of the type under discussion *can* also be the effect of borrowing, as shown by Matras’ contribution to this volume. Second, not all the IE languages do possess those rich, (more or less) consistent, paradigmatic morphological systems we would need to establish a

<sup>12</sup> Compare for, example, the following case endings: Locative *nām(a)ni* in Sanskrit and *nōmine* in Latin; for fuller paradigms and other examples of this type compare Kazanas in this volume (par. 3c.).

wealth of 'individual-identifying' grammatical evidence. In fact, this kind of evidence is found mainly among Indo-Iranian and Greek (the so-called *Greco-Aryan* model), and, to a much lesser extent, in Latin, Germanic, Balto-Slavic, Armenian and Celtic. As to Hittite<sup>13</sup> and the other Anatolian languages, there appears to be consensus on the fact that the "Greco-Aryan model does not work well as an explanation for the Anatolian verb" (to use Clackson's (2007:115) words). As mentioned above, many scholars explain this situation by assuming that the less morphologically rich languages have 'lost' / 'reduced' the original, complex morphological / paradigmatic systems present in the proto-language<sup>14</sup> (this model is in line with the original views by Bopp, Schleicher, Brugmann, etc., who reconstructed indeed a nominal and verbal system very similar to that of Greek and, particularly, of Sanskrit). In this volume this view is embraced by Kazanas as well as (with some reservations) by Di Giovine, who also puts forwards several proposals on how to account for the peculiarities found in the Hittite verbal system. However, other scholars think that the opposite holds<sup>15</sup>, that is: the special bond existing between Greek and Indo-Iranian in their morphological (particularly verbal) system appears to point (mainly) to widespread processes of possibly late contacts and areal diffusion<sup>16</sup>, rather than to archaism. This position is defended in this volume by Drinka, and, to a certain extent, implicitly, by Schmitt, when he claims that (Vedic) Sanskrit is not necessarily the oldest language within IE (see below). In particular, Drinka proposes a "stratified model of PIE, one which takes into account the systematic, layered morphological correspondences of Greek and Indo-Iranian". This in turn means that those "languages which share only archaic elements, such as Hittite and Germanic, are presumed to have separated from other IE languages at an earlier time". Thus, Drinka relies on the "stratification of data" – innovative layers building on more archaic layer – to account for the morphological correlations observed among the IE languages (for a 'stratified' interpretation of IE see also Andersen's chapter).

The idea of the existence of some sort of 'Indo-Greek branch / unity' is not totally new, having already been proposed, for example, by Pisani (1933, 1940 & 1974). As is known, this idea was mostly rejected at the time, since there are several other grammatical (as well as phonological) isoglosses that cut across such an assumed branch (compare for example, the reduplicated perfect of Germanic, or the future ending in -s of Lithuanian, shared with Sanskrit and Greek<sup>17</sup>). Some scholars still nowadays continue to reject the thesis of the Indo-Greek unity, arguing that it is a rather naïve notion, based on selected, partial evidence.

<sup>13</sup> Hittite has no aorist; it has one preterit in opposition to the present tense; it has only indicative and imperative, no reduplicated perfect and no separate thematic and a-thematic conjugations. The verbs either follow the *-mi* conjugation or the *-qi* conjugation, although there are claimed to be a few, good matches to stems, suffixes and even whole paradigms with other IE languages.

<sup>14</sup> It could be argued that the thesis of the 'loss' of rich morphology is a typically 'circular' explanation, even if (one could counter-argue) this explanation appears to be supported by the fact that the modern IE languages have all manifestly lost the original inflected structure, as shown by the modern Indo-Iranian languages, the Romance languages as well as the case of Romani and Domari, for which see again the chapter by Matras in this volume.

<sup>15</sup> The issue of the presence *vs* absence of a rich (mainly inflectional) morphology has long been known; see for example Fortson (2004:224-5); (Meillet 1908); Birwé (1955); Meid (1975); (Neu 1976); Polomé (1981 & 1985); Drinka (1993/95, 1995, 2001 & 2003); etc.

<sup>16</sup> There appears to be evidence, from archaeology and palaeo-anthropology, of pre-historical, as well as, of course, historical contacts between Asia Minor and the Indus Valley. See Sedlar (1980); Tucci (1982); Rawlinson (1916 & 1975) and Basham (ed. 1975).

<sup>17</sup> Compare also the isoglosses that separate Hittite and Italic from Sanskrit and Greek (such as augment in past tense and prohibitive Skt. *mā* and Gk. *mē*), or the morphological isogloss of the locative plural ending *-su* / *-si*, which encompasses Sanskrit, Greek and Slavic. On the other hand, palatalization separates Sanskrit from Hittite, Greek and Italic. Notice also that palatalization is not at all a clear-cut isogloss, especially in Baltic and Slavic, for which see Miller (1976).

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This is clearly stated in Kazanas (2004) and claimed by Kazanas in this volume. However, Drinka (in her contribution to the volume), on the basis of new data and new methods of analysis, argues that this unity does indeed exist, and uses the definition: “eastern languages” to refer to the Greek and Indo-Iranian special morphological bond. On the other hand, Di Giovine (in this volume) argues that looking at the issue in simple, binary terms of ‘archaism’ *vs* ‘innovation’ does not lead anywhere, since the status of the IE verbal system is much more intricate and subtle than this dichotomy would lead us to believe. All this, once again, ties in with, and lends support to, the well known fact that a clear-cut IE family tree is still difficult to draw, both at the phonological and morphological level<sup>18</sup>. This is why Drinka insists that areal / contact models of interpretation have to be adopted in addition to, or, actually, incorporated into, the traditional paradigm: only an integrated, three dimensional model (“an amalgamation of family tree and wave models”) can “explain” the nature and distribution of the correlations found among the IE languages (as also pointed by Andersen (1998: 420) and in this volume).

Whatever the case, one has to admit and reflect on the fact that the morphological reconstructions are essentially based on a few core languages, whose morphological correlations – whether the result of archaism, or innovation, or borrowing, or convergence – are nevertheless well attested, whilst “the attribution of the other languages to the family is necessarily done on partial evidence” (as stated by Morpurgo Davies; Cambridge Seminar<sup>19</sup> [2005]).

### 3. 4. Are morphological reconstructions predictive?

It is reasonable to ask at this point whether the circularity issue (as discussed above with regard to phonology and lexicon) could have a negative impact also on the morphological reconstructions, including the area of diagnostic evidence. In fact, here too scholars can increase the power of the comparative method by making recourse to a number of ‘adjustable parameters’. Actually, the risk of circularity is higher at the morphological level, because of the (supposed) pervasive operation of analogy and the lack of a rigorous definition of ‘morphological correlation / similarity’ (as mentioned above). One can compare at this regard the many difficulties, obscurities and exceptions encountered in the reconstruction of the IE verbal and nominal paradigms. For example, with regard to the (thematic) *o*-stem<sup>20</sup> class of nouns referred to in Table (I) above (certainly a highly productive class of nouns, displaying rather consistent paradigmatic patterns), one can nevertheless find several, more or less serious difficulties. For example, some case endings cannot be reconstructed in an economic, straightforward way, or cannot be reconstructed at all, so that a number of ‘adjustable parameters’ have to be introduced in order to overcome the observed mismatches. The adjustable parameters in question include:

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<sup>18</sup> This problem emerges again in the various IE ‘philogenies’ attained through ‘cladistic’ techniques. See Gray & Atkinson (2003); Ringe, Warnow & Taylor (2002); Garrett (2006), Forster & Renfrew (2006), and again Drinka in this volume.

<sup>19</sup> Between October and December 2005 the Faculty of Classics of the University of Cambridge hosted a series of seminars on issues in IE linguistics, organized by A. Marcantonio.

<sup>20</sup> As an example of a well known noun belonging to this class (masculine), compare: Gk. λύκος, Lat. *lupus*, Goth. *wulfs*, OCS *vlikŭ*, Lith. *vilkas* ‘wolf’; etc.

1. a chain of (at times unverifiable) assumptions, including syncretism and reshaping through analogy;
2. a chain of (at times unverifiable) intermediate reconstructions and alternating forms;
3. a chain of minor but frequent (and, at time unverifiable) language-specific sound changes,
4. if any of these procedures fail, there is always the possibility of giving different interpretations to the internal structure, function and origin of the case endings themselves.

As concrete examples of the complications involved in the reconstruction of several case endings in this class of nouns compare the following<sup>21</sup>:

1. The instrumental singular, whose reconstruction requires the help of a laryngeal segment and vowel alternation: *\*-e-H<sub>1</sub> ~ \*-o-H<sub>1</sub>*, whereby the ending is unmistakable attested only in Indo-Iranian *-ā*<sup>22</sup> (although here too only in a dozen or so forms in the Rigveda, the usual ending being, even in the Rigveda, *-enā*); no *o*-stem form is quotable from Mycenaean.

2. The genitive singular, whose overall picture is not clear, and for which at least two alternative reconstructions have to be postulated: a) the alternating type *\*-os(y)o ~ \*-es(y)o*, attested in Indo-Iranian, earliest Greek and Italic; possibly also in Germanic and Balto-Slavic, although with some obscurities in its development, and, possibly, in the Hieroglyphic Hittite *-asi*; b) the type *\*-ī* (Italic), found in Latin and Celtic (whereby scholars appear to be still divided as to whether the two types of endings do or do not have an ultimate, common origin; see also Hock [1986:453]).

3. The locative singular *\*-ey ~ \*-oy*; here there are some doubts as to which of the two variants may have been the original form.

4. The ablative singular, variously reconstructed as *\*-ad ~ \*-at ~ \*-h<sub>2</sub>et* (whence *-āt* in Vedic and *\*-ōt* in Italic). This does not appear to have the expected reflex in Lithuanian and does have some other, minor oddities in Latin adverbs in *-ē*.

5. The nominative plural *\*-ōs* in nouns and *\*-ōy* in pronouns, whose original distribution (Indo-Iranian, Germanic, Osco-Umbrian, and, with a bit of remodeling, Hittite [*at-te-eš*]), was otherwise "disturbed" (to use Sihler's (1995:261) words), as shown in Old Latin *poploe*. In the Rigveda fully a third of the endings of nominative plural are represented by another ending: *-āsas*, which also occurs in Avestan.

6. The dative / ablative / instrumental / locative plural, which are expressed by various endings: a) *\*-ōys* (attested in Vedic, Greek and Italic; see Old Latin *poplois*); b) *-ebhis*, which is the instrumental plural, occurring in Vedic with almost the same frequency as the ending *-āis*, derived from *\*-ōys* (it is questionable whether an ending of instrumental case can be equated to that of a dative/ablative/locative); c) Old Indian

<sup>21</sup> See Mayrhofer (1973:38-9); Szemerényi (1996:183-7); Sihler (1995: 257-265); Meier-Brügger et al. (2003:198-9); Rix (1992:135 ff.); Fortson (2004:113-7); Clackson (2007: 9-10), etc.

<sup>22</sup> This ending is ambiguous as to its grade.

*-bhyas*, to which Latin *-bus* in the other declensions corresponds; however, in Germanic and Balto-Slavic there is an ending *\*-m-* (*\*-mis*); d) locative *\*-oi-su*, present in Vedic, Homeric Greek, etc.

7. Finally, notice the Genitive plural *-ē* of Gothic, “still an unsolved problem” (according to Szemerényi [1996:185]).

Accounting for these morphological difficulties (however minor they may look) requires a number of justifications which all contribute to build up the complexity and, most likely, the ‘post-dictive’ nature of the explanatory apparatus.

### 3. 5. Counter evidence within morphology

It has been argued above that the morphological evidence on which the IE theory is founded appears to be rather ‘malleable’, this problem being caused by the general lack of criteria and guidelines on how to evaluate morphological correlations. This in turn means that it is equally difficult, if not impossible, to identify evidence potentially counter to the model (with perhaps the only exception of the evidence from languages in contact that wholesale morphological paradigms *can* be borrowed). For example, the absence of the fundamental IE category of feminine gender in Anatolian<sup>23</sup> could be considered a paradigmatic example of the inability on behalf of the IE theory (and the methods of historical linguistics in general) to make very clear-cut, testable predictions. There have been no claims (as far as we know) that the absence of this ‘diagnostic’ category should be classified and set apart as potential evidence counter to the IE origin of Anatolian<sup>24</sup>. Quite the contrary, efforts have been devoted only to finding traces of the feminine gender here and there or to justify its absence in the most plausible, natural way (see for example Oettinger (1987); Melchert (1992); Justus<sup>25</sup> (2002), Clackson (2007:104ff.), etc). The same could be argued with regard to one of the most puzzling aspects of Hittite verbal morphology, the ‘*qi*-conjugation’, which, admittedly, does not easily slot into any reconstructed category of IE (although there are claimed to be some good lexical correspondences between verbs adopting this conjugation and verbal roots in other IE languages; see Jasanoff (2003); Neu & Meid (eds, 1979); Clackson (2007:139 ff.) and Carruba in this volume). The situation does not appear to be much different at the level of morpho-phonology, as is evident from the state of the research regarding the IE *Ablaut* (vowel alternation / apophony). Here, the scanty and irregular distribution of vowel alternations across the IE area has generated an array of complex, sophisticated explanations, none of which however appears to be either testable or satisfactory, as clearly illustrated in the chapter by Carruba in this volume. Carruba suggests that the original function of *Ablaut* is to be sought

<sup>23</sup> Hittite, for example, has no nominal declension corresponding to the feminine stems in *\*-eh<sub>2</sub>* or *\*i-h<sub>2</sub>* (as these are currently reconstructed).

<sup>24</sup> The same could be said about the many difficulties and mismatches encountered at the phonological level between Anatolian and the other IE languages (as clearly pointed out by Melchert (1994); Kimball (1999) and Hawkins & Morpurgo Davies [1998]). Compare also Zeilfelder’s view (1994/2001:3 ff.), according to which the phonology (and lexicon) of the Anatolian languages is not of much help for the purpose of verifying either the thesis of the archaic or that of the innovative nature of the Anatolian branch.

<sup>25</sup> Justus’s article (2002:121-123) offers a summary of “contemporary views” on the problem of ‘gender’ in Hittite and related attempts to solutions.

in the 'deixis' – a rather archaic, elementary function, but a very productive one within IE. He ascribes the poor attestation of coherent patterns of vowel alternations across the IE area to the very ancient origin of phenomenon itself. This is certainly a plausible explanation; however, again, there may be alternative explanations. For example, it could be argued that the *Ablaut* present in Sanskrit (and partially in Greek), does not represent a phenomenon to be traced back to the IE proto-language, but on the contrary, a language-specific phenomenon. This is, in fact, basically Kazanas' opinion, as expressed in his assessment of *Ablaut* in this volume: Sanskrit would be the only IE language that has roots and a "proper vowel gradation". Compare also Marcantonio's contribution, which investigates the 'statistical significance' (or lack of it) of the present *vs* perfect vowel alternation. Finally, compare Di Giovine (1995) and Di Giovine in this volume, who instead embraces a more conventional approach to IE *Ablaut*.

### 3. 6. Is there hard core evidence at the foundation of Indo-European?

Bearing in mind the shortcomings of the morphological correlations as discussed above, it is understandable how several scholars, such as Campbell (2003a & b) and Morpurgo Davies (Cambridge Seminar 2005; see note [19]) have reiterated and emphasized the role of the phonological (/lexical) correspondences for the purpose of assessing genetic relations. One could compare also the case of Proto-Dravidian, where it is the lexicon that has seen the greatest amount of relevant, comparative work, as illustrated by Steever (1996). As a matter of fact, phonology has at times offered us the possibility of testing the validity of the comparative method, in the sense that some predictions made by the comparative analysis have subsequently been proven correct in connection with the discovery of new linguistic material. For example, new evidence from Hittite and Linear B Mycenaean has confirmed the validity of earlier reconstructions, in this way also allowing us to redefine the processes which lead to the attested languages. The evidence in question includes:

1. The laryngeal segments found in Hittite, which would fill in and confirm the existence of the 'coefficient sonantique' predicted by Saussure in his *Mémoire*.

2. The so-called 'r/n-stems' (the best known class of 'heteroclitic' stems), an archaic group of nouns<sup>26</sup> displaying a different final consonant in the direct *vs* oblique cases. This alternating stem had been reconstructed (through the means of both internal and external comparison) long before further evidence for the alternation became available with the discovery of Hittite, where, in fact, this class of nouns is somewhat productive (contrary to what happens in the other languages, where it is a relict; compare Latin *iter ~ itineris*, or Greek *ἵδωρ ~ ἵδατοζ*).

<sup>26</sup> Famous heteroclitic r/n-stem, neuter nouns include: 1) the term for 'water': \*wod-r/n-, with alternating stem \*wed-n-, preserved almost exactly in Hitt. *wātar* → Gen. *witenaš*; 2) the term for 'fire': \*peh<sub>2</sub>-w<sub>1</sub>, continued most faithfully in Hitt. *paqqur* → Gen. *paqqenaš*; 3) the term for 'blood': *ēšqar* → Gen. *ešqanaš*. Compare also: 4) Lat. *femur ~ femin-is* 'thigh'; 5) *iecur ~ iecineris* (from earlier *iecor ~ iecinis*), the latter corresponding to Skt. *yakṣ ~ yakanas* 'liver'. Contrary to what happens in Anatolian, the original mode of inflection is scantily attested elsewhere, in which case 'conflation of paradigms' is typically assumed (see Szemerényi (1996:173); Fortson (2004:110-2) & Clackson [2007: 94-5]).

3. A series of separate sounds in Mycenaean, testifying in favor of the postulated IE labio-velars stops, which were lost by the time of alphabetic Greek (having merged with other stops according to specific rules and dialects). This distinct series of stops, transliterated as *q*, not only are attested in Linear B, but are also still preserved exactly where predicted<sup>27</sup>, as shown, for example, by *qe* 'and', with *q* as a reflex of labio-velar (< \**k<sup>w</sup>e*).

These are undoubtedly remarkable discoveries, and fine analyses. However, the data and analyses under discussion are not void of difficulties. For example, with regard to the Mycenaean material and the postulation of labio-velar segments, one should bear in mind that, given the Mycenaean spelling system, we do not really have direct proof that the segment in question is indeed a labio-velar, although this represents, without doubt, "a strong contender" (to use McMahon & McMahon's (2005:14) words<sup>28</sup>). As to the heteroclitic stems, it is recognized that the Hittite evidence is contradictory, or at least unsatisfactory, since not all the *r/n*-stem Hittite nouns available appear to have IE counterparts, as clearly pointed out by Sihler (1995: 299-300). Furthermore, one could observe that the more linguistic material, the more *comparanda* one brings into the equation whilst doing comparison, the easier it is to find a phonological /lexical (as well as morphological) match with a given form or reconstruction. As Campbell (1998: 277) puts it, referring to the issue of mass-comparison and macro-families: "The potential for accidental matching increases dramatically ... when one increases the pool of words from which potential cognates are sought or when one permits the semantics of compared forms to vary even slightly" (for similar remarks see Hock (1993)<sup>29</sup>; compare also the chapter by Marcantonio in this volume). Finally, one could always argue that a few instances of fulfilled predictions – the occurrence of the right, predicted reflex at the right place – are not enough to prove the supposed predictive nature of all the conventionally established IE laws.

As to the laryngeal theory, it is undeniable that it represents one of the most outstanding examples of a successful theory within historical linguistics. As Andersen (2007)<sup>30</sup> puts it: "From a purely algebraic theory in Saussure; given putative phonetic justification by Hermann Moeller; then actually justified by

<sup>27</sup> Compare Fortson (2004:227): "Before the decipherment of Mycenaean, it had been assumed that the labiovelars were still intact in Proto-Greek [...] even though none of the dialects then known actually preserved them as labiovelars. This assumption had been made because it was the easiest way to explain certain consonant alternations within the dialects [...]. Mycenaean proved this hypothesis correct: it still preserved the labiovelars intact, or at least preserved them systematically as something different from labials, dentals, or plain velars".

<sup>28</sup> Since we do not have a segment-by-segment spelling system for Mycenaean, we cannot directly prove that the segment at the beginning of those syllables (where we would reconstruct a labio-velar) is indeed a labio-velar. All one can safely state is that the symbol in question is not the one used in syllables which have /p/, /t/ or /k/ in other Greek dialects; therefore, the initial consonant must be something else.

<sup>29</sup> Hock (1993:221ff.) observes the following: "Preliminary results [of an experiment in which English, Finnish and Hindi are compared] suggest that enlarging the data base does not improve the reliability of the method. In fact, if there is any change at all it may consist in a slight increase of false friends". On the other hand, Hock (*ibidem*) also recognizes that working through a comparison of only a small number of languages (three in the specific case), may be equally misleading.

<sup>30</sup> Personal communication; 2007.

the discovery of some of the posited segments in Anatolian; found to correlate with the 'prothetic<sup>31</sup> vowels' and 'Attic reduplication' in Greek, and of 'final lengthening' in Homer; and then with the Baltic and Slavic distinction between acute and circumflex long vowels and diphthongs". Nevertheless, the laryngeal theory is not uncontroversial, at least with regard to the number of laryngeal segments to be postulated. Compare for example the divergent views by Martinet (1953), Szemerényi (1973 & 1996:121-6), Meier-Brügger et al. (2003:107), Lindeman (1982), etc. Compare also, for example, Beekes (1989:1 ff.), who argues in favor of three laryngeals, whose number would be required by Greek, although Hittite "certainly points to two laryngeals", and Di Giovine (2005), who, in line with other scholars, calls into question the postulation of /h<sub>1</sub>/, since this segment does not have direct reflexes in any of the IE languages, including Anatolian<sup>32</sup>. However, the main problem associated with the laryngeal is that the advantages attained by making recourse to the laryngeal theory are counterbalanced by a number of disadvantages. For example, Winter (1990: 20-1) referring to Saussure's original idea of the 'coefficient sonantique' and subsequent developments, describes one of the known 'disadvantages' of the laryngeal theory as follows:

We attempt to simplify our statements by subsuming overtly differing phenomena under one common formula which may or may not require positing directly unattested elements conditioning the differences [...]. This analysis had the tremendous advantage that now all subtypes of ablaut in Proto-Indo-European could be treated alike and that canonical forms of roots could be set up; along with this, however, went the disadvantage that the Proto-Indo-European system of vowels apparently had to be reduced in an unreasonable way

Sihler (1995:111 ff.), who reconstructs an *Ablaut* system "considerably leaner" than others thanks to the support of the laryngeal theory, appears to (implicitly at least) identify another, major 'disadvantage'<sup>33</sup>. In fact, he states that the economy of his model is achieved by "a complication elsewhere in the system", because his reconstruction also requires "a number of sound laws" applying to the postulated laryngeals<sup>34</sup>. Here, the fundamental questions (which have not been asked yet, to our knowledge) are: a) how many more sound laws do we need to make the laryngeal system work? b) is this extra number of segments and

<sup>31</sup> The claim that prothetic vowels can be explained through laryngeals in Greek and Armenian has been contested by Wyatt (1972) and Beekes (1969:18-98).

<sup>32</sup> Compare also the volume edited by Vennemann (1989), where the 'new sound of Indo-European' appears to have had triggered some reshaping in the definition of the laryngeal theory. Furthermore, it is now recognized that Hittite does not really lend much support to the laryngeal theory, since the evidence available from this language is, admittedly, 'fragmentary', 'contradictory', 'disappointing' (see Lehmann (1952: 25 ff. & 1993:125); Szemerényi (1996: 121-126 & 137-8); Mayrhofer (1986:123ff), Clackson (2007:58 ff); etc.). The absence of satisfactory evidence for laryngeals from the Anatolian languages seems to have been compensated, in recent years, through data drawn from Greek, which is now claimed to show an unparalleled 'triple reflex', that is, a distinct outcome for each of *h*<sub>1</sub>, *h*<sub>2</sub> and *h*<sub>3</sub>, when they occur within a few specific contexts (see Clackson 2007:58-60). This analysis appears to be gaining acceptance, even though laryngeals nowhere survive as consonants in Greek.

<sup>33</sup> Sihler's reconstruction is much leaner than others because it works with only "a SINGLE pattern of alternations, \**e* ~ \**o* ~ Ø", together with "three new consonants of obscure phonetics", the laryngeals \*H<sub>1</sub>, \*H<sub>2</sub> and \*H<sub>3</sub>. Note however that Sihler does not appear to consider this extra number of rules as a 'disadvantage', that has a negative impact on the 'economicity' of the system. This is, in fact, an interpretation of the Author (A. M.) of the present *Introduction*.

<sup>34</sup> Similar remarks can be found also in Collinge (1985) and Gusmani (1979).

required operating rules added to the system as high as to nullify the otherwise attained benefits? In this regard, Marcantonio (in this volume) argues that adding laryngeal segments to reconstructions does indeed increase the explanatory power of the comparative method<sup>35</sup>.

Thus, if we accept the analyses, objections and counter objections expounded thus far, it is still not clear what exactly the uncontroversial, hard core evidence that lies at the foundation of the IE theory is supposed to be<sup>36</sup>.

#### 4. Is there a pre-historical reality behind reconstructed Indo-European?

##### 4.1. Conventionalism *vs* realism

The 'conventionalist' *vs* 'realist' approach to linguistic reconstruction, and related concept of proto-language (in general as well as within IE), is another long standing debate within historical linguistics, and one for which, yet again, there does not appear to be much of a consensus. Compare Koerner (1989) for an overview of the debate; compare also Bonfante (1945); Pulgram (1959 & 1961); Meid<sup>37</sup> (1975) and Clackson (2007:16 ff.). It is true that between the extreme conventionalist approach on the one side and the full realistic approach on the other there are plenty of more moderate, intermediate positions. Nevertheless, it appears that the fully realistic approach, which in turn is based on the controversial method of palaeo-linguistics<sup>38</sup>, is the one that has so far attracted many supporters, and not only among archaeologists (such as Mallory (1989 & 2001) and Renfrew [1987, 1990]), but also among linguists. For example, in his criticism to the thesis by Gimbutas (1970) regarding the location of the IE home land, Schmitt<sup>39</sup> (1974: 283) observes that "[with] the methods of linguistic paleontology anything can be proved as Proto-Indo-European, but it can *not* be proved as *typically* Proto-Indo-European". Thus, Schmitt appears to have confidence in the validity of the realist approach, although he has doubts regarding the factual interpretation of single reconstructions and the hasty conclusions reached about the reconstruction of single 'pre-historical facts'. It could be said that in this case too (like in the case of the 'regularity principle') it is not always made explicit what the motivations are, what the evidence is, that provides the basis for the choice of one

<sup>35</sup> For an up-to-date state-of-the-art report on the laryngeal theory & essential bibliography see: Polomé (1987); Mayrhofer (2004:17-39); Meier-Brügger et al. (2003:106 ff.); Di Giovine (2005) & Clackson (2007); see also Sturtevant (1942).

<sup>36</sup> Last, but not least, one should take into consideration also those further limitations of the comparative method as pointed out in recent years by scholars such as Dixon (1997), Aikhenvald & Dixon (2001) and Nichols (1992, 1993/1995, 1996b, 1997 & 1998); see also Andersen (2006). These are: a) the inability of the method to get at a more remote linguistic prehistory than the generally assumed 8000/6000 years); b) its inability to account for the actual distribution of the degree of linguistic 'diversity' *vs* linguistic 'uniformity' found in the world. These issues are not dealt with in this volume.

<sup>37</sup> For example, Meid (1975), in proposing his 'Space-Time' model, claims that the spoken IE parent language did exist, and that there must have been a corresponding very small, tight-knit community which slowly expanded over a vast area, and through a long stretch of time. On the contrary, Clackson (2007:16) states that PIE "is not a real language" and that: "Reconstructed IE is a construct which does not have any existence at a particular time and place".

<sup>38</sup> Among the criticisms raised in the literature against the methods of palaeo-linguistics see Renfrew (1987:77-86, 103-104, 109-110) and Marcantonio (2002/5); compare however Polomé (1990) for a rather positive evaluation.

<sup>39</sup> Please, note that although R. Schmitt contributes with a chapter in this volume, his contribution by no account deals with the issue of how real(istic) reconstructions may be.

approach, or the other. In this volume, this issue is dealt with, or touched upon, by Andersen, Bryant, Drinka and Di Giovine. The chapter by Andersen deals (also) with the ontological status of proto-languages and the relation between such a 'language' and community languages. The chapter by Bryant (also) discusses the 'malleability' of the palaeo-linguistic evidence relating to the origin of the Sanskrit language and culture. The chapter by Drinka draws attention (among others) to the fact that the linguistic evidence (in particular, specific morphological correlations) provides more complete information as to the relatedness of population and to historical /cultural reconstruction than archaeological records. Di Giovine, on the basis of the analysis of the IE verb inflectional system presented in his contribution, argues that PIE was not a compact language or not even a community language at all. Compare also the chapter by Häusler, as discussed in the following paragraph.

#### 4. 2. The Aryan debate

Within the camp of the realist approach the debate has revolved mainly around the issue of the whereabouts of the (assumed) IE proto-community. A strand of this debate has recently re-surfaced under the definition of: 'the Aryan Debate', for which see Bryant (2001), Bryant & Patton (eds, 2005), Kazanas (2001, 2002, 2003 & 2004); Polomé (1985); Tripathi (ed., 2002/2005); Masica (1991:32-60), as well as the two debate-volumes on the origin of complex societies in Central Eurasia, edited by Jones-Bley & Zdanovich (2002). On this topic, compare the chapter by Bryant, Drinka, Häusler and Kazanas (and, to a certain extent, Schmitt) in this volume. The Aryan debate centers on the following, major interconnected issues:

1. Was the assumed homeland located in the west, somewhere in Europe, or in the east, somewhere in North India /Pakistan? In other words, were the bearers of the Old Indo-Aryan (Vedic and Sanskrit) culture indigenous or intrusive in North India?
2. Why, how, when and in which direction did the original proto-community disintegrate and migrate / spread, so as to bring about the distribution of the IE languages as we found them in historical times?
3. How old is Old Indo-Aryan? Is it as old as the traditional IE theory claims (about 1700/1500 BC), in line with the 'migrationist' model, or is it much older than that, in line with the 'indigenist' model? In turn, these questions are connected with the following major issue (already touched upon above):
4. Is Old Indo-Aryan the most archaic, and therefore, the most important language for the purpose of reconstruction?

The issues raised in points (3) and (4) are of particular relevance, not only because they are strictly connected to the home land issue, but also because the assumption of the great antiquity of (Vedic) Sanskrit is (or, at least, it has been) one of the pillar tenets at the foundation of the IE theory, and related traditional reconstructions. As we have seen above (par. 3. 3.), this tenet is now under attack, since several scholars believe that the *Greco-Aryan* morphological paradigm represents in fact innovation, and not archaism. As to the more specific issue of the archaicity of Sanskrit, Schmitt in this volume calls into question this

traditional view, arguing that: “the archaism particularly of the Old Avestan language makes it only too clear that, despite the old age of the earliest Vedic texts, the Old Indo-Aryan language is not the only fundament of the IE proto-language [...] and that its data are not with absolute necessity genuinely antique; therefore Old Indo-Aryan (both Vedic and Sanskrit) is not so close to PIE as many people think”. On the other hand, Kazanas (quoted, and in this volume) supports the traditional way of thinking as well as the indigenist model claiming exactly the opposite: Old-Indo-Aryan is indeed the oldest language within the family (actually, much older than conventionally assumed), as well as the closest language to PIE, even though a PIE language cannot be reconstructed. In turn Drinka, in her contribution to this volume, rejects the indigenist / ‘Out-of-India’ model, arguing against it (mainly) on the basis of those very “archaic” morphological isoglosses Indo-Aryan shares with some IE languages and those very “innovative features” it shares instead only with Iranian and Greek.

#### 4. 3. Sanskrit and the South Asia linguistic area

This already articulated debate is in turn connected with the other, equally tangled issue of how to interpret the lexical, phonological, structural and even morphological correlations that have been identified as existing between the Old (and Modern) Indo-Aryan languages and the other non IE languages of India, such as (mainly) Dravidian and Munda. In fact, these (and other) various languages / language families of India are widely claimed to form what is usually referred to as the ‘South Asia linguistic area’, for which see Steever (1993:10 & 1996:11 ff.); Masica (1976) and Emeneau (1956, 1971 & 1980). In particular, on the basis of (supposed) lexical borrowing from Dravidian into Indo-Aryan and from the modern geographical distribution of the Dravidian languages, it is often claimed that Dravidian and Indo-Aryan must have been in contact – since prehistoric times– in those extreme northwestern areas of India first inhabited by the Indo-Aryans (but see Hock (1996:38) and discussion below for different points of view<sup>40</sup>). Thus, the relevant question here, the question which has been hotly debated and whose resolution, if ever attained, could help to break the deadlock of the indigenist *vs* migrationist model – together with the issue of the degree of antiquity of Sanskrit – is the following: Are the non IE features present in Old-Indo-Aryan the result of sub-stratum, super-stratum, ad-stratum, convergence, or even genetic inheritance? At this regard, compare the (at times clashing) views by scholars such as: Burrow (1946, 1973 & 1975); Emeneau (1980); Hock (1975, 1986, 1996 & 1999); Kuiper (1955 & 1991); Witzel (1999 & 2000); Kuzmina (2003); Misra (2005); Kazanas (2004) and Kazanas in this volume. Several scholars, such as Southworth (1979), Kuiper (1955, 1991 & 1992) and Burrow (1973), have compiled long lists of (Rig-)Vedic words believed to be of Dravidian or, more generally of non-Aryan origin. Other scholars, particularly Hock (1996: 36ff.), have drawn attention to the fact that the situation is not that clear-cut after all. In fact, in many instances it is difficult to trace back the origin of the lexical as well as structural similarities observed in this intricate linguistic area, and to sort out whether they are attributable to borrowing, convergence, or chance resemblances<sup>41</sup>. On the other hand, there are Indian scholars (such as

<sup>40</sup> Hock (1996:38) states that: “given the multiply different possible explanations, no explanation can be considered certain enough to be used as evidence for or against prehistoric Dravidian/Indo-Aryan contacts”.

<sup>41</sup> Hock (1993) observes that within well established families too, such as IE, a significant number of chance resemblances may occur, or, conversely, a number of cognates are very hard or even impossible to detect because of the changes they have undergone through time.

Misra (2005) and Aiyar [1975]) who challenge the traditional classification of Sanskrit as an IE language, observing that under a different socio-politico-cultural context, the similarities under discussion could have been interpreted differently. For example, they could have been ascribed to the postulation of a shared, common ancestor for Sanskrit and Dravidian, and the studies of the South Asian linguistic area, as we know them today, might have taken a totally different course, generating a different production of knowledge, and, possibly, a totally different proto-language as the ancestor of Sanskrit. In turn, these scholars are accused to be strongly biased in their research, being driven by nationalistic feelings and political motivations. Certainly – it has to be said – there are elements of truth in this statement, since the main concerns (mostly unexpressed) of these Indian scholars are for the cultural unity of India under a Sanskritic proto-banner, whereby Dravidian would be some sort of Sanskritic prakrit (on these issues see the chapter by Bryant and Annamalai & Steever in this volume). Nevertheless, it is also fair to recognize that had the events, the accidents of History been different, in other words, had Sanskrit not been brought to the attention of the western scholars (as a consequence of the English colonization of India, for which see Bryant [2001]), the cultural scenario of historical linguistics might well have been totally different indeed, and one can easily imagine how a different paradigm might have been created to account for the origin of Sanskrit and the other languages (/language families) of India. Actually, the fact that the initial choice of comparing a certain pair / set of languages, rather than others, is by its own nature ‘intuitive’ and may be also influenced<sup>42</sup>, or even dictated by circumstances external to linguistics, can increase the risk of circularity embedded in the traditional methods of analysis, as discussed above (for similar remarks see Clackson<sup>43</sup> [2007: 3]). It would therefore be desirable to be able to ‘demonstrate’ that the correlations shared by Sanskrit and Dravidian, for example, are the genuine effect of a typical process of *Sprachbund* convergence, rather than simply ‘labeling’ them as ‘contact-induced’ just because it is widely accepted that the languages in question are not genetically related. The thesis that the correlations shared by Sanskrit and Dravidians are the genuine effect of *Sprachbund* is embraced by Annamalai & Steever. In their chapter the Authors do not directly address the issue of the origin of the non IE features present in Old (and Modern) Indo-Aryan languages, but argue that Dravidian is a clearly distinct linguistic family from Indo-Aryan, although the latter does indeed display Dravidian features. In other words, according to Annamalai & Steever, “some of the four genetically distinct language stocks in South Asia have clear genetic linguistic relations outside of the subcontinent”. As a matter of fact, a comparison of two classical languages of India, Sanskrit and Old Tamil, as carried out by the Authors, would reveal and illustrate a typical *Sprachbund* situation, where “two languages may be genetically distinct, yet grammatically related”.

At this point it is interesting to observe that there is at least one aspect within this intermingled debate about which there appears to be consensus among all scholars: there is no archaeological evidence for the assumed migrations of (part of the) original IE community, either from a supposed western homeland eastward into North India, or the other way round, as pointed out by Bryant (2001), Chapman & Hamerow

<sup>42</sup> The classification of Sanskrit is not the only known instance of linguistic classification which has been influenced by historical /political events. There is a clear, well documented example of this process: Hungarian, for which see Marcantonio 2002.

<sup>43</sup> Clackson (2007: 3) says: “The operation of the comparative method does not guarantee a language’s place in the family; only the initial recognition that two or more languages are related can do that [...]. When does a linguist decide that there is enough material to relate a language to the IE family? There is no absolute set of criteria beyond the general rule that the evidence must convince both the individual linguist and the majority of the scholar community”.

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(eds, 1997) and Häusler (2002, 2003a, b& c, & 2004). Compare also the contribution by Häusler and Kazanas in this volume. Häusler (2004:1) points out that the explanation of the spread of the Indo-European languages as “the spread of the culture of a concrete group of people from a cradle [...] has not been proved by anthropology and archaeology as yet”. The Author (*ibidem*) also believes that “all attempts to reconstruct the old culture of the Indo-Europeans as existing in a concrete cradle, by the means of ‘linguistic paleontology’ are wrong”. On the other hand, the objection could certainly be raised that the ‘absence of evidence is not evidence of absence’, so that scholars are faced here with yet another area of IE studies where the ‘evidence’ (or lack of it) is malleable and un-decisive. As a matter of fact, Häusler himself believes that “the IE linguistic community must have existed at some point in ancient times, since the linguistic classification has been fairly safely established, even if this (assumed) linguistic community is not at all retrievable by the means of archaeological and anthropological research” (as stated in his contribution to the volume). Whatever the case, the fact remains that, thus far, the required, supporting (one way or the other) archaeological and palaeo-anthropological evidence has not been found.

As to the palaeo-linguistic evidence – place and river names, terms for flora & fauna, names of deities, etc. – mainly from Sanskrit, some scholars claim that this evidence is malleable, and therefore inconclusive when it comes to trying to establish the indigenous or intrusive character of Old Indo-Aryan; see Bryant (2001) and Bryant in this volume. In contrast, other scholars claim that the palaeo-linguistic evidence clearly supports the indigenist / ‘Out-of-India’ model; see for example Kazanas (2004) and Kazanas in this volume. Drinka, on the other hand, argues that the thesis of late contacts between Greek and Indo-Iranian – as put forward in her contribution to the volume – constitutes one more piece of evidence that the ‘Out of India’ theory is untenable.

Finally, it is worth mentioning that recent genetic evidence (whatever its relevance may be in this context) appears to support the indigenist hypothesis of the origin of the Sanskrit speaking peoples and culture; see Kazanas (2002 & 2004), as well as Kazanas’ and Bryant’s chapter in this volume.

#### 4. 4. The conventionalist approach

Those scholars who adopt the conventionalist approach to reconstruction claim that linguistic classifications do not necessarily imply and guarantee the existence of the corresponding speech community. Therefore, searching for the original IE community, and the pre-historical, highly intertwined linguistic and extra-linguistic processes that would have brought about the IE languages, is a pointless task. This remains the case even if linguistics is assisted by other disciplines whose methods of analysis are claimed to have recently achieved high levels of reliability, such as archaeology and genetics. In other words, comparative historical linguistics cannot in any way shed light onto pre-historical ‘facts’ (homelands, migrations, institutions etc.). Historical linguistics was not meant to be, it is not and cannot be used as a ‘branch of prehistory’. As Harrison forcefully (2003:231) puts it:

Too many comparative historical linguists want to dig up Troy, linguistically speaking. They consider it more important that comparative historical linguistics shed light on prehistoric migrations than that it shed light on the nature of language change [...]. I do not consider comparative historical linguistics a

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branch of prehistory, and I sincerely believe that if we cared less about dates, maps, and trees, and more about language change, there'd be more real progress in the field

## 5. Introduction to the chapters (in alphabetical order)

**Chapter II.** Henning Andersen's chapter: *The satem languages of the Indo-European Northwest. First contacts?* focuses on some of the key concepts of comparative linguistics: *Stammbaum*, proto-language, the relation between stemmas and the '(re)construction' of language histories, diffusion, Schmidtian dialectal areas, etc. The Author's specific field of research is the Slavic and Baltic languages – a very good starting point for the debate in question. In fact, these languages show obvious similarities, underpinned by regular and systematic correspondences on the one hand, and "significant irregularities in phonological development, divergences in morphology and deep differences in vocabulary" on the other hand. This in turn means that a coherent, pre-historical account of their origin and mutual relationships proves difficult to draw. In particular, Andersen observes that even some of the regular correspondences holding within and among the two branches, such as instances of regular developments of their respective accent (/accent system), may be deceiving, in the sense that it is difficult to ascertain whether these developments are the result of shared innovation or, instead, of parallel but independent developments, triggered by language contact (including contact with pre-IE languages). Actually, the Author argues, some of the earliest accentual features shared by Baltic and Slavic, traditionally interpreted as shared innovation and, therefore, as inherited, are in fact most likely to be the result of separate development. The Author further remarks how the laws conventionally established to account for the Balto-Slavic accentology cannot really be considered 'law', since they are merely plain, "laconic restatement" of the observed phenomena, often relying upon massive recourse to unjustified (and atypical) analogical changes. A new, more rigorous interpretation of the relevant data and processes, a "paradigm shift", is then proposed. The Balto-Slavic issue offers Andersen the opportunity to re-affirm his thesis of the "stratification of the IE Northwest", according to which the substantial differences existing between the two language groups as well as within Baltic itself can be accounted for by positing various, distinct substrata, distinct IE language traditions underlying the later pre-Baltic and pre-Slavic dialects. In other words, the European Northwest would be one of those areas where "admigrations" took place, i.e., migrations staggered at different times – just as there must have been other admigrations from the IE core area. It could be said that Andersen criticizes IE/comparative linguistics from within – as it were – accepting the basic paradigm and traditional procedures whilst focusing on appropriate and inappropriate ways of interpreting the established correspondences and tenets, in an attempt to attain realistic and more broadly acceptable results. At this regard the reader might want to compare the chapter by Marcantonio, who calls into question the (supposed) 'scientific' nature of the comparative method and, as a consequence, of the IE family. Compare also the chapter by Drinka, who shares with Andersen the view that a "stratified model of IE relationship" can better account for the relevant evidence. Kazanas, on the other hand, lines up with both Andersen and Drinka in reaffirming the validity of the IE theory in general, but he has no faith in the conventional IE reconstructions.

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**Chapter III.** The chapter by E. Annamalai & S. B. Steever: *Ideology, the Indian homeland hypothesis and the comparative method*, focuses on a wide range comparison of two classical languages of India: Old Tamil and Sanskrit. By paying particular attention to the phonological, morphological and morpho-phonological components of their respective grammar, the Authors point out where and how these languages converge and where and how they diverge. After evaluating the nature of the observed similarities and differences, the Authors argue that Old Tamil and Sanskrit belong to two distinct language families (Dravidian and IE, respectively), according to mainstream view. They also reaffirm the conventional thesis that the shared similarities of these languages clearly bear the diagnostic marks of a *Sprachbund* convergence. Thus, according to Annamalai & Steever, the application of the comparative method to the Indo-Aryan languages continues to uphold the traditional IE hypothesis, by means of the well known, systematic correspondences existing among the various IE languages. On the contrary, those scholars who put forward claims about the historical relation of the South Asian languages to an Indian homeland – also by calling the methods of historical linguistics into question – find themselves at a loss to economically explain the systematic correlations that hold within IE. More specifically, the proposed comparison of the selected structural features of Sanskrit and Old Tamil is claimed to show how these two languages do not incorporate reference to a so-called ‘geographic index’ – an index that would point toward a historical origin of the languages in question in South Asia. As a consequence, the hypothesis of an Indian homeland, as well the criticisms raised against the comparative method, would owe more to ideological currents of thought than to scientific, linguistic analysis. At this regard, the reader may want to compare the views expounded by Annamalai & Steever with those put forward by Bryant and Kazanas. For example, Bryant points out how major areas of the conventional (linguistic and extra-linguistic) evidence on which the (Indo-)European origin of Sanskrit is based, can actually lead to a circular reading. Kazanas instead supports the Indigenist hypothesis for the origin of Sanskrit (/Indo-Iranian) and reaffirms its IE classification, but denies that the IE languages are held together by regular and systematic correspondences.

**Chapter IV.** Edwin Bryant, in his chapter: *The Indo-Aryan migration debate* summarizes the debate relating to the origin of the Vedic culture and language. The numerous, intricate facets of this debate include: the underlying ideological background, some of the more prominent aspects of the data brought forward by either camp (migrationists *vs* indigenists), the methodological shortcomings underpinning the relevant evidence, including a widespread, embedded circularity, etc. The main claim made by the Author is that the relevant palaeo-linguistic evidence, as well as some aspects of the extra-linguistic evidence, is so flexible, so ‘malleable’ that it does not permit a determination of whether the Vedic culture is of local origin or is instead intrusive into North West India. More specifically, much of the evidence associated with the Indo-Aryans, whether philological, linguistic or even archaeological (such as the absence of horse and chariot remains in the relevant archaeological sites), can be construed in sometimes diametrically opposed ways by scholars approaching the issue from differing angles. With little or nothing in the data itself to favor either models of interpretation, marshalling the data in support of one or the other position is clearly a consequence of the starting up assumptions. Then, typically, the selected reading of the data is configured to support the initial assumption, in this way generating an obvious circularity. This circularity becomes particularly acute in the case of the vexed issue of the (supposed) borrowed elements in Sanskrit.

Here again, on many instances the data itself would not allow to trace back the source and /or direction of the borrowing. Thus, if one did not accept that the IE classification is well founded, the overall borrowing issue, the connected substratum *vs* ad-stratum issue, and, ultimately, the conventional classification of the Indian languages involved (including Dravidian and Munda) would certainly demand revisiting. Bryant's views are at odds with the views of those scholars (in general and in this volume) who argue instead that there is enough linguistic and extra-linguistic evidence to allow linguists to support either the indigenist model (see Kazanas) or, conversely, the migrationist model (see Drinka, Annamalai & Steever and Schmitt). Bryant's chapter can be read also in connection with the chapter by Marcantonio, who challenges the normative model of IE historical reconstruction. Finally, Bryant points out that there is at least one clear, undeniable 'fact' amongst so much malleable evidence: no trace of migration (in either direction) can be found in the field of archaeological, paleo-anthropological and historical research (as also claimed by Häusler), whilst the genetic evidence appears to suggest a local origin for the bearers of the Vedic culture (as also pointed out by Kazanas).

**Chapter V.** Onofrio Carruba, in his chapter: *Indo-European vowel alternations: (Ablaut/ apophony)* intends first to review the state of the research regarding the IE vowel alternations, and then to put forward his own views regarding the origin and nature of this phenomenon. In fact, the Author argues, despite intense research and long standing debates, neither the origin nor the original function of the apophony have really been identified. More specifically, Carruba intends to investigate the connection between the "mobility" of position of the accent and the alternating vowels – and related "mobility of meaning" – both at the PIE and the "pre-PIE" stage of development. To do so, one should depart from the traditional schemes of analysis and investigate instead the relationship between the stem of the present in *-e* and that of the perfect in *-o vs* the vowels in question. It is here in fact that one can clearly observe the 'deictic' value and origin of the apophony, both in the sense of its 'temporal' value ('vicinity' (/presence) *vs* 'distance'), and in the sense of its 'markedness' value (*-o* being marked and *-e* unmarked). This would mean that the accent – now such a vital element for the apophony – must not have been that vital, or must have been a pitch type of accent, in an earlier phase of IE. To trace back the ablauting structure of this pre-PIE phase it is not only necessary to take into account the morpho-phonemic schemes of the actual IE languages, but also to make recourse to linguistic notions, linguistic elements that are proven to be really archaic, as is indeed the 'deixis' (a rather elementary notion, but a very productive one within IE). Having identified the 'deixis' as the founding factor of the (pre-*proto*-)IE apophony, Carruba focuses his attention onto the Anatolian languages. Here we would observe a clear example of the "breaking down" of the typical IE ablauting schemes (that are in any way well preserved only in Old Indian, and, to a lesser extent, in Greek). In fact, Hittite typically displays several new ablauting forms (such as *sāk(ah)hi* 'to know'), although it also preserves some old, known ones. Carruba concludes that: "There are still numerous difficulties that hinder scholars from fully understanding the nature and behavior of the apophony [...]. It appears to be lost in the depth of time, at the point that it is practically impossible to achieve an un-equivocal and coherent reconstruction". In line with mainstream doctrine, Carruba ascribes these difficulties of reconstruction to the 'loss' of the relevant *comparanda*, due to the great time depth involved. An alternative explanation for the poor occurrence of the perfect *vs* present *Ablaut* in most IE languages is proposed by Marcantonio. In her opinion, both the perfect *vs* present alternation and the IE *Ablaut* in general may not be an ancient,

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residual IE phenomenon, but simply an artefact of the method of analysis, as is suggested by the clear lack of 'statistical significance' in the relevant data. Compare also Kazanas' view, according to which only Sanskrit possesses a proper *Ablaut*.

**Chapter VI.** Paolo Di Giovine, in his chapter: *Verbal inflection from "Proto-Indo-European" to the Indo-European languages: A matter of coherence?* intends to re-visit that coherent bundle of grammatical features which join together Old Indian, Avestan, Ancient Greek, Gothic, Old Church Slavic, Latin and Hittite, and on which the reconstruction of a PIE verbal system is traditionally based. Against this background, the Author observes, several other grammatical features, several 'independent innovations' can also be identified, whose close scrutiny would show to what extent a different feature "springs up as an independent innovation and to what extent it points to different archetypes" - the latter case in turn would suggest that PIE was not a "compact language, or not even a language at all". In particular, Di Giovine focuses on the still hotly debated issue of the innovative *vs* conservative character of the verbal system of the various IE languages, ranging from the Greco-Aryan verbal system at one end of the spectrum to the Hittite one, at the other. The Author comes to the conclusion that looking at this issue in terms of the usual, simple opposition of: 'archaism *vs* innovation', can be misleading. Similarly, the schematic, binary family tree model is not of much help to properly account for the actual distribution and nature of the morphological (particularly verbal) correlations among the IE languages (as indeed recognized nowadays by many scholars, also in this volume). The type of stratified model of IE put forward in this volume by Drinka and Andersen, according to Di Giovine, does not appear to be satisfactory either. Instead, the model of interpretation that would allow us to 'explain' the morphological correlations in a more realistic way - although by no means in a totally satisfactory way - is the one that depicts IE as a pool of dialectal varieties, a pool of languages that progressively develop and differentiate from one another according to each language internal, specific mechanisms, specific 'drift'. For example, it would be inadequate to consider the Greco-Aryan verbal system purely and simply as an innovation, given that those languages that possess a less complex verbal morphology actually show relics of ancient verbal categories that are no longer productive, or are extinct. This is the case of the Latin *perfectum* (the result of the convergence of aorist and perfect), or the Latin conjunctive of the type: *sīm* and *velim* (formally a continuation of the optative suffix, which is no longer attested in this language). Thus, a detailed investigation of the history of the single languages /language groups would be required in order to achieve "coherent" and reliable results. Such an investigation would reveal how it is the 'simplification' of the inflectional system that represents innovation, rather than the other way round. This view is held in this volume also by Kazanas, although he reaches this conclusion through different arguments and a different approach to reconstruction.

**Chapter VII.** Bridget Drinka, in her contribution: *Stratified reconstruction and a new view of the family tree model*, focuses on the debate of whether and how we can sort out genetic *vs* areal correlations, and connected issues of how to interpret the conventional concepts of IE proto-language and family tree. Drinka starts from the assumption that linguistic evidence is an indispensable resource in cultural reconstruction, one which provides more complete information as to the relatedness of particular groups than the archeological record can furnish. The validity of IE as a construct is not contested here, nor is the

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importance of comparative reconstruction as a valuable tool. What is contested, however, is the oversimplistic characterization of PIE (still maintained by a number of scholars) as a single unified entity, or as an abstraction which serves only to represent the relationship of the daughter languages to one another (not a language which was truly spoken). Drinka presents a critical interpretation of the comparative data, with an eye to constructing a more realistic picture of the development of the IE languages, including stratification of layers and areal contact as important considerations. Specifically, Drinka claims that the conventional genetic model is only endorsed in her analysis as a 'descriptive' model of the ancient relationship of IE languages, but its usefulness as an *explanatory* model is questioned. What the family tree represents is a skeleton, providing a mapping of common ancestry, but no information as to how or why the languages diverged. As it has been amply illustrated since the time of Schmidt (1872), linguistic change occurs through contact, through the spread of innovation across populations. Hence, some similarities among languages will represent inherited features whilst others will represent shared innovations, acquired through contact (on this topic compare also the chapter by Andersen and Matras). Recent work on languages in contact are claimed (by the Author) to help linguists to sort out the data as archaic or innovative, and to construct a stratified view of PIE accordingly. An updated model which attempts to combine the 'family tree' and the 'wave' models is then proposed. The Author argues that, adopting this new integrated model, it is possible to sort out those morphological similarities (such as iterative, inchoative and intensive affixes) that represent an archaic relationship between the IE languages, from those similarities (such as the development of the imperfect) that represent instead later, shared innovations. The stratification of archaic *vs* innovative structures would allow us to recognize not only that the IE languages are genetically related, but also that some IE languages, like Indo-Iranian and Greek, must have remained in contact longer than others, like Hittite and Germanic. This is testified by the fact that, for example, Indo-Iranian and Greek possess an extremely similar temporal-aspectual system, whilst both Hittite and Germanic have extremely simple verb morphologies. Thus, the shared morphological complexity of Indo-Iranian and Greek would not represent archaism, as traditionally assumed, but rather shared innovation, due to late contacts in the "eastern" areas of IE. In this regard, please compare the chapter by Di Giovine and Kazanas, who support instead the traditional model, although through different corpora of data and different arguments. Compare also the chapter by Schmitt, who argues that (Vedic) Sanskrit is not necessarily the oldest language within IE. Finally, the thesis of late contacts between Greek and Indo-Iranian, according to Drinka, constitutes evidence that the 'Out of India' theory, put forward by Kazanas in this volume, is untenable. However, both Kazanas and Drinka concord in recognizing the validity of IE not only as a linguistic construct, but also as the expression of a real, pre-historical speech community.

**Chapter VIII.** In his contribution: *The origin and spread of the Indo-Germanic people*, Alexander Häusler states that IE has only a linguistic meaning, and therefore it is methodologically wrong to identify IE or the Indo-Europeans with archaeological cultures, or anthropological types or races. The Author backs his claim through the results of recent archaeological and anthropological research. As a matter of fact, Häusler argues, there is clearly no archaeological or anthropological evidence in favour either of the popular thesis of the existence of a restricted, ancient *Urheimat* or of an *Urkultur* of the Indo-Europeans – see however the chapter by Kazanas, who has a different stand regarding these issues. Similarly, there is

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no extra-linguistic evidence in favour of the mainstream, migrationist-type of theories, according to which the Indo-Europeans would have dispersed from their *Urheimat* located in the area of the South Russian or Pontic Steppe. On the contrary, at least with regard to the region extending between the North Sea and the Caspian Sea, there is only evidence for a continual, progressive development of the old-established population and culture – undisturbed by migration and conquest – since the Mesolithic / Neolithic times. On the other hand, the Author observes, the so-called ‘diffusionist’ theses, according to which the old IE population would have diffused peacefully and slowly, due to their attained economical advantages, do not appear to contradict the archaeological evidence. Even so, the (assumed) IE linguistic community is not at all identifiable by the means of archaeological or anthropological research, although it must have existed, since the corresponding linguistic classification has been fairly safely established. This chapter interacts mainly with the chapter by Bryant, Kazanas and Drinka, all dealing (also) with the issue of the IE home land(s) and related migrations, and all holding (more or less) different views.

**Chapter IX.** Nicholas Kazanas, in his: *Indo-European linguistics and Indo-Aryan indigenism*, argues in favor of the indigenist, ‘Out-of-India’ hypothesis – as against the Aryan Invasion / Immigration Theory – although he also holds the possibility that the IE home land might have been a very wide area, stretching from North West India up to the Pontic Steppe. The Author bases his claim on data and argumentations drawn from linguistics (particularly from the lexical, morpho-phonological and morphological structure of Sanskrit), archaeology and genetics, as well as from the system of cultural and ideological values found in the Vedic Hymns. The Indigenist hypothesis supported in this chapter is in contrast with the mainstream doctrine of the European origin of Sanskrit, which is re-proposed and defended in this volume by Drinka and Schmitt. On this issue please compare also the chapter by Bryant, who argues that there is nothing in Indian archaeology “that in any way ...supports a migration of peoples into the Indian subcontinent during even the broadest of time frames associated with the post-PIE dispersions”. Kazanas also claims that Sanskrit represents the oldest language within the family, in this respect holding a view which is line with the traditional model, even if, in Kazanas’ opinion, the Vedic language and civilization is actually much older than typically assumed within the conventional framework of IE studies: Sanskrit is as old as to even predate the Harappan civilization (but see Schmitt in this volume, who argues instead that there is no sufficient evidence to uphold this claim). Thus, Kazanas believes that the IE theory is well founded, and that an IE language family /speech community did exist in pre-historical times, in this being again in line with the mainstream (and realist) approach to IE. However, Kazanas distances himself from the mainstream doctrine when he argues the following: a) PIE cannot be reconstructed, not only because the widely assumed ‘regular and systematic’ sound laws have not really been established, but also because reconstructions are, by their very nature, highly conjectural and non-verifiable; b) most of the conventionally established correspondences are actually ‘similarities’, that is, they do not, strictly speaking, correspond to each others, but are nevertheless ‘similar’ in sound and meaning. In this respect, Kazanas’ overall assessment of reconstructed IE is in line with that of Marcantonio in this volume, although the interpretation of the relevant data and, consequently, the conclusions drawn by the two Authors differ altogether. In fact, Marcantonio interprets the similarities in question mainly as ‘chance resemblances’, which in turn would be an indication that the IE theory is not, after all, as well founded as widely claimed. In contrast, Kazanas interprets these similarities as ‘cognates’, because the possibility of

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chance resemblances or of borrowing is to be excluded given the wide distribution of most of these similarities across Eurasia – but not, significantly, in the Near East. Similarly, according to Kazanas, the well known, extensive similarities of morphological elements and paradigms would be difficult to account for through chance, or borrowing (although there are attested instances of borrowed, wholesale morphological paradigms, as reported in the chapter by Matras).

**Chapter X.** The main thesis of Angela Marcantonio's chapter: *Evidence that most Indo-European lexical reconstructions are artefacts of the linguistic method of analysis*, is that the methods of historical linguistics, including the comparative method, can be so flexible – by their very nature – that they can be stretched to account for almost any data. This means that the explanatory system runs the risk of becoming dangerously circular, and, therefore, of yielding misleading results – in this case within the field of IE studies. Over the course of about two hundred years of everyday practice of reconstruction within IE the encountered counter-evidence has been typically 'explained away' through all sorts of (often ad-hoc) justifications, so that today one can find hardly any evidence counter to the model. The Author examines this 'explaining away' process critically, by asking the question: have we fitted the IE data to the model, or have we made the model so flexible that it can fit almost any data, including potential counter evidence? Marcantonio analyzes the full comparative corpus of the verbal root reconstructions contained in the most recent IE etymological dictionary (LIV), by adopting simple, quantitative methods of analysis. She argues that the great majority of these reconstructed roots lack the required 'statistical significance', the required 'cumulative effect'. As a consequence, the cognates relating to these roots are to be interpreted as 'similarities', rather than 'correspondences'; in turn, these similarities can be interpreted as instances of 'chance resemblances'. Marcantonio also argues that adding laryngeal segments to the process of reconstruction (whatever the rights and wrongs of the laryngeal theory may be), dangerously increases the explanatory power of the comparative method, in this way further contributing to the flexibility of the overall explanatory model. This chapter interacts mainly with the chapter by Andersen and Kazanas. For example, Kazanas objects to Marcantonio that the scientific method is not applicable to this field of studies – actually, the proper 'scientific' approach would be to ignore rigidity, regularity and uniformity, since the IE linguistic and cultural changes occurred in diverse ways and certainly under no observable laws. Similarly, Andersen believes that scientific proves cannot really be delivered in a field such as this, where scholars cannot observe the objects of investigation directly. Also Drinka and Di Giovine (in this volume) defend the validity of the IE theory, despite their criticism of the one or the other traditional IE tenets. Finally, the reader may want to compare the analysis of the present *vs* perfect *Ablaut* offered in this chapter as against the one proposed by Carruba in his contribution. Both Carruba and Marcantonio point out the scanty and irregular distribution of the vowel alternations across the IE area (with the exception of Old Indian and, partially, Greek). However, Carruba interprets this fact as the fading away through time of a very archaic, IE phenomenon, whilst Marcantonio emphasizes the lack of 'statistical significance' of the phenomenon itself and interprets it as linguistic artefact.

**Chapter XI.** In: *Defining the limits of grammatical borrowing*, Yaron Matras examines two 'mixed' languages – Romani and Domari – whose populations are known to have come into contact with several other populations and languages in historical times, and it is therefore possible to trace the borrowing that

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has occurred. These dialects show indeed extensive borrowing, both at the lexical and grammatical level. Close examination of the borrowed grammatical elements shows that some categories are more likely to be borrowed: discourse markers, connectors, focus particles, indefinite expressions, indefinite markers, and expressions of modality; whilst other categories tend to be borrowed less easily, such as: nominal derivation, plural formation, prepositions, lower numerals, negation and interrogatives. Despite these patterns, no areas or categories of lexicon or of grammar appear to be completely immune from borrowing. Matras presents evidence that case and verbal inflections tend to be borrowed wholesale, if they are borrowed at all. For example, the Greek-derived tense markers in Romani are characterized by a “wholesale import of the entire paradigm, rather than of individual markers, and, in all cases, the imported verb paradigm exists in complementary distribution alongside the inherited inflection paradigm”. This evidence illuminates one of the most striking aspects of IE: how does one explain the ‘wholesale correlations’, spanning entire paradigms, which are observed across some IE languages? For example, textbooks frequently cite tables illustrating the verb ‘to carry, to bear’, which shows amazing correspondences across Sanskrit, Greek, Latin, Gothic, O. Irish, etc., in most forms of the declension of the present tense, as well as in the lexical forms (compare Sanskrit. *bhārāmi*, Latin *ferō*, Greek *φέρω*, Gothic *baira*, O Irish *\*berū* ‘I carry’, etc.). The verb ‘to be’ also shows similar – lexical and morphological – correlations across the entire paradigm. There has been an underlying assumption, upon which the IE theory was founded, that these patterns cannot be borrowed and therefore they must be inherited. For example, Nichols (1996a) says that these patterns are “individual-identifying evidence”, that is, evidence “obvious to the naked eye of a trained scholar” that the languages are genetically related (this interpretation is supported in this volume by Kazanas). However, there is a minority view, in which Authors question this assumption. For example, Greenberg (2005) and Croft (2005: XIX) remark that these correlations are too good to be the result of inheritance, because of the language change that would have occurred in the millennia involved. Matras’ study provides attested, concrete evidence that the borrowing of entire verbal forms – lexical item + paradigm – does indeed occur. This suggests that the familiar examples of verbal paradigms cited in textbooks could equally well be accounted for through borrowing, and this, in turn, calls into question one of the main, traditional tenets of the IE theory.

**Chapter XII.** Rüdiger Schmitt, in his: *Iranian archaisms vs. Vedic innovations - and the Indo-Iranian unity*, argues that with regard to the comparative study of the IE languages altogether and to the reconstruction of the IE proto-language it is not sufficient and adequate to refer to the (Old) Indo-Aryan data alone. The wealth and diversity of the textual evidence written in younger Iranian languages as well as the archaism particularly of the Old Avestan language make it only too clear that despite the old age of the earliest Vedic texts the Old Indo-Aryan language is not the only fundament of the IE proto-language, that its data are not with absolute necessity genuinely antique and therefore Old Indo-Aryan (both Vedic and Sanskrit) is not as close to PIE as several people still think. Rather, scholars should take into account the “(proto)-Indo-Iranian linguistic unity”, which is indeed “defined by a large number of phonological, morphological, syntactical and phraseological isoglosses found exclusively in both these language families, including the speakers’ self-designation as *Aryan*”. The Author puts forward a long list of data and argumentations (mainly from the level of phonology), which would show how the Iranian branch often reveals more archaic traits than the Indo-Aryan one. In particular, Schmitt sets himself the task of

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drawing the list of those Iranian archaisms “that are faced with secondary changes, and are therefore obvious innovations, on the Vedic side”. This list, it seems, has never been drawn thus far, despite its obvious relevance for the issues under discussion. Schmitt’s findings are at odds with one of the major, traditional tenets of IE studies, that is, that Vedic Sanskrit is the oldest language within the family, and therefore the most important for the process of reconstruction. This tenet is supported in this volume by Kazanas and, to a certain extent, by Di Giovine – the reader might want to compare also Drinka’s chapter regarding the status of the Indo-Iranian branch. In particular, Kazanas draws a list of data and features which, in contrast to Schmitt’s list, would demonstrate how Vedic is actually more archaic than (Old) Iranian, and the most archaic language within IE. Kazanas also points out that Schmitt’s conclusions rely entirely on linguistic data – amenable to a reverse interpretation – and ignore other aspects relevant to the debate, such as evidence drawn from mythology, archaeology and genetics. The claimed archaism of Old Iranian also gives Schmitt the opportunity to consider as correct the mainstream doctrine (re-affirmed in this volume by Drinka and Annamalai & Steever), according to which the Indo-Aryan language must have immigrated into India, rather than having originated there. For an alternative point of view, please compare again the conclusions reached by Kazanas, who, whilst not denying the existence of an Indo-Iranian linguistic unity, argues that this unity must have formed and existed for some time in the Saptasindhu area, whence later on the Iranians left.

## 6. Conclusion

The reader has seen in this book a variety of views about IE, ranging from the belief that it represents the language of a real pre-historical community; through the thesis that it is only a model to embody linguistic correlations; all the way to statistical evidence that (many) linguistic correlations themselves may be merely an artefact of the method of analysis. In fact, when the various components of the theory are brought together so that they can be seen holistically, it is hard to pin down what the foundations of the theory are actually supposed to be. For example, one of the founding principles of the traditional version of the theory was the assumption that morphological paradigms cannot be borrowed, and therefore it is possible to trace genetic inheritance through them. However, we have seen evidence of wholesale paradigm borrowing, based on studies of languages in contact. In any case, some scholars now hold that morphology is less relevant than other factors – but it is at present unclear whether, or how, these other factors may be verified or falsified. It has been the purpose of this book to bring to the fore these contradictions and open questions associated with the theory. It is for the reader and the linguistic community to decide the way forward.

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