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## SOME METHODOLOGICAL REMARKS ON THE PHONOLOGY OF ANCIENT GREEK

*In the second half of the twentieth century, phonological explorations of Ancient Greek and its dialectal diversity took two daring steps closely connected with the methodological advances in general linguistics. The first step was taken by Martín Ruipérez in 1956, who attempted to explain the sound changes in the vocalic systems of Attic and Boeotian by indicating what he thought to be the underlying structural causes of these changes. The second step was taken in 1984 by authors associated (at that time) with Université de Nancy, namely Monique Bile, Claude Brixhe and René Hodot, in a plea for proper incorporation of relevant sociolinguistic factors that could be held accountable for phonological changes in Ancient Greek. These attempts were deemed to be nothing short of methodological revolutions in the study of the development of Ancient Greek phonology and mark a decisive break with non-teleological description of the sound changes predominant in the nineteenth and first part of the twentieth century. The main aim of this paper is to evaluate the application and validity of these approaches in the study of Ancient Greek dialects in general, as well as through one specific example, namely the graphical unsteadiness of the representation of original inherited Proto-Greek /ē/ in Elean dialect.*

**Keywords:** Greek; phonology; structuralism; sociolinguistics; dialectology; vocalism; Elean

*Fool:* The reason why the seven stars are are no more than seven is a pretty reason.

*Lear:* Because they are not eight?

*Fool:* Yes, indeed. Thou wouldst make a good fool.

William Shakespeare, *King Lear*<sup>1</sup>

It is a mistake to think you can solve any major problems just with potatoes.

Douglas Adams, *The Hitchhiker's Guide to the Galaxy*<sup>2</sup>

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<sup>1</sup> JOWETT, JOHN – MONTGOMERY, WILLIAM – TAYLOR, GARY – WELLS, STANLEY [EDS.]. 2005. *The Oxford Shakespeare. The Complete Works*. Oxford: Clarendon Press.

<sup>2</sup> ADAMS, DOUGLAS. 2002. *The Ultimate Hitchhiker's Guide to the Galaxy*. New York: Del Rey.

To state the obvious, phonetical features of Ancient Greek cannot be observed directly, since actual speakers of this language are long gone.<sup>3</sup> Therefore, the phonological system has to be reconstructed on the basis of epigraphical evidence,<sup>4</sup> literary evidence,<sup>5</sup> loanwords, and occasional remarks of ancient grammarians.<sup>6</sup> Raw empirical data such as these in turn have to be processed, given phonological interpretation, and (optionally) the assessment of the relative or absolute chronology. Although this process is more often than not complicated and interpretations of same sets of data can be largely divergent, as two large-scale studies of the phonology of Attic dialect by Sven-Tage Teodorsson<sup>7</sup> and Leslie Threatte<sup>8</sup> have shown, there is no question about the fundamental importance of this undertaking for the study of the phonology of Ancient Greek.

The study of Ancient Greek dialects and their phonology in its early years has been largely (if not entirely) exempt from teleological explanations of the sound changes themselves. Simply put, scholarship has been based on the two main components I have outlined in the preceding paragraph: Manuals from the first half of the twentieth century collected the wealth of (mostly) epigraphical data and ordered it in respect to provenience and phonological content. Rest assured, theoretical and methodological discussions thrived during this period as they indeed do today, but the focus was mainly on “Sprachgeographie”, not causal explanation. To use the expression coined by Anna Morpurgo-Davies, in the “constant tug-of-war between ‘classifiers’ and ‘separatists’”,<sup>9</sup> central topics were those of genealogy, taxonomy, classification, and internal relationships between the dialects. This line of methodological considerations culminated with the publishing of some major mid-century works, e.g. TOVAR (1944), RISCH (1949, 1955),

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<sup>3</sup> With the possible exception of Tsakonian, which conserved some important characteristics of the ancient Laconian dialect.

<sup>4</sup> At times obscuring the data instead of clarifying them, BILE – BRUXHE – HODOT (1984: 160) even suggest, that dialectologist should constantly control the output of the epigraphist.

<sup>5</sup> Literary evidence of the Greek dialects is sometimes questionable due to the normalization of language during the transmission of manuscripts, see, for instance, CHANTRAINE (1948: 5–16) for Homeric poems and HAMM (1957: 3–4) for Lesbian poetry.

<sup>6</sup> See ALLEN (1987: 162–168) for a convenient sample.

<sup>7</sup> TEODORSSON (1974).

<sup>8</sup> THREATTE (1980).

<sup>9</sup> MORPURGO-DAVIES (1992: 415). Pages that follow (415–420) offer a succinct overview of these methodological questions. See also BARTONĚK (2009: 11–28) for a concise history of Greek dialectology in the twentieth century.

ADRADOS (1952), and PORZIG (published 1954, drafted as early as 1945) and continues to attract considerable attention of scholars up to this day.<sup>10</sup>

To illustrate this methodological procedure with a specific example from Greek dialectology, let us consider the long vowel system of the Elean dialect. To set the stage, it seems that the vocalic system of (late) Proto-Indo-European consisted of two analogical fivefold series of short (/a/, /e/, /i/, /o/, /u/) and long (/ā/, /ē/, /ī/, /ō/, /ū/) vowels. Putting some recurrent issues aside,<sup>11</sup> it is safe to say that this is the *communis opinio* shared by the majority of scholars.<sup>12</sup> Since Ancient Greek conserved original Proto-Indo-European vocalism with the highest degree of precision of all the daughter languages,<sup>13</sup> we assume the same vocalic system for Proto-Greek and indeed find its attestation in the oldest extant Greek dialect, Mycenaean Greek.<sup>14</sup>

Now, let us see what the situation in Elean is. If we collect Elean inscriptions, we will find a striking peculiarity. Namely, there is a variance in the spelling of the inherited /ē/ vowel, which is represented graphically at times as <E>, at times as <A> (before 400 BC), and after the introduction of Ionic alphabet as <H> or <A>, respectively. We can also observe that the secondary /ē/, that is, /ē/ created by first compensatory lengthening or tautovocalic contraction, is unequivocally represented as <E>. At the same time, every /ā/, be it inherited or the result of the lengthenings or contractions mentioned just above, is represented graphically as <A>. We can also add that this unsteadiness in spelling is invariant of the phonological neighborhood of the inherited /ē/ vowel and that it is attested from the oldest Elean inscriptions (6<sup>th</sup> century BC) down to the 2<sup>nd</sup> century BC. These are the empirical facts that Elean inscriptions offer.<sup>15</sup>

Given the data, we can attempt their phonological interpretation: Unsteadiness of the graphical representation of primary /ē/ vowel, as opposed

<sup>10</sup> See HAJNAL (2007) for a recent review.

<sup>11</sup> Especially the question of the existence of original Proto-Indo-European /a/, denied by LUBOTSKY (1989) and BEEKES (1995: 138–139).

<sup>12</sup> MAYRHOFER (1986: 90); LEHMANN (1993: 95); SZEMERÉNYI (1997: 37); MEIER-BRÜGGER (2002: 71, 76); CLACKSON (2007: 34); FORTSON (2010: 66).

<sup>13</sup> CHANTRAINE (1948: 17); MORANI (1999: 22); FORTSON (2010: 256).

<sup>14</sup> BARTOŇEK (2007: 89–90).

<sup>15</sup> See MINON (2007: 288–290) for detailed epigraphical evidence. Typological parallel for the opening of inherited /ē/ to /ā/ can be found in the transition from Proto-Germanic to West-Germanic: *OHG sāmo* ‘seed’ (cf. *lat. sēmen*); *OHG māno* ‘moon’ (cf. *lat. mēnsis*). See also FORTSON (2010: 343) citing German *Tat* and Old Norse *dād* < *PIE* \*dheh<sub>1</sub>-tī-.

to the straightforward representation of secondary /ē/ and /ā/ of every and any provenience indicates, that the pronunciation of the primary /ē/ vowel was very open, probably [æ̃], which explains reasonably well the empirical evidence at our disposal, namely the fluctuation in the representation of the said vowel as both <E> and <A>. We could conclude that the Elean dialect had three distinct phonemes: Primary /ē/ realized phonetically as [æ̃], secondary /ē/ realized phonetically as [ē] or [ē̃] and /ā/ realized phonetically as [ā̃]. At the same time, though, Elean had only two graphical representations of these phonemes, <E> and <A> (*ante* 400 BC) or <H> and <A> (*post* 400 BC). This situation caused the unsteady graphical representation of the phone [æ̃] with <E> and <A>, since there was no special character to represent it with.

Should we turn our attention to older manuals of Ancient Greek dialects, we find, in one form or another, this very analysis. Albert Thumb lists<sup>16</sup> a few examples for the spelling of <A> instead of <H> (ἑράτρα, μά, καθαλήμενοι = καταδηλούμενοι, πατάρ, ἔα = εἶη, δαμοσοία, φαίναται = φαίνηται, δοθαῖ = δοθηῖ, ἀποσταλᾶμεν = -ῆναι etc.) and adds with caution that this phenomenon might be due to the open pronunciation of <η>. Friedrich Bechtel shares this interpretation and connects it with a similar phenomenon in the short vowel system,<sup>17</sup> Eduard Schwyzler simply notes that in Elean we maybe (*sic!*) find the graphic <ā> for original inherited Proto-Greek <η><sup>18</sup> and Carl Darling Buck adds the obvious and fairly unproblematic phonological interpretation given just above: The reason behind the fact that <η> was “frequently, but by no means consistently” represented as <ā> lies in its open pronunciation.<sup>19</sup> It needs to be pointed out, though, that Buck’s analysis is at best incomplete, since one has to specify that this sound change can only be observed in the cases of primary inherited Proto-Greek <η>. Notice that there are no attempts to incorporate this “innovation” in the vocalic system of Elean into relative or absolute chronology and, more importantly, no efforts are made to analyze the underlying causes of this sound change. The interpretations of the unsteady graphical representation simply presuppose the opening in the pronunciation of primary /ē/, which is in turn justified by its explanatory potential (i.e., explains the graphical representation, but does not present any teleological explanation for the sound change itself). To be sure, even older scholars ventured at

<sup>16</sup> THUMB (1909: 173–174).

<sup>17</sup> BECHTEL (1923: 829).

<sup>18</sup> SCHWYZER (1953: 92).

<sup>19</sup> BUCK (1955: 25).

times to indicate underlying causes for a particular sound change, but when they did, they proceeded with utmost caution.<sup>20</sup>

As mentioned already in the opening section, the first great turn in the methodological approach to Ancient Greek phonology came with the publication of the study of the development of Attic and Boeotian long-vowel systems by Martín Ruipérez in 1956, entitled *Esquisse d'une histoire du vocalisme grec*.<sup>21</sup> In his study (nowadays considered “epochal”)<sup>22</sup> Ruipérez, armed with the structuralist theory of sound change developed by prominent French linguist André Martinet and published a year earlier,<sup>23</sup> is proud to announce that he is set to explain the “internal dynamic” and “immediate causality” of the said changes, while indeed rightly noting that the history of Greek vocalism so far has been “purely descriptive” in this regard.<sup>24</sup> In spite of the severe criticism of this approach by his fellow Spanish colleague José Lasso de la Vega,<sup>25</sup> the structuralist explanation of sound changes in Greek dialects has been the main driving force behind the first overall systematic description of the development of long vowel phonology of Ancient Greek provided Antonín Bartoněk in his important monograph from 1966.<sup>26</sup> This particular application of structuralism in the field of Ancient Greek dialectology marks a definitive departure from the largely descriptive and non-teleological approach typical for the first half of the twentieth century, since it aims to expand mere phonological interpretations of graphical representation by identifying the causes of sound changes themselves.

The second great turn can be seen in the publication of the so-called “sociolinguistic manifesto”<sup>27</sup> by Monique Bile, Claude Brixhe and René Hodot under the title *Les dialectes grecs, ces inconnus*. This paper, written in a strong polemic tone (as the authors specify, it is not because they would

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20 Notice, for instance, the careful wording of KRETSCHMER (1909: 33) while arguing that the change of Ionic /ā/ to /ē/ was due to the Carian substrate influence: “So viel wird man, denke ich, zugeben, daß die Annahme, das ionische η für ā beruhe auf karischer Artikulation, eines gewissen Anhaltes in den Tatsachen nicht entbehrt, und für die richtige Einschätzung der historischen Bedeutung dieses Lautwandels ist auch schon die bloße Möglichkeit von Interesse.”

21 RUIPÉREZ (1956).

22 GARCÍA RAMÓN (2006: 62).

23 MARTINET (1955).

24 RUIPÉREZ (1956: 67).

25 LASSO DE LA VEGA (1956).

26 BARTONĚK (1966).

27 Term coined by MÉNDEZ DOSUNA (2004: 180).

enjoy it, but because Greek dialectology needs to “wake up”),<sup>28</sup> demands a total overhaul in the methodology of the treatment of Greek dialects (“véritable rupture épistémologique”).<sup>29</sup> Bile, Brixhe and Hodot react directly to the structuralist tendencies and cite the aforementioned article of Ruipérez as the prime example of this approach, while noting, of course, that the structural study of any given dialect on its own cannot account for each and every sound change. Special attention has to be given to a wide array of external factors that shape sound changes, be those historical, social or geographical, while it is safe to say that the main focus of these scholars rests on the influence of the society on language.

Let us observe the merits of these two methodological turns in our example of the Elean sound change. We shall start with the analysis proposed by Bartoňek (since Ruipérez discusses only Attic and Boeotian), who has been dealing with this phenomenon in a number of articles and monographs and applies internal as well as external factors in the explanation of the sound change at hand.<sup>30</sup>

Bartoňek dates the opening of primary /ē/ prior to the first compensatory lengthening in relative chronology, which in turn gives the estimate of the 12<sup>th</sup> century BC in absolute chronology,<sup>31</sup> on the following grounds: The developments on the front long-vowel axis are not paralleled on the back long-vowel axis, where the products of the first compensatory lengthening merged together with the already existing mid vowel /ō/. By placing the opening of primary /ē/ before the first compensatory lengthening, we can presuppose free articulatory space on the front long-vowel axis (due to the shift of /ē/ to /ǣ/). This, in turn, due to the “economy” of phonological systems (i.e., due to structural reasons), allows the products of the first compensatory lengthening in /e/ vocalism to “fill in an empty slot”. Since there was no “empty slot” on the back long-vowel axis, the secondary /ō/ merged with the primary /ō/. The result of these changes is the unsteadiness in the graphical representation of primary /ē/, as discussed above.<sup>32</sup>

Since the datation of the opening of primary /ē/ to /ǣ/ is set before the compensatory lengthening, compensatory lengthening itself cannot be the cause for this change. Accordingly, Bartoňek explains the opening of primary /ē/ by the influence of substrate language (an external factor), which

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28 BILE – BRIXHE – HODOT (1984: 199).

29 BILE – BRIXHE – HODOT (1984: 155).

30 BARTOŇEK (1964); BARTOŇEK (1966: 89–99); BARTOŇEK (2009: 138).

31 BARTOŇEK (1964: 108–109).

32 BARTOŇEK (1964: 99).

can be characterized by a general tendency to open vowels.<sup>33</sup> To sum up, the Elean dialect first opens the pronunciation of primary /ē/ because of the influence of substrate language (external or social factor), leading to the “empty slot” on the front axis of long vowels. Compensatory lengthening takes place, secondary /ē/ (as a product of this lengthening) “fills in” the vacated spot, while secondary /ō/, having no “free slot” to occupy, simply merges with primary /ō/ (internal or structural factor).

Bartoněk’s proposal, in spite of its apparent ability to neatly explain the empirical data in Elean, was met with reserve. José Luis García Ramón objected that the explanation of the cause of the opening of primary /ē/ by postulating a Pre-Greek substrate language with no empirical evidence of the structure of its vowel system is a *deus ex machina* invocation with no explanatory value.<sup>34</sup> To be sure, in the latest recapitulation of the problematique by Bartoněk, there is no longer any mention of a Pre-Greek substrate language and the process is explained in purely structural terms.<sup>35</sup> This discards the alleged cause of the opening of the primary /ē/, but not the rest of the argumentation. Nevertheless, García Ramón proposed different considerations on this sound change, by the use of the same structural arguments, but with important changes in chronology. He posits the opening of primary /ē/ after the first compensatory lengthening and thus this very process could be seen as the cause for the opening. According to this interpretation, secondary /ē/, as a result of the first compensatory lengthening, was realized phonetically with a half-closed quality as [ē̄], which in turn caused the opening of primary inherited /ē/ to [ā̄], once again due to the presumed effect of the “economy” of phonological systems.<sup>36</sup>

A few years later, Julián Méndez Dosuna, while agreeing with García Ramón in rejecting the theory of substrate influence, tried to go one step further and attempted to discard Bartoněk’s argument about the asymmetrical distribution of the phonemes on the long-vowel front and back axis by pointing out that this phenomenon is due to the inherent asymmetry of the articulatory space within the front and back axis of the long-vowel system (the back axis having considerably less articulatory space, thus being able to “support” a reduced number of phonemes).<sup>37</sup> He also agrees with García Ramón that the first compensatory lengthening is anterior to the vocalic

<sup>33</sup> BARTONĚK (1964: 104) *et passim*.

<sup>34</sup> GARCÍA RAMÓN (1975: 73).

<sup>35</sup> BARTONĚK (2009: 138).

<sup>36</sup> GARCÍA RAMÓN (1975: 74).

<sup>37</sup> MÉNDEZ DOSUNA (1980: 186–188).



change in question and he explicitly states that one was the cause of the other (“el proceso de apertura de /ē/ ha comenzado necesariamente después del de los alargamientos compensatorios, que han sido su causa”).<sup>38</sup>

Marisa del Barrio Vega is essentially in agreement with both Spanish scholars (and she refers to both in her paper).<sup>39</sup> Her exposition, though, does not treat the sound change in Elean as an isolated phenomenon, but considers it from a more general point of view of her genuine hypothesis, according to which the so-called “vocalisme mitior” (in which the outcome of first compensatory lengthening is the creation of secondary /ē/ and /ō/ with a rather closed quality) is to be admitted within every single dialect of Ancient Greece, and the subsequent merger with primary mid vowels is to be treated as an innovation.<sup>40</sup> This hypothesis, hinted at already in 1979 by George Sheets,<sup>41</sup> completely reverses the *communis opinio*, according to which “vocalisme mitior” was the innovation and “vocalisme severior” the archaism. This obviously has important implications for the dialectal classification of Elean.<sup>42</sup>

Finally, the author of the recently published two-volume exposition of Elean dialect, Sophie Minon, is apparently also in agreement with García Ramón and Méndez Dosuna.<sup>43</sup> Since there is no directly identifiable phonological neighborhood in which the lowering and opening of the original /ē/ could be found, it is impossible to give a reason for the opening of the vowel in question, if we assume that compensatory lengthening took place only after this change. The substrate hypothesis obviously does not solve anything and the argument from the comparison of front and back long-vowel series does not seem to be valid due to the inherent asymmetrical distribution of the articulatory space.

It would seem that the scholarly consensus discarded both explanations proposed by Bartoněk: Substrate hypothesis was abandoned completely and the structural argument has been reverted because of the different chronology presumed. But is the opposing interpretation better founded? In fact, setting the rightly criticized substrate hypothesis aside, is there any fundamental difference between the structural analysis offered by Bartoněk and

38 MÉNDEZ DOSUNA (1980: 200).

39 DEL BARRIO VEGA (1998: 263–264, 273).

40 DEL BARRIO VEGA (1998: 272).

41 SHEETS (1979: 565).

42 BARTONĚK (2009: 139) does not accept this hypothesis, since he terms the dialects with a sevenfold long vocalic system “innovative” and dialects with a fivefold long vocalic system “conservative”.

43 MINON (2007: 292–293).

the structural analysis offered by, say, García Ramón, other than whether the secondary /ē/ “pushed” the primary /ē̄/, or simply occupied its vacant position? More importantly, is there any empirical evidence that could resolve the issue at hand? And if not, where precisely is the difference between an attempt to explain the origin of the sound change by postulating the influence of the substrate language (about which we know nothing) and an attempt to explain the origin of the sound change by postulating two structural processes without any empirical evidence to support either of them?

The best proposal is probably that of del Barrio Vega, since it is clear that her hypothesis is not an *ad hoc* solution but a corollary of the general thesis according to which all Greek dialects created new (rather) closed mid vowels following the first compensatory lengthening. This is in turn vindicated by greater explanatory potential than its predecessor (“l’hypothèse [...] apporte une solution à certains des problèmes que ne résout pas l’explication courante”),<sup>44</sup> which pronounced the sevenfold long vocalic system to be an innovation of certain dialectal groups.

Yet more importantly, the structural argument offered by García Ramón and accepted by other scholars is not an explanation properly speaking, but a mere description.<sup>45</sup> It does not apply any *general* structural principle that would ascertain the change at hand. After all, the development following the first compensatory lengthening in Northwest Greek, Ionic–Attic and dialects of Isthmus and the Saronic gulf on one side and the development of the rest of Greek dialects on the other side was completely different. In Northwest Greek, Ionic–Attic and dialects of Isthmus and the Saronic gulf the products of the first compensatory lengthening did not merge with the original long vowels, thus creating the long vowel system with seven members (which resulted in further major changes), while in the rest of the dialects, the products were completely integrated and merged with the original vowels, thus falling back to the “original” Proto-Greek long vowel system with five members.

If we are going to base the argument in the asymmetry between the front and back long-vowel axis (which is general, since it is a feature of the properly developed vocal tract and articulatory organs of humans as a species,

<sup>44</sup> DEL BARRIO VEGA (1998: 272).

<sup>45</sup> SHEETS (1979: 565) arrives to the same conclusion in regard to the treatment of compensatory lengthening as such: “Now when we observe that a phonemic ‘merger’ has occurred, say /e/ > /ē̄/, we should not be misled into inferring a teleological principle from it. Yet the formulation ‘compensatory lengthening’ implicitly rests on such an inference. It purports to identify causal relationships and to predict outcomes. It claims to explain phenomena which it is, in fact, merely describing”.

thus applicable for every language and dialect), noting that it is only “natural” for Elean to accommodate new phone on the front vowel axis (containing more articulatory space) and merge analogous phone on the back vowel axis (containing less articulatory space) and explain the sound change in Elean consequently by structural reasons alone, how is it possible that this explanation does not hold in the case of, say, Ionic-Attic (which merged neither secondary /ē/ nor secondary /ō/) or Doric (which did merge both secondary /ē/ and secondary /ō/)?<sup>46</sup>

I doubt we can operate within a framework of some *general* “structural principle” that would govern the outcome of the “intrusions” in the vocalic system of Greek. Our “explanations” are just simple tautological descriptions in the form of “we have a change of primary /ē/ to /ǣ/ in Elean, because in Elean (and only in Elean) /ē/ changed to /ǣ/.” The first compensatory lengthening is not a cause in itself (as Méndez Dosuna thinks, for instance, as we have seen),<sup>47</sup> it is just the catalyst of the cause. It cannot be the cause if the time-tried “same causes, same effects” principle should hold. In the case at hand, the same “cause” (namely first compensatory lengthening) would have three different outcomes in the long-vowel system of Ancient Greek dialects: Sevenfold long-vowel system of Northwest Greek, Attic-Ionic and dialects of the Isthmus and the Saronic gulf, sixfold long-vowel system of Elean and fivefold long-vowel system of all other dialects.

Moreover, if the change in question is a simple vowel shift – and I believe that the case of Elean, unlike the development in, say, Attic, can be treated as a simple vowel shift – there is almost no structural pressure present. As William Labov states,<sup>48</sup> “there are very few constraints on such simple movements: it is not difficult to find examples of vowels becoming higher or lower, backer or fronter, rounded or unrounded, nasalized or un-nasalized”.

These considerations inevitably bring us to the evaluation of the capacity of a purely structural explanation to account for the data we have at our disposal. As José Lasso de la Vega showed in a paper published in reaction to the groundbreaking study by Ruipérez, this capacity is indeed very limited.<sup>49</sup> In fact, the problem was anticipated two years earlier by Manu Leumann, who expressed grave doubts about the exclusive use of structur-

<sup>46</sup> Similar point has already been made by LASSO DE LA VEGA (1956: 284–285) as a reaction on RUIPÉREZ (1956) in regard to the vocalic systems of Ancient Greek dialects in general.

<sup>47</sup> MÉNDEZ DOSUNA (1980: 200).

<sup>48</sup> LABOV (2010: 30).

<sup>49</sup> LASSO DE LA VEGA (1956).

alist principles within the area of “dead” languages. In his own words: “In der Theorie klingt all das ganz einleuchtend. Doch die sprachlichen Realitäten fügen sich, wie mir scheint, nur recht mühsam zusammen mit dieser Auffassung vom symmetrischen System der Phoneme und der Tendenz zu dieser Symmetrie.”<sup>50</sup>

Indeed, it could be objected that the failure of the structuralist approach to convincingly account for all the intricacies of the sound changes in Greek dialects does not mean that some other theory could not supplement this approach based on the analysis of internal factors, or take its place. This possibility brings us to the second milestone in the study of the phonology of Ancient Greek dialects. We have already mentioned the manifesto from 1984 by Bile, Brixhe and Hodot and its main aims. Other works in the similar vein followed in a short span of time, be it Brixhe himself and his study of consonantism<sup>51</sup> or Yves Duhoux dealing with vocalism.<sup>52</sup> Sociolinguistic approaches with an emphasis on the appreciation of external factors in the purported explanations of sound changes have been fairly popular in the past twenty years, as Méndez Dosuna rightly states in a review of the 1984–2004 period of the scholarly output in the field of Ancient Greek phonology.<sup>53</sup>

As far as sociolinguistics go, the question remains of whether the fragmentary and limited nature of the data related to the social milieu in ancient Greece can be of any use at all. As we have seen in the case of Elean, the hypothesis of the influence of some unknown language has been quickly dropped, simply because we don’t know anything about this “substrate” language and we are effectively trying to explain something obscure with something even more obscure, which recalls Aristotle’s critique of Plato’s theory of forms: ὡσπερ εἴ τις ἀριθμῆσαι βουλόμενος ἐλαττόνων μὲν ὄντων οἴοιτο μὴ δυνήσεσθαι, πλείω δὲ ποιήσας ἀριθμοῖη (*Met.* 990b1–4).

Of course, one could object that if a sociolinguistic analysis does not fare well in this particular case, it does not mean it will not work in other cases. The most important factor here is, in my opinion, an (at least theoretical) possibility of falsification of any proposed hypothesis. If a hypothesis cannot be falsified, it is of no use to us. Unfortunately, this happens all too often even in cases where we have the best available data pertaining to both social milieu and language. The studies of Attic by Teodorsson and Threatte mentioned in the beginning of this paper work with the same epigraphical

50 LEUMANN (1958: 121).

51 BRIXHE (1996: 7–24).

52 DUHOUX (1987: 195).

53 MÉNDEZ DOSUNA (2004: 180).

and historical data, but come to strikingly different conclusions in chronology, mainly due to the attempt to distinguish “low” and “high” classes of population speaking Attic.<sup>54</sup>

The scarcity of relevant data for a decent sociolinguistic treatment of any ancient Greek dialect is best illustrated in the article by Duhoux from 1987, where he uses Plato’s *Cratylus* and its fantastic etymologies as a reliable source of information for the sound changes in Attic dialect in order to make a case for the alleged itacism within higher ranks of Athenian society at as early as about 450–400 BC,<sup>55</sup> in spite of the fact that “virtually no modern interpreter since Grote, 130 years ago, has suspected that Plato could possibly have believed them”.<sup>56</sup> One could indeed argue that Duhoux is interested in phonetic features, not etymologies. But if Plato made up the etymologies, how can we know if he did not make up also the phonetic features he constantly uses to back them up. Setting the issue of etymologies aside, would Duhoux agree, for instance, that Greek letter *rho* expresses “change of place, movement and hardness” (τὸ ῥῶ τῆ φορᾶ καὶ κινήσει καὶ σκληρότητι προσέουκεν, *Cratylus* 434c1–2)? And if not, on what grounds? Presumably because this explanation has nothing to do with the more general phonetic feature he is set to defend, which amounts to cherry-picking the source for information confirming one’s own hypothesis and leaving the rest aside.

Proponents of the sociolinguistic approach in the field of Ancient Greek linguistics do not seem to appreciate the insurmountable problems connected with this approach. A quick look in the second part of Labov’s *Principles of Linguistic Change* speaks volumes about the amount of data needed to assess social factors in a sound change. To give just a single example, in the study of the Philadelphia vowel system, details available about participants look roughly as follows:<sup>57</sup> “Figure 4.2 shows the main values of the vowel system of the oldest male speaker on Wicket St., Joe Donegan, who was 84 years old when he was interviewed in 1974. His parents came from Ireland in the late 19<sup>th</sup> century, and he was of the first native-born Irish generation.

<sup>54</sup> WYATT (1970: 624) advocated this distinction even for “dialectally uniform” Proto-Greek!

<sup>55</sup> DUHOUX (1987: 192).

<sup>56</sup> SEDLEY (1998: 140). To be fair, it has to be noted that Sedley himself is criticizing this *communis opinio*. But if Duhoux intends to use *Cratylus* as a reliable source of information on linguistic matters, given the reputation of Plato’s works, it would be advisable to present rigorous arguments defending its veracity, rather than to simply presuppose that the information contained in the dialogue is of any historical value.

<sup>57</sup> LABOV (2001: 133).

He had an eighth grade education, and worked as an unskilled laborer, at a lower level than \$5000, but received an upkeep rating of 3, “improved.” Donegan gives us the most conservative view of the Philadelphia vowel system, in some ways more conservative than that of the middle-aged Atlas informants who represent his age cohort.” To say that we will never get this kind of information along with the phonetic data about any single speaker of any dialect of Ancient Greek is stating the obvious, and that is not to speak about technical features of the measurement of formant positions and alike.

Obvious as these problems and the question of the possibility of application of these methods in the field of Ancient Greek might be, Claude Brixhe recently reiterated the key points made in his 1984 programmatic article, deploring the current state of Greek dialectology and the failure to appreciate the importance of the sociolinguistic elements in the scholarly research.<sup>58</sup> He accepts the appreciation for some structuralist principles in Greek dialectology as a huge step forward, but laments that, in the last twenty years, the situation in the Greek dialectology has not improved at all, presumably due to the lack of sociolinguistic enterprises in the field.

In my opinion, there is one crucial difference between internal and external factors, namely the problem of generalization: Structuralism could have at least partially worked, since it is based on very general principles, such as the properties of the human vocal tract and articulatory organs. These are equivalent for all speakers and culturally and socially independent. Sociolinguistic factors are *ex definitione* socially dependent, and therefore it is impossible to extrapolate from any data pertaining to the external factors in modern languages to the “dead” languages of the past. This is at least partially possible only when purely structural (or internal) factors are taken into account. If we lack relevant information about the social structures and their matching linguistic competences (as we surely do in the case of Ancient Greek), we simply lack any empirical footing to back our assertions.

Moreover, scholars critical of sociolinguistic approaches do not criticize sociolinguistics *as such*. They just appreciate the difficulties in making it work in the context of a language with no speakers alive and only fragmental epigraphic and social data. Méndez Dosuna rightly notes<sup>59</sup> that it is one thing to acknowledge the importance of external or social factors in the development of any language (which is unproblematic and obvious), but it is clearly something completely different to pretend to have the power to identify these factors in a language like Ancient Greek and its dialectal

<sup>58</sup> BRIXHE (2004: 116–117). *Non vidi*, quoted after GARCÍA RAMÓN (2006: 80).

<sup>59</sup> MÉNDEZ DOSUNA (2004: 176).

varieties. García Ramón is probably too harsh in ascribing the thesis that “the Greek inscriptions offer us the same possibilities of study as a spoken dialect”<sup>60</sup> to Bile, Brixhe and Hodot, but he rightly warns us that this approach opens the gates for questions that generate answers which cannot be readily falsified and are thus to be rejected as “naïve optimism”<sup>61</sup> and pure “fantasies”.<sup>62</sup> Ivo Hajnal apparently shares the same viewpoint, since he notes that the sociolinguistic approach is “destined to fail” (“solche Versuche sind jedoch per se zum Scheitern verurteilt”).<sup>63</sup> Even scholars generally favorable to sociolinguistics, as is Anna Morpurgo-Davies, for whom the 1984 propositions by Brixhe et al. are “worthwhile and ought to be listened to”, cannot do otherwise but note the discrepancy between “limited evidence” and much (the author of this paper is inclined to say too much) faith being put into it.<sup>64</sup>

If some scholars in the field reject the proposals of sociolinguistics, it is not because they would fail to appreciate its merits, but simply because the data for this kind of analysis are not there for Ancient Greek, and most likely they will never be there. This is, then, not a failure of the progress in the phonology of Ancient Greek, as Brixhe would want to have it, but a successful move to evade the sweet melodies of sociolinguistic sirens. We should heed the wise words of Circe here: *παρεξέλααν* (*Od.* 12.47).

To conclude, internal factors alone, while readily applicable to Ancient Greek, are not sufficient to account for even the majority of the changes. As Sidney Allen suggested after the evaluation of the papers of Ruipérez and Lasso de la Vega, “in the present state of our researches it might be wiser to do no more than observe that phonetic developments do in fact frequently have the effect of resolving asymmetries in the phonological system – and to keep our statements on this descriptive level rather than that of casual or teleological explanation”.<sup>65</sup> On the other hand, external factors of sociolinguistic provenience are simply out of our reach, buried under the dust of the past.

From this state of affairs immediately follows that any phonological analysis of Greek dialects which goes beyond the mere description of graphical evidence and its non-teleological phonological evaluation has to

<sup>60</sup> GARCÍA RAMÓN (2006: 61).

<sup>61</sup> García Ramón in discussion following the paper presented by Anna Morpurgo-Davies in 1992, *q.v.*

<sup>62</sup> GARCÍA RAMÓN (2006: 72).

<sup>63</sup> HAJNAL (2007: 143).

<sup>64</sup> MORPURGO-DAVIES (1992: 420).

<sup>65</sup> ALLEN (1958: 240).

use careful methodology to set apart internal and external factors of the sound change and, most importantly, carefully assess whether we have any possibility of discovering these factors from the data we have at our disposal.

Méndez Dosuna, while accounting for the years 1984–2004 in the area of Greek phonology, might have been right in pointing out the insufficient theoretical and methodological background for the study of Ancient Greek dialects, save for “honorable exceptions”.<sup>66</sup> I think that Greek dialectology, after two major changes proposing structuralist and sociolinguistic explanations, is ripe for throughout critique of its own methods. It seems that the field lacks its own version of Immanuel Kant’s *Prolegomena zu einer jeden künftigen Phonologie, die als Wissenschaft wird auftreten können*. After a period of creation and trial of new methodological principles and growth of knowledge, it is time to ponder for a while and acknowledge definitive limits in our knowledge: *Non liquet* should be perfectly acceptable position in Greek dialectology. Because, as Louis Hjelmslev pointed out, in the study of the phonology of an ancient language, there are always inherent limitations. Not only that, he even defines “dead languages” as languages in which “le système phonique reste conjecturale, hypothétique à des degrés divers, il est vrai, mais toujours en principe hypothétique”, while maintaining that this applies “sans exception aucune”.<sup>67</sup> The acknowledgement of these limits should be the first condition of any further description of the phonological system of any dialect of Ancient Greek (and any other “dead” language, for that matter). “Historical linguistics can then”, as Labov states, “be thought of as the art of making best use of bad data. The art is a highly developed one, but there are some limitations of the data that cannot be compensated for”.<sup>68</sup>

To return to our example from Elean, it might be that we are not entitled to do much more than to simply describe the sound change without entangling ourselves in the speculative reasons lying behind it, as, for instance, Vít Bubeník does.<sup>69</sup> Though this would require taking a few steps back from the purported causal explanations to a largely descriptive approach and from the hunt of inherent causes to simple statements of the fact. Rest assured, this paper does not want to suggest that we should resign completely on an explanatory approach in regard to the sound changes in

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<sup>66</sup> MÉNDEZ DOSUNA (2008: 315).

<sup>67</sup> HJELMSLEV (1958: 109).

<sup>68</sup> LABOV (2010: 11).

<sup>69</sup> BUBENÍK (1983: 36).



Ancient Greek dialects, but it seems clear to me that a careful critique of methods used is a necessary (but by no means sufficient) condition thereof.

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## RESUMÉ

Článek pojednává o možnosti aplikace strukturalistického a sociolingvistického přístupu na fonologii klasické řečtiny, a to jak teoreticky, tak aplikovaně na příkladě elejštiny. Autor dochází k závěru, že použití obou zmíněných přístupů je v klasické řečtině značně omezeno: strukturalismus bez dalšího nepostačuje na kauzální analýzu hláskových změn a pro úspěšnou aplikaci sociolingvistiky scházejí relevantní data. Každé teleologické uchopení hláskových změn by tedy mělo být opatřeno důslednou metodologií, v rámci které by autoři nejdříve demonstrovali samotnou fundamentální možnost kauzální analýzy dochovaných dat.