

The historical grammar of Greek: A case study in the results of comparative linguistics

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1. Introduction

1.1 In order to evaluate the methods and principles of comparative Indo-European philology, I wanted to discuss a well-circumscribed corpus of facts. I thought, of course, of an Indo-European handbook, but of these there are in fact only two: Meillet's *Introduction* (1937⁸), and Szemerényi's *Vergleichende Sprachwissenschaft* (1980²). The first is at present too dated to evaluate the present status of Indo-European linguistics. Szemerényi's book, on the other hand, is too personal for this purpose. There is too much in it with which I completely disagree, and in many respects his book is not representative of the opinions of scholars in this field.

The next book that came to my mind is Rix's *Historische Grammatik des Griechischen* (1976), and it is this book that I have chosen for discussion. There are several reasons why I consider this to be a good choice. In the first place, Rix is a reputed scholar, in the center of present-day Indo-European linguistics — though Rix devoted much of his time to Etruscan and is one of the leading figures in this field, too. His book is recent, at least in our field: edited in 1976, it dates actually from 1975. Of course this is already more than ten years ago, and there has been rapid progress in our field since then; nevertheless, it is sufficiently up-to-date; it incorporates the newest insights of its time, and there is no other book of this kind which is more recent.

An advantage is also that it covers a whole field, not just a section, though, of course, Greek is just a section of Indo-European as a whole. Further, Greek is one of the best-studied languages, which means that we do not have the excuse that we did not have enough time to work things out. To colleagues in non-Indo-European languages I sometimes say: Just work on it for another hundred years or so, as we did with Indo-European. Also, except for work on the historical interpretation, Greek, though a dead language, is

either before Rix (which means that it is in his book), or after his book appeared. Remarks concern phonology and morphology; syntax is not treated in the book. Etymology appears only insofar as phonology is affected. Thus, the enormous amount of substratum words in Greek is not of interest here. (The most extensive treatment is that of Furnée 1972.)

1.3 Let me make a statement in advance about the results of my — necessarily short — inquiry. I give some figures first. I made, for the present purpose, some 120 notes. As there is some repetition in them, some 100 remarks or objections could be made. I present some sixty of them in this paper. Part of them also relates to Proto-Indo-European rather than to Greek. This may seem a large number, but it is not if one considers that they concern the whole of the historical interpretation of a language with a rather complicated morphology. I could make it worse by saying that in the whole book — which I use for courses — I have an average of three remarks on every page, and the book has 266 pages; if we say 250 pages, this gives some 750 remarks. But there is a certain amount of repetition.

Nevertheless I venture to say that the historical interpretation of Greek is nearly complete, and that hardly any major questions remain to be solved. It may be of some importance to note that the “methods” developed for Indo-European linguistics are sufficient and have led to this brilliant result.

The same can certainly not be said of most other Indo-European languages. Why is it that in the case of Greek the result is so remarkable? I think that a number of reasons can be given. First, as we have already noted, the study of Greek has been very intensive. A second point is that it so happens that Greek is very close to Sanskrit — I deliberately use a vague term — which means that Greek and Sanskrit, though very different languages, can be compared well. A third point is that the distance from Greek to Proto-Indo-European is not too great. If we compare, for example, the situation of Latin — which is not attested very much later than Greek — we see notably that the verbal system has been totally reorganized, which makes both comparison and historical explanation much more difficult. In the case of Latin we are confronted with a system radically different from that reconstructed for Proto-Indo-European; for Greek, this is true, if at all, only to a limited extent. It is the kind of “jump” of which the Germanic weak preterite

is the best-known example: we find a system of which no intermediate stages can be determined, assuming that they existed at all; such a “jump” very rarely occurs in Greek. The only comparable case in Greek is to my mind the passive aorist with *-thē-* to which we shall return below. A fourth point one might mention is typical for most Indo-European languages: I have the impression that, contrary to what is found in most other languages of the world, there is no influence upon Greek from other Indo-European languages; there is some mutual influence among dialects, but that too is very limited. A fifth point which I would mention is that sound developments in the history of Greek are not very complicated. If we again compare Latin, I have the impression that sound developments happen to be much more complicated there. There is simply much more that we do not know in Latin. Also, Greek does not pose such problems as the “Auslautgesetze”, or syncope, or relative chronology, which can make the prehistory of some languages so complicated.

I do not mean to say that there remains nothing to be done in Greek. Of course very many problems remain, and the phonology provides its difficulties too, like the nasty problem of the vowel contractions. But in my estimation we do not have fundamental problems. The conclusion for the moment can be that the classical principles of historical change are quite adequate.

We shall now proceed to look at the more important questions which I noted. One remark I would still like to make in advance. If there are problems, a major point is whether they concern conflicting evidence, or rather the absence of any relevant evidence. In the case of the passive suffix *-thē-*, for example, I would rather speak of lack of evidence. We simply have no good parallel with which to compare it; it just hangs in the air. Conflicting evidence is, e. g., the development of word initial **sm-*, which normally loses its *s-*, but sometimes retains it. (We shall return to this later.)

2. Phonology

Page numbers refer to Rix (1976).

2.1 p. 29. Regarding Proto-Indo-European, I do not accept the existence of *a*, *ā*, *ī* and *ū*. As a consequence, I do not accept the

diphthongs *ai*, *au*, *āi*, *āu*. (Note on p. 51: wordfinal “-*āu* kommt nicht vor.”) Thus for the suffix *-īno-* (p. 50) I assume **-iHno-*. (However, I am not sure whether Rix writes *ī*, etc., as a shorthand for *iH*, etc.) Here it may be added that the word for ‘goose’, and the antecedent of Greek *apó* are not in my view “Elementarwörter”. The first word could be a loan, but that is a different matter. For ‘from’ I reconstruct **h₂epo/h₂po*.

p. 34. The relation between zero grade and *o*-grade in unstressed syllables, which I explained in Beekes (1985: 156 ff.), need not be discussed here, as it is a Proto-Indo-European question rather than a Greek one.

p. 38. I do not understand Rix’s remark that a possible distinction between a vocalic and a consonantal laryngeal “allein in dieser Umgebung [i. e., between consonants] ist nicht zu rechtfertigen”. For if there was such a distinction, which means that there were (three times) two phonemes, it could be phonemic only in this position. (There is no apparent evidence for this distinction. Compare Beekes (1988 a).)

p. 39. If Lat. *ambāgēs* has Dehnstufe of *e* as Rix assumes, this provides an argument against the assumption that long *e* was affected by adjacent *h₂*.

2.2 p. 48. Turning to Greek, I note as a problem the fact that some final diphthongs count as short and others as long in Greek. Rix’s view is relevant for his interpretation of the third person singular thematic ending *-ei*, as it could not continue old *-ei* in his view. We come to this later. I have no firm convictions on this matter. Kortlandt has suggested to me that we should distinguish between, e. g., *-e_i* and *-e_i*, the latter with vocalic *-i* resulting from a more recent “contraction”.

p. 51. Rix states that **-ōi* remains as such. Here it might be repeated that final long diphthongs might already have been shortened in Proto-Indo-European (p. 30, and p. 146 on *ēkhó*). To my mind this question requires a new investigation.

p. 54 presents a difficulty in that Rix states that different contiguous vowels after the loss of *u* were not contracted in inflectional paradigms, but that they were contracted elsewhere. Thus we have *hēdeī hēdeīs* beside *hēdéos hēdéa*, but *āthlon* from *áethlon*. It is evident that in the paradigms the (hiatus and the) two vowels were restored, but it is hard to see on what basis, and why the restoration occurred only between nonidentical vowels. We must assume, I

think, that the hiatus was once restored everywhere in the paradigm, after which contraction occurred, but of identical vowels first. However this may have happened, it must be a matter of normal analogy.

p. 58. To my surprise Rix allows for the possibility that before *r*-Greek might have known real prothesis. Rix accepts in principle the explanation of such vowels provided by the laryngeal theory, which assumes that these vowels resulted from the vocalization of an initial laryngeal which was lost in all other languages (except Armenian, Phrygian and Macedonian; on Armenian see now Kortlandt, in press; Beekes, in press). He therefore writes the laryngeal between brackets on p. 84. Of course it is possible that there was another source of "prothetic vowels", but until now it has not been shown that it is necessary to assume a second phenomenon, and thus we should not accept one. Here he mentions the word for 'fish', *ikhthús*, which is normally assumed to have real prothesis; as, however, *khthón* does not have prothesis, I suggest that 'fish' had an initial laryngeal, i. e., **h₁dǵ^huH-*. That we find *i-* instead of *e-* may be due to the double stop following.

p. 60. A well-known problem is the development of initial **ǵ-*, which seems to give either *ζ-* (e. g. *zugón*) or *h-* (*hēpar*). Several attempts have been made to explain the distribution. One is that *ζ-*, which was probably [zd], originated from a group *Hǵ-* (Rix considers this the simplest solution, and I think he is right), but it has not been definitely proven. (It should be noted that if a laryngeal is demonstrated this does not yet prove that it is the cause of the different development. We would have to demonstrate that all cases with *ζ-* had a laryngeal and that all cases with *h-* did not have a laryngeal.) Another idea is that the words with *ζ-* are loanwords from another language (cf. Ruijgh 1967: 66). This seems improbable for *ζεόγνυμι*, *ζέω*, *ζώννυμι*. Of course, the "Ausnahmslosigkeit" is at stake. However, I am convinced that the problem is just that our evidence does not allow us to decide what happened.

p. 61. The development of *rǵ*, *nǵ* seems to depend on the preceding vowel: after *a* and *o* the *ǵ* is lost with epenthesis of *i* (e. g. *baínō* < **g^wmǵō*), but after *e*, *i*, *u* we get compensatory lengthening (as in Arc. *phthērō* < **phtherǵō*). Rix supposes that *ǵ* first became *h* in all cases. I think that this is improbable (also because this seems impossible for *lǵ*, which became *ll* in all dialects, e. g., *állos*). In my view we have anticipation (umlaut) of the *ǵ* in all cases, which

resulted in lengthening of *e*, *i* and *u*. This problem concerns the phonetic explanation of observed facts.

p. 62. The development, only in Attic, of initial *u* to *h*- is also problematic. The condition seems to be that the vowel after the *u* is followed by *s*. This explanation does not work everywhere, but it seems to me more essential that this condition is phonetically hard to understand.

p. 64. The development of *iū* is also problematic. It results in forms written with intervocalic *ι* (e. g. *ἐλαιον*), but Ionic-Attic shows different reflexes. Here we find no *ι* (*πόᾱ*, against Dor. *ποῖᾱ*) except before *o* (*ἐλαιον*), while a preceding *a* was lengthened (*εῖᾱᾱ*, *ᾱεῖ*). Rix's formulation is different, and difficult. He seems to assume that the lengthening of *a* was general in Greek; thus he suggests — if I understand him correctly — that *αἰεῖ* in Homer must be read *ᾱῖεῖ*, whereas the normal interpretation is that it means *αιῖεῖ*. I doubt whether this solves anything. It remains strange that the lengthening occurs only in the case of *a*, and also that *ii* is retained (only) before *o*. Lejeune (1972: 247) ends with: "Aucune explication satisfaisante n'a été jusqu'ici donnée de ces faits." The problem concerns the phonetic probability of developments.

p. 66. I note the strange terminology where Rix says that vocalic nasal before syllabic or "consonantic vowel" became *an*, *am*. Probably he means semivowel, i. e., *j*, *u*.

p. 69. The laryngeals. Rix accepts for *HuC*- both the development to *VuC*- and to *uC*-, the latter with hesitation. This was the subject of Peters' study (1980). I consider the latter development to be the correct one; see my review (Beekes 1982).

p. 69. The development of what is usually noted as *H_lC*-, *H_gC*- to *VrC*-, *VnC*- (proposed by Beekes 1969: 132–133) shows that Greek vocalized the laryngeal, not the resonant. We should therefore write *HrC*-, etc., with the phonemic notation of the Proto-Indo-European form. There are more cases where this notation is simpler than the notation with *ɣ*, etc., which blocks a proper understanding. I have noticed that my notation leads to misunderstandings, but clarifying these will take time. It is simply a matter of phonemics against phonetics.

p. 72. Aspiration of a stop by a laryngeal is in my opinion not found in Greek. The only instance that could be adduced is the second person singular perfect ending *-tha*, which is to my mind not enough. Forms like *platús*, where analogical restoration is

improbable, can be adduced to refute the assumption. Voicing by a laryngeal I do not acknowledge at all. Rix's example of *ógdoo*s is in my view not certain enough. The problem here is just that the evidence is very meagre.

p. 72. Rix accepts a development of *R̥H* before consonant to *VRV*, beside well established *R̥V̄*, e. g., *aRa* beside *Rā*. It would have been conditioned by a — secondary — stress. This is to my mind improbable. The forms of the type *aRa* can be explained by a sequence *R̥h₂e*. It has never been demonstrated from the evidence that the accent was responsible. (An evaluation is in preparation.) The idea that *Rh₁* gave *eRe* is improbable if the assumption is correct that *R̥h₁* before vowel resulted in *aRe*. For this would mean:

Crh₁C > *ere* *Crh₁e* > *are*

which is phonetically quite improbable. Rix accepts the latter development (p. 74). I originally assumed that the latter sequence resulted in *ere* (the so-called laryngeal umlaut), but I am now of the opinion that this has to be given up (see Beekes 1988 a). It must be added that I do not see how the aorist type *époron* is to be explained. Rix says only that it must not be based on **prh₃-ent*, but he does not say how it could be otherwise explained. These problems concern the phonetic probability of developments, but particularly the careful weighing of seemingly conflicting evidence.

p. 76. The group **sm-* seems partly retained, partly represented by *m-*, for which no distribution has been found. This is a serious problem inasmuch as no conditioning seems possible. The one conceivable solution is that we have sandhi variants, e. g., that *sm-* following stops was maintained. However, sandhi variants are a kind of ultima ratio: with dead languages the fact can never be demonstrated; and the number of facts for which this interpretation has been accepted is extremely small. I have viewed the material and note that most words with *sm-* are evidently non-Indo-European or have no etymology, so that they might also be loan words. (Several of these words also have forms without *s-*.) Only three words are probably Indo-European: *smerdaléos* 'terrible', *smúkhō* 'smoke' and *(s)mīkrós* 'small'. My suggestion is that that these words retained the *s-* because of their expressive force. There can be no doubt that *m-* is the normal representation.

p. 80. The development of *s̥j* and *j̥s* poses a problem, in that the *j̥j* to which these groups developed in the first place was in some

cases reduced to *j* which then disappeared; however, we do not know when this happened. Rix writes that it is retained before *o*, “sonst meist vereinfacht”. This is again a matter of phonetic probability, and of weighing of the evidence, this time of a great number of forms, where analogy will have played a considerable part.

p. 87. It is remarkable that the voiced and aspirated labiovelars became labials before *i*, whereas the voiceless ones became dentals and all labiovelars were palatalized to dentals before *e*. Stephens – Woodard (1986) pointed out that voice and aspiration tend to inhibit palatalization, which explains the difference for voiceless sounds. But it does not explain – as they admit – that the voiced and aspirated sounds were palatalized before *e* and not before *i*. (That the voiced labiovelars behaved differently has a parallel in Old Irish, where they became *b*, while the voiceless ones became *k*).

p. 89. The development *ti* > *si* is found in Mycenaean, Ionic-Attic, and Arcado-Cyprian (and Lesbian), except in “isolated dissyllables”. Rix does not explain the latter. I think he should at least have pointed out that it is a problem, for the term “isolated” suggests that it contains the explanation, whereas in fact it makes the problem all the greater; in isolated words we expect the regular sound change. It concerns forms like *ἔτι*, *ἀντί* which can be explained from apocopated forms (*ἔτ'*) and from cognate forms where no *i* followed the *t* (*ἄντα*, *ἄντην*); the regular change is seen in *κασίγνητος*, with *κασι-* from *κατι-*. A serious problem lies in the dialects that do not present the development: These show *si* from *ti* in one category only, i. e., the words in *-σις* (type *βάσις*). Rix explains this fact by assuming that the whole phenomenon is in fact based on the development of antevovalic *tj* > *ss* (which is a normal Greek development). Thus **potis* would have had a genitive **potios* > **possos* > **posos* (replaced by *posios*), from which the *s* would have been introduced in the whole paradigm. Apart from the fact that it seems improbable to me that nom. **potis*, acc. **potin* would have been changed by the genitive, etc., this assumption cannot, in my judgment, explain why the Northern dialects generalized the *s* only in the words in *-sis*. It seems inevitable to me to conclude that these words were loans from Southern dialects. Lejeune (1972: 64, note 7) objects to this on the grounds that these words would have been taken over very early from Ionian (I do not know why he mentions only Ionian). At present we can also point to the Mycenaean data (we have *apudosi* /*apu-dosis*/). Lejeune is of the opinion

that we have to reckon with *ti* > *si* in different positions at different times with different extensions. This is probable enough in itself, but it cannot explain the curious fact that only words of one morphological category show the development. Therefore I think we have to return to Schwyzer (1939: 271): “Die dorischen Beispiele für -σις ..., alle in technischen Wörtern (z. B. des Rechts, staatlichen Lebens, des Handels und Wandels), sind Einfluss des Ionischen und Attischen”, to which must now be added “and Mycenaean”.

3. Morphology

3.1 The noun

p. 123 ff. As regards the noun I am at a loss what to do. My remarks are mostly connected with my theory about the ablaut types of Proto-Indo-European, published in Beekes (1985). This theory is rather far-reaching, and I cannot propound it here in any detail. It is not useful to compare it with Rix's conceptions, because it is simply a new complex of ideas. I shall limit myself to a short indication of my view, a few remarks about the methodology, and shall make a few comments on Rix's ideas which are not directly connected with my theory. It should also be noted that my theory concerns Proto-Indo-European, and in particular its prehistory, rather than Greek, though on a number of points it is directly relevant.

Rix's presentation of his view in an introductory handbook, without a full-scale exposition, poses a difficulty. In many instances I do not know why Rix has chosen a particular interpretation.

pp. 145, 150, and 167. Several types are classed by Rix in a way that differs from my classification (and from the usual one). Thus he considers *poimén* as proterodynamic, *téktōn* as static (145); he assumes that nominative endings in *-ōR* could be proterodynamic or static (150); and he takes the comparative inflection of the suffix *-jes-* (nom. *-jōs*) as proterodynamic (167). All forms cited are in my view hysterodynamic.

Let me state in brief outline my view of the Proto-Indo-European nominal inflection. In diagram it looks as follows:

| | STATIC | MOBILE | |
|------|--------------------|----------------------------|---|
| | (static) | proterodynamic (neuter) | hysterodynamic (masculine/feminine) |
| nom. | <i>CéC-R</i> | <i>CéC-R</i> | <i>CéC-R</i> , <i>CC-éR</i> , <i>CéC-ōR</i> |
| acc. | (<i>CéC-R-m</i>) | " | <i>CC-éR-m</i> |
| gen. | <i>CéC-R-s</i> | <i>CC-éR-s</i> | <i>CC-R-ós</i> → <i>o</i> -stems |

So there was one static type and there were two mobile types, for which I retain the terms protero- and hysterodynamic (I could also have called it "mobile", which is an easier term, but it seemed useful to retain the old terms).

Both mobile types had in the nominative (stressed; in the earliest phase only the stressed syllable has *e* or *o*) full grade in the root, zero grade in the suffix. The hysterodynamic type had an accusative (originally a directive). On the basis of this accusative which had (stressed) full grade of the suffix, the nominative also usually had full grade in the suffix (*CC-eR*). If the stress remained on the root, this suffix had *o*-vocalism (*CéC-oR*). Then *-eR*, *-oR* became *-ēR*, *-ōR*. The nominative, originally identical in both types, was in fact an absolutive case (which explains why the neuter and masculine-feminine 'nominatives' were identical); the genitive (in *-ós*) served as an ergative. From this ergative originated the *o*-stems (nom. *-os*).

What is new from a methodological point of view? The essential step was that I wanted to reconstruct a phase in which the ablaut rule that only the stressed syllable could have *e* (or *o*) was strictly observed. Therefore the nominative in *-ēR*, *-ōR* provided a problem. I tried out what would happen if the nominative was *CéC-R*, following a suggestion from my colleague Kortlandt. After that, everything followed almost automatically, everything fitting in. Important confirmation is provided by the fact that several old theories, until now more or less "dormant", proved to be right and fell into place: the ergative theory; the identity of the nom. *-os* with the gen. *-os*; and the fact that the genitive of the *o*-stems seemed to be *-os*, which was the cause of the difficulties with this ending.

p. 130. Thus the nominative singular of the *ā*-stems was in my view single *-h₂*. This is confirmed by the neuter plural ending, Gr. *-a*, Skt. *-ī*, which can only have been a single laryngeal, not *-eh₂*; that these two endings were identical (the form could indicate collectives; this explains why neuter plurals were constructed with the verb in the singular) was recognized long ago.

p. 132. Thus the genitive singular is given by Rix as *-eh₂-s*, whereas I consider it to have been *-h₂-os*. Rix says that the form cannot have been *-eh₂-es*, because this form would have two full vowels (*e*). It is strange to see that on the same page he assumes for the dative *-eh₂-ei*, which does have these two full vowels. Now it is not remarkable that in a handbook that discusses so many problems there are a few inconsistencies like this, but the objection against the two full grades turns up repeatedly. This is, of course, a matter of relative chronology: there was probably a time when an Indo-European word could have only one stressed vowel (*e* or *o*). However it is also quite clear that Proto-Indo-European, that is, by definition, the latest stage of the common language, could have words with two and more full vowels in a word; nobody objects to **h₁ebh₂érete*, Gr. *ephérete* 'you (pl.) were carrying'. Between these two stages there was one in which an unstressed syllable could have a vowel, but only *o* (as we saw above). In Beekes (1985: 156 ff.) I reconstructed three stages for the prehistory of Indo-European:

| | | | |
|--------------|-----|-------------|--|
| accent stage | I | é, ó — ø | zero stage |
| | II | é, ó — o | o-stage (o possible in unstressed syllables) |
| | III | é, ó — e, o | e-stage (e is also possible) |

Thus the objection against two *e*-vowels is valid in stage I and II, but not in stage III.

p. 136. The *o/e*-stems were in my view *o*-stems only, for they originated from the genitive (= ergative) ending *-os*, which was interpreted as stem in *-o* + ending *-s*. In Beekes (1985) I was not yet aware of the fact that Rix too considers the forms that seem to have a stem in *-e* (with the exception of the vocative; see Beekes 1985: 99 ff., 191) to be — probably — pronominal in origin.

p. 139. Greek has evidence for a genitive ending of the *o*-stems in **-osio* and in **-oso*. Rix is of the opinion that **-osio* was the ending of the demonstrative pronouns and **-oso* that of the interrogative pronoun. This idea is based on the fact that the interrogative presents evidence for **-eso* (e. g., Gr. *téo, teũ, toĩ*, OCS *česo*). However, there is no specific evidence that the demonstratives had **-osio*. I assume that this is based on the fact that Sanskrit has *-asya*, but that is not conclusive evidence, as in Sanskrit there is no ending other than **-asya*. In the next line, Rix states that in Greek the noun probably had **-osio*, which was replaced by *-oo* < **-oso*

from the pronoun. This would mean that the noun first took over *-osio from the demonstrative (which would give a stage in which both the noun and the demonstrative pronouns had *-osio), and that this ending was replaced by that of the interrogative. Such a dominating influence of the interrogative pronoun is quite improbable. Therefore I maintain that *-so (i. e., both *-oso and *-eso) was the ending of the pronouns (for which there is further evidence in Germanic; Beekes 1988 b), and that *-osio was the Proto-Indo-European ending of the noun. I agree (e. g., with Rix) that the oldest ending of the noun was *-os, but this must have been replaced already in Proto-Indo-European by *-osio, since 1) several languages have this ending, and 2) there is no place from which it could have been taken. It may be added that we find the ending *-so in the masculine \bar{a} -stems, which Rix explains (p. 132) as “den Endungsrest -o von -io < -sio”. This is to my mind improbable.

p. 154. Rix mentions that the dative ending of the consonant stems in Mycenaean was /-ei/, with -i only in the *s*-stems, while classical Greek has only -i. Rix does not point out that this poses a problem: how is it possible that the -i, which is so rare in Mycenaean, came to be the only ending later? I suggested (Beekes 1985: 117 ff.) that in Mycenaean -ei was the ending of the hysterodynamic nouns and -i that of the proterodynamic nouns, which is what we expect on purely theoretical grounds: hystero-dynamic CC-*R-éi* against protero-dynamic CC-*ér-i*. The methodological point is that earlier stages of a language may show phenomena that are hard to reconcile with what we find in later stages. Often the conclusion must be that the two stages do not represent the same dialect. This is true of Mycenaean as compared with later Greek dialects, but it does not seem to be the essential point here.

p. 165. In passing on to other questions I note that the pattern of Greek *píōn* ‘fat’, fem. *píēra* agrees exactly with Sanskrit *pívā*, fem. *pívarī*, so that we can reconstruct PIE **piHuōn*, **piHuerih₂*, but that, as far as I know, the derivation of this feminine has never been explained within Proto-Indo-European. We would expect in any case -*en-ih₂*. One might think of deriving the feminine not from the masculine adjective but from the (neuter) noun in -*r*, but then — by mere coincidence — the difficulty remains exactly the same, as neuters in -*r* have -*en-* in the oblique cases. Even so, this may be the solution, and it may show that the oblique cases in -*en* were

only later associated (into one paradigm) with the nominative and accusative in *-r*.

p. 168. Greek has a comparative ending in *-ōn*, of which the long *-i-* presents a difficulty. It is suggested that it goes back to **-ison-* > **-ijon-*. Rix explains the length as follows: an *i̇* was realized as [*i̇i̇*] after a long “element”, a phenomenon known as Sievers’ Law; and there was another suffix, *-jos-*, with a Sievers’ variant *-ijos-*. Rix proposes the following analogy:

$$\begin{aligned} \text{-}i\bar{o}s\text{-} : \text{-}i\bar{j}o\text{-} &= i\bar{j}o\text{-} : x \\ x &= \text{-}i\bar{j}o\text{-}. \end{aligned}$$

The rules of analogy are of course among the most controversial points in historical linguistics. To my mind this analogy does not work. I cannot explain what is wrong with it; I can only say that this seems to me an ad hoc construction that does not work.

I know of no alternative. I considered the possibility that the genitive must have had **-isnos* > **-īnos*; if now in this form the full grade was introduced, we might have got **-īonos*, while the accusative had **-iona* < **-is-on-m*. From there the long *ī* might have been generalized in Attic. However, I do not think that this is the right solution, as I expect that the full grade was generalized much earlier, giving **-isonos* or **-ihonos*, with short *i*. A connection with the forms of the type **sophóteros* (cf. *kakítonos*) is not probable either, as this development was general in Greek, and limited to a preceding short syllable.

p. 177. The pronouns. Regarding the personal pronouns, Rix makes a remark which I find of some methodological interest. He says: “Die isolierten und heterogenen Formen waren analogischen Umbildungen besonders ausgesetzt, so daß schon für die späte Grundsprache mit Allomorphen zu rechnen ist.” We find this kind of remark fairly often when the situation is difficult. Now it is acceptable to say that the situation is complicated and unclear, and it is true that the paradigm of the personal pronoun was isolated and therefore prone to change, but it is quite another thing to add that it may have been in a state of confusion in the proto-language. I also have the impression that if one adds “in the latest stage of the proto-language”, scholars are led to think that here confusion may be tolerated because it is only the latest stage. This is, of course, not correct, and we should avoid such remarks, as they might give the impression that we are facing an abnormal situation or that we

must not try to reconstruct in the normal way. Even the latest stage of a proto-language was a natural language for which we have to reconstruct a normal set of forms. Moreover, the term “the latest stage” is misleading in that it suggests that we are dealing with a different stage of the proto-language, while in fact we must always reconstruct the latest stage of the proto-language; so there is nothing strange about this stage.

p. 177. The acc. sg. Gr. *emé* derives from **h₁mé* (Beekes in press). The reasoning is that comparative evidence (Arm. *im*, Hitt. *ammuk*) leads to this form, and that such a reconstruction has priority over an analogical explanation (*e-* from Gr. *egō*).

p. 177. For the genitive there were not three different formations in Greek (*eme-so*, *eme-os*, *eme-then*), as this suggests that there was no Proto-Greek form. All forms can be easily explained from **h₁me-so*, with the normal pronominal genitive ending *-so* (Beekes 1986).

p. 186. For Gr. *min*, Dor. *nin*, the enclitic form for ‘him’, Rix assumes a reduplicated form **imim*. I think that this is improbable, since this type of word is not reduplicated, as opposed to indefinites and, less often, demonstratives. I prefer a different explanation (1983: 229 ff.), viz., that the *-m* was taken from a preceding form (in Doric after final *-m* had become *-n*) — Old Prussian *dim* originated in the same way. My reason is that the explanation given is improbable because it assumes an improbable word formation. The explanation I have given here is probable because a parallel form in another language can be explained in a parallel way.

p. 187. Rix reconstructs for the interrogative-indefinite pronoun nom. **k^wis*, neuter **k^wid*, with a gen. masc. **k^wo-so*, neuter **k^we-so*. This is based on a false interpretation of Avestan forms (the difference there is between indefinite and interrogative, not between masculine and neuter). I think Rix’s remark on the next page (p. 188) is correct in saying that the *o*-stem is that of the adjective, the *e*-stem that of the independent forms. It is true that Slavic has the distinction which Rix assumes for the proto-language OCS masc. *kogo*, neuter *česo*, but here all masculine forms are formed from the *o*-stem (nom. *kъ-to* from **k^wos*), while the neuter retained the old *i/e*-stem. Rix’s reconstruction of a stem variation *i/o* beside *i/e* is daring; such new approaches must be undertaken, albeit with the utmost care, and only if there is either direct evidence or other confirmatory evidence of whatever kind. I consider both to be absent in this case.

3.2 The verb

p. 201. The type *philēō* is always explained as containing the *e*-form of the *o*-stem *phílos*. As the *o*-stems did not, in my view, have an *e*-stem variant, this explanation cannot be correct. It must contain the suffix *-ei-*.

p. 205. For Old Irish *t-ánaicc* Rix reconstructs **h₁o-h₁nonk̑-*, with a reduplication vowel *-o-*. I do not consider this to be permissible, as there is no evidence for it. I have tried to demonstrate that we must distinguish a root **h₂nek̑-* ‘attain, reach’ from **h₁nek̑-* ‘to carry’ (Beekes 1979: 18).

p. 206. Rix states that the distinctive set of thematic endings, as opposed to the athematic endings, is a Greek innovation. This is fundamentally wrong. We return to the endings below.

p. 208. Rix suggests that the presents that had a reduplication vowel *e* might have had *o* in the root: **dé-doh₃-ti*. This is an interesting suggestion, but I see no evidence for it.

p. 214. Rix adopts the idea that the first and second person plural endings of the aorist had full grade *e*. I agree with Bammesberger (1982) that the root originally had the same ablaut as the present, i. e., full grade *e* in the singular and zero in the plural. Note that on p. 215 Rix admits this ablaut for the Greek *k*-aorist, and for some other aorists without *k*. As these are in origin evidently the same type, they must have had the same ablaut. Rix’s idea is based on some Sanskrit forms. In my view it is a mistake to assume that everything in Sanskrit must be old.

p. 215. I agree with Rix that the origin of the *k*-aorist (*éthēka*, pl. *éthēmen*) is not quite clear, though it seems evident that the *k* is identical with that of Lat. *faciō*, Phrygian *addaket*, etc.

p. 215. Rix assumes, with most others, that the thematic aorist arose from thematization of the root aorist. This seems improbable as the few forms that occur in more than one language, like **uid-e/o-*, belong to the most essential part of the vocabulary. An alternative view is given by Kortlandt (1983). The question of course is whether a formation which is rare is recent or archaic.

p. 219. One of the well-known problems of Greek is the origin of the suffix of the passive aorist *-thē-*. Rix gives two explanations. One is that it originated from the second person singular middle ending **-th₂ēs*, Skt. *-thās*. I doubt this for several reasons. One is that *h₂ē* in my opinion did not result in *ē*, but in *ā*. The second is

that I am not sure that the reconstruction of this form is correct — as Rix admits. The third is that I think it extremely improbable that a second person singular ending formed the basis for a new category. Rix's second explanation is that it contains the root **d^heh₁-* 'to put'. However the intransitive, later passive, meaning cannot be explained in this way. It is most probable that it is the *ē* of the *ē*-aorist (which is intransitive) + the suffix *-t^h-* seen in, e. g., *éskhethon*, but I admit that the missing links cannot be demonstrated.

p. 222. The *k*-perfect must have its *-k-* from the same source as the *k*-aorist. But I doubt whether we have to assume direct influence of the aorist on its formation.

p. 257. The origin of the pluperfect, qualified as "unklar" by Rix, has been brilliantly explained by Berg (1977). It proved to be a Greek innovation, as was generally supposed, and, as always, its explanation is simple, though it contains a series of successive steps.

p. 231. Rix assumes that the optative of the root aorist had static ablaut (*e*-grade of the root throughout). In my estimation this is wrong. Note that the indicative of this aorist would also have an aberrant ablaut (p. 214). Rix does not explain the third person plural ending *-ent* (p. 232), which is not oldt in his types. Kortland, (1987: 221) assumes a special ablaut for the third person plural ending, of the type *CeC-ih₁-nt*. It is to be noted that the discovery of new types of ablaut, i. e., the static paradigm, provides new possibilities of explanation. The question arises whether different types of ablaut could occur within one stem (here the aorist indicative as against the optative). The answer is yes as regards the subjunctive (with, for example, full grade of the root in the middle, e. g., Skt. subjunctive *bráv-a-te* : indicative *brū-té*). But the subjunctive is a category clearly integrated in the paradigm of the stems only later. For the optative, this is a different matter. If its ablaut were to be different from the indicative, I would suppose that it would have its own type of ablaut throughout, just like the subjunctive. As this is not the case, I think that it is most probable that it agreed in ablaut with the indicative with which it belonged. See the next point.

p. 233. The Aeolic optative of the *s*-aorist, with its interchange of *-ai* and *-ei* (*-s-ai-mi*, *-s-eias*, *-s-eie*, *-s-ai-men*, *-s-ai-te*, *s-eian*) has been the subject of much speculation. We now know that the *s*-aorist optative, like its indicative, had a static inflection, i. e., full grade *e* of

the root throughout its forms (with lengthening of the *e*, originally in monosyllabic forms only, later generalized to the whole indicative [cf. Kortlandt 1987]). This gives forms of the type *CeC-ih₁-nt* (3rd pl.). This form seems actually attested in Cretan *Iusian*. Rix suggests that the problematic *-eian* is just a direct development of [-*ijan*]. I do not consider this to be the solution. My reason is that an isolated form, 3rd pl. *-s-eian*, must probably be explained from a recently discovered type of ablaut, but as an exact parallel is not found and as it does not fit any theoretically postulated form, it remains unexplained. (See now Kortlandt in press.)

p. 233. One more problem concerning the optative. The thematic optative had a stem in *-oih₁-*. This form presents difficulties both before a consonant, where we expect, e. g., 3rd sg. *b^her-oih₁-t* > **phero(i)e(t)* instead of *phéroi*, and before a vowel, where we expect **b^her-oih₁-ent* > **phero(i)en* instead of actual *pheroien* [*pheroijen*]. (Here we must add that the ending *-ent* replaces older *-nt*, and that this would have been the only form with a following vowel.) I have no opinion on these forms.

p. 240. As to the verbal endings of Proto-Indo-European and the explanation of the Greek ones, I differ in several points with Rix's view.

p. 243. The 1st pl. ending *-men* is hardly the ending *-me* with addition of the first person singular ending *-n* < **-m*. The latter is rather improbable. The comparison with Hittite *-wen(i)* seems to prove that the final nasal is old. Kortlandt (1979: 63–64) tried to demonstrate that the *-m* belonged to the thematic endings.

p. 252. The primary ending 2nd pl. *-te* must originate from **-th₁e* because of Skt. *-tha*. The aspirate here is generally neglected, for no good reason. The opposition in Sanskrit of primary *-tha* to secondary *-ta* must be old.

p. 245. The type 3rd pl. *édidon* is explained by Skt. *ábibh₁ran* < **é-b^hi-b^hr-ent*. However, this Sanskrit form is unique, and all forms of this type have the ending *-ur*, which replaces **-at*, as is shown by Avestan. It seems clear that *ábhibh₁ran* stands for older **abhibhrat*, probably to avoid **ábhibhrur*; later a form *abhibharur* was created. Therefore *édidon* may go back to **é-di-dh₃-nt*. But the ending *-ent* may have been introduced early.

p. 246. The ending 1st sg. middle *-mēn* is unexplained according to Rix. He suggests *-eh₂* beside *-h₂e*, which in itself is quite improbable. (If the second person singular had **-teh₂s*, as Rix thinks, it is

quite impossible to understand that a system 1st sg. $-eh_2$, 2nd sg. $-teh_2s$ would not have been retained in both Greek and Sanskrit.) Rix is right when he remarks that the ending cannot be explained from Greek $*-a < *-h_2e$. This implies that this ending, too, is wrongly reconstructed. Kortlandt solved the problem by showing that the ending was simply $*-h_2$ (1981). Greek $-mai$ must be explained from postvocalic $*-mh_2 > *-ma$, while $-mēn$ came from postconsonantal $*-mh_2 > *-mā$. The postvocalic form will have been the thematic form $*o-mh_2$. Why this form became the primary ending, the other the secondary one, still requires explanation. I think that the athematic form was typical for the secondary forms, as it occurred in athematic aorists (including the s -aorist, $-s-mh_2$).

p. 247. For the third person singular middle ending Rix assumes $*-e$, because then it is identical with the perfect ending. It would have become $-o$ after the 3 pl. $-ro$. However, while the middle endings are clearly related to the perfect endings, they are nowhere identical with them, at least as far as we can see. Moreover, there is evidence for o -vocalism in the younger form $*-to$, but there is no direct evidence for $-e$. So $-e$ is a theoretically possible proposal, but direct reconstruction leads to o .

p. 208, 215, 248. Rix assumes a third person plural middle ending $-entoi$, while normally only $-ntoi$ is posited. The matter is complicated by the fact that the ending was really $-ro$; but $-nto$, beside $-ntro$, may have already been formed in Proto-Indo-European. The form $ēprianto$, supposed to be $*h_1e-k^wriH-ento$, was nicely explained by Bammesberger (1984: 47 ff.), who suggested that $*h_1e-k^wriH-nto > *epriato$ was reshaped into $-anto$ as it seemed a third person singular ending. The form $dientai$ is too difficult to be used as reliable evidence.

p. 250. The first person singular thematic ending $-\bar{o}$ is supposed to be $-oH$ or a lengthened $-\bar{o}$. The latter would be the subjunctive ending. His idea (p. 261) that this ending would be originally “ending-less” with “Dehnstufe” (for which there is to my mind no parallel) or with “emphatischer Dehnung des Konj.-Suffixes” is quite arbitrary. Here we can be quite certain: the thematic endings were identical with the subjunctive endings (Beekes 1981), and the ending resulted from $-oH$ because of the Lithuanian acute ($vedũ$). It is unclear whether the laryngeal was h_2 as in the perfect and middle ending (“inhaltlich nicht begründbar”); h_1 as Kortlandt (1979: 68) argues, because of the 2nd sg. $-eh_1i$; or perhaps h_3 , in which case also $-eh_3$ is possible.

p. 251. The 2nd sg. *-eis* and 3rd sg. *-ei* are explained by Rix from **-esi*, *-eti*, through metathesis. Rix has told me that he no longer holds this view. Lithuanian points to 2nd sg. *-eh₁i*, and the 3rd sg. was *-e*, as was shown by Watkins (1969: 164 ff.). When Greek added the primary marker *-i* in the third person singular the form became identical with the second person singular, which then added *-s* from the secondary endings.

p. 256. The second person singular perfect ending *-tha* does not directly represent **-th₂e*; see my comments on Rix (1976: 72) above. It must have arisen after aspirated stop, where (e. g.) **k^hst* > *k^hst^h*.

p. 243. The imperfect forms 2nd sg. *ēstha*, 3rd sg. *ēen*, of which Rix explains the latter as continuing a third person plural, which I consider quite improbable, are explained by Kortlandt (1986: 255) as continuing a real perfect. I may add here that the presentation chosen by Rix has the effect that problems specific to certain inflections (the same holds for the noun) cannot be presented in a coherent discussion, which is a serious drawback.

p. 259. As far as the subjunctive endings are concerned, I have pointed out (Beekes 1981) that we do not have to assume two types of subjunctives, one with primary and one with secondary endings. There was only the set with primary endings. (In Sanskrit the secondary endings are frequent, but they have a distribution which can be explained by assuming that the original system was that of the primary endings.) Greek seems to present forms with secondary endings, so these must be explained. The relevant information is presented in table 1.

Table 1. Secondary endings in Greek

| Indicative Thematic | | Subjunctives in Greek | | | | |
|---------------------------|-------------------|-----------------------|--------------|--------------|---------------------------------------|---------------|
| Proto-Indo-European | Proto-Greek | Primary Thematic | | Athematic | 'Secondary' Thematic | Athematic |
| <i>*-oH</i> | > <i>*-ō</i> | <i>*-ō</i> | <i>-ō</i> | <i>*-ō</i> | <i>*-ō</i> , <i>-ōmi</i> | <i>*-ō</i> |
| <i>*-eh₁i</i> | > <i>*-ei</i> | <i>*-eis</i> | <i>-ēis</i> | <i>*-eis</i> | <i>-ēs</i> , <i>*-ēstha</i> | <i>-es</i> |
| <i>*-e</i> | > <i>*-e</i> | <i>-ei</i> | <i>-ēi</i> | <i>-ei</i> | <i>-ē</i> , <i>*-ēti</i> ¹ | <i>*-e</i> |
| <i>*-omom</i> | (>) <i>*-omen</i> | <i>*-omen</i> | <i>-ōmen</i> | <i>-omen</i> | <i>*-ōmen</i> | <i>*-omen</i> |
| <i>*-eth₁e</i> | > <i>*-ete</i> | <i>*-ete</i> | <i>-ēte</i> | <i>-ete</i> | <i>*-ēte</i> | <i>*-ete</i> |
| <i>*-o</i> | (>) <i>*-onti</i> | <i>-onti</i> | <i>-ōnti</i> | <i>-onti</i> | <i>*-ōnt</i> | <i>*-ont</i> |

¹ If *-ēisi* really stands for **-ēsi*.

The third person plural ending takes different forms according to the dialects. For the sake of simplicity the middle forms are not discussed.

The first column in table 1 gives the Proto-Indo-European forms of the thematic indicative; the second column gives the normal reflexes (or replacements) of these forms in Proto-Greek. I assume that all subjunctive forms derive from the one set of thematic (indicative) forms.

For the Greek forms the third column gives the forms (attested and reconstructed) of the thematic subjunctive forms with short (thematic) vowel and the fourth column gives the normal thematic subjunctive (the only set which is completely documented). We may conclude that the rise of the long vowel subjunctive was a Greek innovation (and that the Indo-Iranian equivalent was an independent innovation).

The fifth column are the well-known athematic subjunctives with short (thematic) vowel, the type *eídomen eídete, paúsomen*. The second person singular is not found; the first person singular cannot be distinguished from other types.

The last two columns give the (few attested) forms with secondary endings. I explain forms as follows: The 3rd sg. *-ē* is simply the old ending *-e* (of column 1) but with lengthened vowel. Thus, this subjunctive ending testifies to the original thematic ending **-e*. If the form in *-ēisi* indeed represents **-ēsi* (that is, if the iota is only a later addition of redactors after the normal ending *-ēi*), it contains the same ending but with added third person singular ending. In fact, if the form was really *-ēsi*, we could not understand why *-si* was added; if, on the other hand, it was *-ē*, the addition of a clear (third person singular) ending is understandable. (Note that Rix considers *-ēsi* as old, but as a primary form, because he thought at the time that the third person singular indicative *-ei* resulted from **-eti*. I consider it to be originally *-ē* with a much later added primary [athematic] ending *-si*). 2nd sg. **-ēstha* will continue older *-ēs*. (After these forms 1st sg. *-ōmi* was formed.) The 2nd sg. *-ēs* was formed on the basis of the 3rd sg. form (as it cannot represent **-eh₁i*). As an athematic form with short vowel, we have only the second person singular, which must also be analogical after 3rd sg. *-e*. Note that, in these last two columns, except for the second and third person singular, only the third person plural would have been different from the other categories.

p. 264. One last remark, on a quite different form. The imperative ending of the *s*-aorist, *-on* in *deikson* has not been explained. Rix suggests that it is the accusative of an *o*-stem used as infinitive, which was again used as an imperative. This is not impossible, but it is just a guess. The problem is that there is no form in any other language with which it could be compared, nor can its formation be understood within Greek. There is no solution in sight.

4. Concluding remarks

Which conclusions about the method can be drawn? I intimated my conclusion at the beginning: there are many problems of detail left, and it will be apparent that we have not gone into very much depth. (To give an example: a difficult point is the development of the sequence HRHC-, i. e., word initial laryngeal + resonant + laryngeal before consonant. This is rather specific, but it is the kind of thing we are working on today. It is probably found in Gr. *ónoma* 'name' < **h₃nh₃mn.*) The other side is that the historical phonology and morphology are, with some exceptions, clear. We have seen that this must be due to special circumstances (Greek is not too far removed from the proto-language; it is very close to Sanskrit; etc.). The important point to note here is that, if the circumstances are favorable, i. e., if we have sufficient documentation, the historical interpretation of the language provides no problems that make us doubt the methods used. I would like to end, then, with this conclusion: the method of historical linguistics developed for the Indo-European languages has proven adequate for the interpretation of the whole structure of the language in the case of Greek.

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