MYKENAÏKA

Actes du IXe Colloque international sur les textes mycéniens et égéens
organisé par le Centre de l'Antiquité Grecque et Romaine de la Fondation Hellénique
des Recherches Scientifiques et l'École française d'Athènes
(Athènes, 2-6 octobre 1990)

ÉDITÉS PAR

Jean-Pierre OLIVIER

1992

MYCENAEAN, ARCADIAN, CYPRIAN AND SOME QUESTIONS OF METHOD IN DIALECTOLOGY

1. After almost forty years of discussions we still do not know how to classify the Mycenaean dialect : does it belong with Arcado-Cyprian or with Aeolic or with Attic-Ionic or is this an unanswerable question ? We accept, however — or most scholars do — that Mycenaean is a Greek dialect. If so, we have a Greek dialect written and spoken in a localizable part of Greece some five hundred or more years earlier than those later dialects that we can describe on the basis of alphabetic evidence. This ought to induce us to reconsider somewhat more broadly than has been done so far whether the methodology and results of our studies in Greek dialectology are compatible with the new data. I may anticipate at this stage the problem on which I shall focus : is it possible to compare Greek dialects and reconstruct backwards just as we compare related languages and reconstruct the parent language from which they descend ? Or is it the case that mutual influences or, one may say, mutual contamination between dialects is such that the normal techniques of genealogical comparison do not apply ? Before I turn to this question it is useful, however, to give a look, necessarily quick and superficial, at some of the major studies on Greek dialectology from the beginning of the nineteenth century until now, at the methodology they followed and the aims they pursued. Historiography will help us to avoid the errors of past research and to clarify the nature of the current problems.

2. The history of Greek dialect studies from the beginning of last century at least (and probably earlier) can be seen as a constant tug-of-war between «classifiers» and «separatists». Ahrens, the official founder, occupies an intermediate position[1]; building on Strabo's classification of the Greek dialects in the 1830's and 1840's he marked the

---

(1) H. L. Ahrens, *De graecae linguae dialectis* I. *De dialectis aeolicis et pseudaeolicis* II. *De dialecto dorica* (1839 and 1843).

beginning of the *modern* tradition to group all dialects into three main divisions, Aeolic, Doric and Ionic. Under the Aeolic heading he gave separate descriptions for Lesbian, Boeotian and Thessalian, while for Doric he provided a unitary account pointing out, of course, the divergences between the various regional dialects.

The first chapter of Richard Meister's *Die griechischen Dialekte* (1882), which is meant as a second edition of Ahrens' *De Graecae linguae dialectis*, accepts an Aeolic classification similar to that of Ahrens but spells out some of the assumptions[2]. If the classification is correct there must have been, Meister argued (vol. I, p. 7), a period of Aeolic unity, i.e. a unitary linguistic community which spoke Aeolic at a period which is later than that of the Greek unity, and earlier than that of our evidence. Yet the linguistic features of the Aeolic of Asia, Boeotian and Thessalian are so different that there is no hope to produce a unitary description and these dialects must be treated separately. Meister's second and last volume (1889) offered separate descriptions of Elean, Arcadian and Cyprian, while emphasizing the close kinship of the last two dialects. Ahrens, who wrote before the decipherment of the Cyprian syllabary, had briefly discussed Elean and Arcadian under the heading of Pseudoaeolic dialects, based again on Strabo, and had concluded that Elean was closer to Doric than to Aeolic and that the same may have been true for Arcadian though the evidence was inadequate.

Otto Hoffmann's *Die griechischen Dialekte in ihrem historischen Zusammenhänge* appeared at Göttingen in three volumes between 1891 and 1897 in the wake of a colossal quarrel between its author and Meister based not only on drastic disagreements in the reading and interpretation of the texts, but also on serious theoretical differences[3]. As Hoffmann made clear in the preface to his first volume (p. III) he saw as the main task of Greek dialectology that of identifying in the multiplicity of the attested dialects the three underlying unities, viz. Aeolic, Ionic and Doric; to do this was of supreme importance both for an understanding of the history of the Greek language and for a reconstruction of the earliest history of the Greek tribes (*Stämme*). In Hoffmann's view newly available data allowed on the one hand to confirm Ahrens' (and Strabo's) intuitions about the three basic groups of dialects, on the other hand to move beyond them. While it was certainly possible to take back Thessalian, Boeotian and the Aeolic dialects of Asia to an Aeolic unity, it was also possible, according to Hoffmann, to show that Arcadian and Cyprian were much closer to Aeolic than to Doric, thus supporting Strabo's statement that the Peloponnese had been inhabited by Aeolians as well as by Dorians. Hoffmann concluded that it was best to speak of an «Achaean» group which was subdivided into Southern Achaean (Arcadian and Cyprian) and Northern Achaean (Boeotian, Thessalian, Lesbian). His arrangement of the data followed his principles : for each phonological or morphological phenomenon of each group it was stated whether it did or did not occur in the various dialects of the group and consequently whether it had to be attributed to the period of original unity. He mentioned, for instance, that North Achaean (i.e. the Aeolic dialects) used the inflection of the present for the perfect participle and then provided the Thessalian, Lesbian and Boeotian evidence for this phenomenon (vol. II, p. 565).

---

(2) R. MEISTER. *Die griechischen Dialekte auf Grundlage von Ahrens' Werk : « De Graecae linguae dialectis »*, 2 vols. (1882 and 1889).

(3) For the relevant texts see the references in the first volume of HOFFMANN, *op. cit.*, p. X-XIII.

2.1. In the background to some of these statements and decisions there is a controversy about the nature of linguistic kinship and reconstruction which in the 1870's and 1880's had engaged the attention of both the neogrammarians and their opponents. Should we represent the development of related languages in the form of a family tree (*Stammbaum*) perhaps with binary branching at all stages? This was the view that had been codified for Indo-European by August Schleicher in his *Compendium* (1861)[4]. Or should we assume that innovations in language, including those innovations which led to language differentiation, were due to a sort of wave effect? The change would have started in one place and from there diffused in ever increasing circles as when one throws a stone into the water. This was the model proposed by Johannes Schmidt in an 1872 pamphlet which had caused considerable stir[5]. When Wilamowitz argued in 1884 that in a description of Greek dialects the ordering must provide a «Stammbaum der Mundarten»[6], the young Hermann Collitz (b. 1855) reacted advocating the use of Schmidt's model for the Greek dialects[7]. In his turn the 23-year old Otto Hoffmann came to the rescue of the *Stammbaum* theory with his 1888 dissertation *De mixtis Graecae linguae dialectis* (Göttingen) where he reasserted the correctness of the *Stammbaum* approach and explained the peculiarities of some of the dialects as due to various migrations of speakers with different linguistic backgrounds into the same area. In other words behind the Meister-Hoffmann controversy there was more at stake than the classification of Greek dialects or the interpretation of some texts; the whole question of the validity of the contemporary principles of reconstruction and classification was involved.

Two decades later Kretschmer[8] firmly inserted Hoffmann's conclusions into the historical framework that has been either attacked or exalted ever since. The classical distribution of dialects, he argued, must be explained through three different migrations which took place in the second millennium : first the predecessors of the Ionians arrived and occupied most of the Peloponnese and Crete, as well as Central Greece. Then they were partly pushed away, partly subdued by an Achaean invasion. Finally, the return of the Heraclides, i.e., according to the ancient tradition, the arrival of the Dorians. With Kretschmer the emphasis is far more on historical conclusions than on the principles of reconstruction but the validity of the latter is a necessary precondition for that of the former.

2.2. Round the same period both Albert Thumb and Carl Darling Buck, who wrote shorter manuals on the Greek dialects at distance of one year from each other (1909 and

---

(4) A. SCHLEICHER, *Compendium der vergleichenden Grammatik der indogermanischen Sprachen* (1861-62).

(5) J. SCHMIDT, *Die Verwandtschaftverhältnisse der indogermanischen Sprachen* (1872).

(6) U. VON WILAMOWITZ-MOELLENDORF, Review of P. CAUER, *Delectus Inscriptionum Graecarum*[2], *Zeitschrift für das Gymnasial-wesen* 38 (1884), p. 105-116, at p. 113.

(7) H. COLLITZ, *Die Verwantschaftverhältnisse der griechischen Dialekte mit besonderer Rücksicht auf die Thessalische Mundart* (1885), p. 14 ff.

(8) P. KRETSCHMER, «Zur Geschichte der griechischen Dialekte», *Glotta* 1 (1909), p. 9-59.

1910 respectively)[9], had wise words to say about the problem of classification and its difficulties; yet in the arrangement of their material they went into opposite directions. Buck treated Greek as a unit and for each phonological or morphological feature of interest gave the various expressions of each dialect. Thumb considered each dialect on its own   partly as Meister had done in his first volume — and gave a brief account of its features. In reviewing Thumb's book in the *Journal des Savants* Meillet (1910, p. 60ff., p. 108ff.) complained about the repetitiveness of this method of exposition and about the loss of the clarification which the comparison with other dialects would have brought to the individual phenomena. There is another potential conflict here, which was already preannounced in the Meister/Hoffmann controversy, that between the linguist and the classicist. The former not only was eager, as Hoffmann had been, to reconstruct some earlier stages of the language, but also found it easier to explain a linguistic feature, any feature, if it could be diachronically compared with a similar feature in another language or dialect; the latter was more inclined to define the rules and characteristics of a specific speech system. He needed no comparison because he was not aiming at historical explanation but at description. At the same time, however, he was far more alert to nuances of expression than the comparativist.

The two attitudes came to the fore (together with an attempt at reconciliation) in the one man who could be trusted to see both sides of the question, Jacob Wackernagel. In a public speech — now completely forgotten — which he gave as Prorektor of Göttingen University in 1913[10], Wackernagel tryed to show to a general audience how great were the recent changes in the study of a traditional subject such as the Greek language and how much progress had recently been made through the combination of a strictly philological and a strictly linguistic approach. He concentrated at first on the Greek dialects. New data (above all epigraphic), new techniques which distinguished the linguistically archaic from the linguistically recent, a new understanding of how dialects develop due to the study of the modern varieties, all this conspired, according to Wackernagel, to create a far richer and more nuanced picture of the Greek dialects. «There were not three dialects, but rather thirty, each with its own peculiarities. These may be classified into groups, but there are bound to be overlaps and intersections». It is implicit in these statements a concern both for the general classification (synchronically and diachronically interpreted) and for the individuality of the dialects. As always, in Wackernagel the linguist and the classicist speak together.

It is no wonder then that when we turn to the next great work about dialects, *Die Griechischen Dialekte* by Friedrich Bechtel, appeared between 1921 and 1924 and dedicated indeed to Wackernagel, we find a new plan. This is not immediately obvious. In homage to Ahrens Bechtel has the usual subdivision of the material into volumes (the third volume is dedicated to Ionic which neither Ahrens nor Meister had covered) but at the same time he starts with a proud declaration that the book is organized so as to offer a description of each dialect with its specific characteristics (vol. I, p. v). The point is

---

(9) A. THUMB, *Handbuch der griechischen Dialekte* (1909); C. D. BUCK, *Introduction to the study of the Greek Dialects* (1910).

(10) «Rede des Prorektors Jacob Wackernagel», in *Bericht über die am 16. Juni 1913 abgehaltene Jahresfeier der Georg-August-Universität und die damit verbundene Feier des XXV-jährigen Regierungsjubiläum Seiner Majestät des Kaisers und Königs Wilhelms II* (1913), p. 3-25, at p. 7f.

reemphasized with reference to the Doric dialects : « My exposition differs from the fundamental work of my predecessors, because ... I have described each dialect not only in its relationship with the others but also in its individuality so that the various trends ... are reunited in one picture» (vol. II, p. v). And Bechtel continues explaining that the new epigraphical evidence has made it possible to see in the dialects real individuality. The volumes themselves then describe each dialect on its own, listing and explaining (historically) its phonological, morphological, syntactical and lexical features. Not a word is said about classification or about prehistorical reconstruction. Indeed the first volume, which contains descriptions of Lesbian, Thessalian, Boeotian, Arcadian and Cyprian, has no general title, no reference to Aeolic or Achaean. Bechtel is the archetypal «separatist»; he ignores both Hoffmann's reconstruction and Kretschmer's prehistory.

3. In the tug-of-war that I have mentioned at the beginning Hoffmann and Bechtel represent opposite sides. Do we work on dialects in the interest of diachronic classification, reconstruction, and consequently prehistory or do we work on dialects in the interest of discovering how the individual forms of Greek functioned within their system, developed through history and perhaps influenced each other? The conflict need not be total, as Wackernagel knew, but is certainly real and has been important in the last thirty years or so of scholarship. The decipherment of Linear B has provided us with the first linguistic data we have for the second millennium; needless to say the need to insert them in the general picture which had previously been reconstructed has absorbed some of the best minds. The result is that an immense amount of energy has been dedicated to the question of dialect classification in the second millennium, i.e. in the period for which, in spite of Linear B, we have little evidence and for which definitive conclusions are difficult to reach. Far less effort has been spent on the problems of dialect description and development, though the data, i.e. the epigraphical material, have been increasing at a rate which is almost frightening [11].

The most recent manuels that we have (none of which is so recent any longer) are the latest editions of the manuals by Thumb and Buck first appeared at the beginning of the century [12]. Bechtel's *Griechischen Dialekte* is still the largest text-book available and is desperately out of date. There have been rumblings. While the Greek dialectologists debated about classification, the methodology of linguistic description had drastically changed and, as I have mentioned, the actual data had multiplied. Bechtel built his description of Cyprian mainly on one inscription. Others existed but editions and interpretations were unreliable. Masson's corpus, first published in 1961, had even then more than 400 texts; recent discoveries have added more than one hundred further documents [13].

---

(11) Needless to say this is an oversimplified account. Among other things it is strongly at fault because it ignores the important attempts at synchronic and historic (rather than, but also as well as, diachronic and prehistoric) classification made by A. Bartoněk in a series of books and articles from the early 1960's; see the account by BRIXHE *et al.*, *REG* 98 (1985), p. 267 ff. and *REG* 101 (1988), p. 75 ff.

(12) C. D. BUCK, *The Greek Dialects* (1955 [with later reprints]); A. THUMB, *Handbuch der griechischen Dialekte, Erster Teil, Zweite erweiterte Auflage von* E. KIECKERS (1932); A. THUMB, *Handbuch der griechischen Dialekte, Zweiter Teil, Zweite erweiterte Auflage von* A. SCHERER (1959). There are of course other manuals which fulfil very useful services but they are all too limited in size to play a role in this discussion.

(13) *ICS.*

Thirty years ago in a fit of youthful arrogance I reviewed the second edition of Thumb revised by Scherer (1959) accusing author and reviser of having completely ignored the progress of modern linguistics[14]. What I was asking for — somewhat too obstreperously — was simply a structural description of each dialect at the various periods of its history. I accused — correctly — Bechtel and Thumb of merely producing lists of individual facts without giving the reader any feeling for what e.g. the Boeotian or the Thessalian linguistic systems were like. Yet I was too ignorant to realize how innovative Bechtel and Thumb were in their decision to analyse each dialect separately and I did not understand that to attack Bechtel and Thumb also meant to attack that «separatist» camp which I wanted to join. In fact I was not asking for a revolution, as I thought, but simply for increased methodological awareness and sophistication. More recently, in an article entitled «Les dialectes grecs, ces inconnus», Claude Brixhe and two of his pupils have deplored the «obsession comparative» (p. 161) of most modern dialectologists and their insistence in using the same few facts for endless and futile discussion about the classification of the second millennium[15]. At the same time they have pleaded for an epistemological break, a severance from the ideological ghetto in which these studies have ticked away for some time (p. 176, 177), the acknowledgement that we need a serious philology, a recognition that each dialect is a system and therefore odd isolated facts are not sufficient to define it, an awareness of the new results of sociolinguistics, etc. There is much that is correct in this criticism and perhaps something that is overemphasized. There is also much faith in what can be obtained with limited evidence. Yet, as a *cri de guerre* it is worthwhile and ought to be listened to.

4. It would not be worthwhile to rehearse here this potted history of Greek dialectology if the new data did not allow us to see the old disagreements in a new light. The main problem has always been whether the «classifiers/separatists» controversy is merely a matter of temperamental differences or whether one of the two sides has the monopoly of the truth. Nobody would deny, I believe, that the «separatists» do sterling work at least in so far as they interpret texts and collect data[16]. The question is rather whether the work of the «classifiers» has any validity at all. An answer will depend on the way in which we see dialect reconstruction. If it can be shown that the standard techniques of reconstruction, as developed e.g. for the Indo-European languages, cannot be applied to dialects which are in continuous contact and are prone to regular interdialectal contamination, all the work of the «classifiers» must be rejected. If, on the other hand, dialect-based reconstruction leads to plausible results we must accept that part at least of what the «classifiers» do is sound. But how do we decide?

---

(14) A. MORPURGO, Review of *ICS*, *PdP*, fasc. 75 (1960), p. 458-470.

(15) M. BILE, C. BRIXHE, R. HODOT, «Les dialectes grecs, ces inconnus», *BSL* 79 (1984), p. 155-203. Cf. also C. BRIXHE, *Verbum* 10 (1987), p. 275-289; but see also the analysis of the current work on dialectology by E. RISCH, *Kratylos* 34 (1989), p. 78.

(16) The objections that have been raised do not challenge the validity of the method but its impact. Risch (*loc. cit.* in note 15) makes this clear : «Dagegen escheint mir die öfter zum Ausdruck kommende Tendenz, einen Dialekt für sich allein zu betrachten und seine Veränderungen aus sich heraus zu begreifen, zur Sammlung des Materials zwar sehr nützlich, letzen Endes aber nur in einem beschränkten Rahmen sinnvoll».

4.1. I propose to make amends for my earlier arrogance by arguing, against a strong separatist position, that, at a simple level at least, a form of dialect comparison that aims at reconstruction is *a)* possible and *b)* fruitful. To support the first point I simply mean to show that we have evidence which confirms the validity of our dialect reconstruction. For the second point I shall make my own a well known argument used by all comparativists. Reconstruction inevitably leads to a chronological and sometimes geographical increase of linguistic data; in their turn these new data may lead to an improved historical understanding of the attested linguistic features. In what follows I shall try to validate both these points with the help, *inter alia*, of Linear B data and the data of other dialects.

5. My choice of evidence for the alphabetic period is unexciting. It falls onto the Arcadian and Cyprian dialects for a number of reasons which will become apparent as I proceed. The first of these, however, must be mentioned now and is the weight of the tradition. A part from Ahrens, who wrote before the decipherment of Cyprian, none of the dialectologists I mentioned so far has ever doubted the close link between these two dialects. Meister in his second volume (*op. cit.*, vol. II [1889], p. III) spoke of Cyprian as «nah verwandte» to Arcadian, Hoffmann (*op. cit.*, vol. I [1891], p. VI) said that the language of the newly deciphered Cyprian texts overlapped remarkably with that of Arcadian. In 1910 Buck (*op. cit.*, p. 6) wrote that «No two dialects, not even Attic and Ionic, belong together more obviously than do those of Arcadia and Cyprus. They share in a number of notable peculiarities, which are unknown elsewhere»; the same sentences reappear with a minor modification in the 1955 edition (p. 7). Even the «non-classifying» Bechtel (*op. cit.*, vol. I [1921], p. 400) speaks of Arcadian and Cyprian as «nächstverwandte».

What do we know about the two dialects? Arcadian in the archaic and classical period was written (and presumably spoken) in the central mountainous zone of Peloponnese. It was a rough area with the few major centres in the eastern plain constantly under the danger of attack from Argos or Sparta. There was no political unity though Mantinea, Tegea and Orchomenos in the sixth century were obliged to join the Peloponnesian league. In the fourth century an Arcadian *koinon* helped by Epaminondas had a brief *floruit* but then the area came under Macedonian influence. We have dialect inscriptions (not many) from the sixth century to the late third/early second century, though *koine* and *koina* are influential as early as the late fourth century.

Cyprus is different. There is archaeological evidence for the arrival of new populations in the island in the twelfth and eleventh centuries and the creation of new urban centres in that period; there are Phoenician settlements from the end of the ninth century. The independent kingdoms of Cyprus had to undergo first Aegyptian and then Persian domination from the sixth century but political and commercial contacts with Greece were frequent. In the archaic and classical periods at least three languages were spoken in the island : Phoenician, Greek and the so-called Eteocyprian, a local language about which we know almost nothing. There may have been more. Three scripts were used : *a)* Phoenician used for Phoenician language, *b)* Syllabic Cyprian used mostly for the local Greek dialect and rarely for Eteocyprian, *c)* the Greek alphabet, hardly ever used at least in the documents we have (there is no local Greek script), but ready to take over from the third century onwards. The syllabic script was in use for the local Greek

dialect, as we now know, from as early as the eleventh century B.C. (though the bulk of the documents starts much later) and remained the dominant script until the third century B.C. We know that it is related to Linear B and that in all likelihood is descended from one of the still undeciphered Cypro-Minoan scripts of the second millennium. Why and how the variant used for Greek was so successful and persistent remains a mystery — did it identify the locals against the Phoenicians, as has been suggested, or against the Greeks?

If it is indeed the case that Arcadian and Cyprian share a greater number of features than chance would allow, the only plausible explanation is the traditional one : Greek migrants from the Peloponnese reached Cyprus in the twelfth century or so at a stage when the ancestors of the Arcadians were probably not yet confined to Arcadia. This means, in traditional terms, that if we applied the standard techniques of comparison and reconstruction to the two dialects and if these did work we should be able to reconstruct the main features of a language spoken in Peloponnese just before the departure of the future Cyprians. Yet we also know, thanks to the Linear B evidence, that at that date or some decades earlier part at least of the Peloponnese was occupied by Mycenaean speakers. Hence we may formulate the (admittedly naif) hope that a comparison of Arcadian and Cyprian and a reconstruction of their common core should identify a set of linguistic features which need not be identical to, but ought to be compatible with, those of Mycenaean as we know it. If this were to happen, the validity of our techniques of reconstruction would be confirmed by a concrete piece of evidence [17].

6. Let us then list, once again in a traditional manner, the features shared by the two dialects, excluding, however, those features which are common to all Greek dialects. A similar analysis was undertaken quite recently by the late Prof. Risch and by Dr. John Chadwick at the Larnaca Symposium of 1986 [18], though neither of these two authors had the same purpose as I do or used the deliberately unsophisticated approach that I am trying out. I shall make use of their work but my aims and methods are different. I start by listing in table I the relevant isoglosses with some information in brackets ({ }) meant to show that the features in question are not found in all dialects.

TABLE I

Main Arcado-Cyprian isoglosses

Phonology

1. *-ti* > *-si* in Arc. (Τριαχάσιοι), Cypr. (*kasignēto-*) {Attic but not Doric}
2. *-o* > *-u* in Arc. (ἐγάμαντυ, ἄλλυ), Cypr. (*genoitu*) {not Attic}

---

(17) I can hardly stress enough how simplistic is the technique that I adopt. *Inter alia*, we would normally expect a valid reconstruction to be based on the comparison of at least three languages or dialects. Yet in what follows I have normally avoided all references to Pamphylian, the third dialect which has often been quoted in this connection. The evidence is too difficult and too limited to be of much use for my purposes.

(18) J. CHADWICK, «Differences and similarities between Cypriot and the other Greek dialects», *The History of the Greek Language in Cyprus* (J. KARAGEORGHIS & O. MASSON ed., 1988), p. 55-61; E. RISCH, «Le développement du chypriote dans le cadre des dialectes grecs anciens», *ibid.*, p. 67-77.

3. Apparent oscillation between *a* and *o* vocalism in the treatment of *ṛ, *ḷ, *ṃ, *ṇ (Arc. hεκοτόν, τέτορτος, Τριακάσιοι, ἔασα, τέταρτος; Cypr. *duwiyakasias, amata, katerworgon*) {fewer or no oscillations in Attic or Doric}

Morphology

4. Gen. sing. of *a*-stems masc. : *-ao > -au* in Arc. (Δεινίαυ) & Cypr. (*Onasagorau*) {most dialects *-ao* or derivatives; Attic *-ου*}

5. Dat. plur. *-a*-, *-o*-stems : Arc. & Cypr. *-αις, -οις* {Ionic & early epigraphic Attic : *-αισι, -οισι*}

6. Acc. sg. *s*-stems : *-εα* (rare) & *-ην* in Arcadian (Δαμοτέλην), *-ēn* in Cyprian (*atelēn*) {most dialects : *-εα* or contractions}

7. Nom. sg. *eu*-stems : *-ēs* in Arc. (ἱερής), rare *-ευς*, *-eus* in Cyprian (*basileus*), rare *-ēs* (*ijerēs*)

8. Nom. plur. article : Arc. & Cypr. οἱ {Doric τοί}

9. Demonstrative : Arc. & Cypr. ὄνυ (and other pronouns) {ὄνυ absent from Attic & most other dialects}

10. Third pers. sg. middle : *-toi* (Arc. βόλετοι, Cypr. *keitoi*) {other dialects : *-ται*, Thess. *-τει*}

11. Third pers. pl. act. : *-an* replacing *-n < *-nt* as in Arc. ἀνέθεαν (but also ἄνεθεν), Cypr. *katelhijan* {other dialects : *-ν* or *-σαν* or *-αν*}

12. Athem. infin. in *-(e)nai* (Arc. δῶναι, Cypr. *do(w)enai*) {*-μεν, -μεναι* in Dor., Aeol.}

Preverbs, prepositions, conjunctions, lexicon

13. Arc. ἀπύ, Cypr. *apu* {Attic ἀπό}

14. Arc. ἰν, Cypr. *in* {Attic ἐν}

15. Arc. ὀν-/ὐν-, Cypr. *on-/un-* [in compounds] {Att. ἀνά}

16. Arc. πός, Cypr. *pos* {Attic πρός}

17. Frequent apocope of prepositions {absent in Attic}

18. Arc. κάς, καί, Cypr. *kas* {other dialects : καί}

19. *-te* adv./conj. : Arc. τότε, Cypr. *ote* {Att. *-τε*, Dor. *-κα*}

20. Archaic «epic» words (Arc., Cypr. αἶσα, etc.)

21. Arc. ἱερός, Cypr. *(h)iero-* {Dor. ἱαρός, Lesb. ἶρος}

Syntax

22. ἀπύ, ἐξ normally construed with the Dative in Arc. & Cypr. {Other dialects, genitive}.

6.1. Though we want to avoid at this stage any hint of *ad hoc* pleading or of unwarranted sophistication in the manipulation of data, a few observations are necessary.

First, three points of a more general nature.

a) The list deliberately does not distinguish between innovations and archaisms; we are aiming at reconstruction rather than at classification and any reconstructed language must have included both archaisms and innovations.

b) The choice of the material included in the lists is cautious and perhaps overcautious. In view of the forms of the gen. sing. thematic Arc. *-ω*, Cypr, *-o* it would be possible to argue, for instance, that the contracted forms go back to Arcado-Cyprian. Yet we do not know if Arc. *-ω* matches exactly Cypr. *-o* or represents a different type of contraction. If the latter were true, then it could be argued that the contractions ought not to be attributed to the second millennium. This is a conclusion that is also supported by more general evidence for these and other dialects.

c) The decision taken earlier, more for sake of simplicity than for other reasons, not to include the features that are common to all Greek dialects, obviously removes a certain amount of data from the discussion but I doubt that it is going to alter substantially our results[19].

Other points are more specific and concern the features listed above. I quote them here with their earlier numeration.

(3) The oscillation between *a-* and *o-*vocalism in the treatment of vocalic liquids and nasals need not be significant since it is difficult, if not impossible, to prove that the nature of this oscillation is the same in Arcadian and Cyprian. Also, it is not altogether clear whether a similar oscillation does or does not occur in the whole of Greek.

(4) The morphological isogloss is provided by the genitive in *-ao*; the final *-u* is due to the change listed in 2.

(6) and (7). The *-ēs* nominative of the *eu-*stems is hard to assess largely because *-ēs* and *-eus* coexist in the individual dialects and we do not know how the change happened; as has often been argued, an earlier inflection with nom. *-eus* and acc. *-ēn* may have led to a new *-ēs* nominative which replaced *-eus*. If so, the common form to be reconstructed would be the accusative and not the nominative[20]. In general whenever we have both the old (or supposed old) and the new form in the dialects it seems difficult to argue that the innovation goes back a long time. This is true for -ευς vs. -ης in the *eu-*stems and for the acc. -εα vs. -ην in the *s-*stems. See also (11) below.

(11) If Arcadian has both -εθεν and -εθεαν forms, it is not easy to establish whether the latter is in fact so old that it must be a shared innovation with Cyprian, though this is possible[21]. See above (6) and (7) for a similar problem.

(13) Since Mycenaean has *a-pu* but no other signs of the change of a final [o] to [u] it is normally assumed that the contrast *apo/apu* is morphological and is not due to the *-o* > *-u* change mentioned above in (2) (see also note 32).

(17) The rules of apocope are extremely uncertain which again means that we ought to hesitate before reconstructing apocope for the earlier language.

6.2. With these observations in mind we can then reconstruct for the second millennium a dialect with the features listed in Table II :

(19) We should note however that this decision removes some potential awkwardness. A purely mechanical comparaison would lead, for instance, to the reconstruction of a form like *leipomenos* not only for the ancestor of Arcadian and Cyprian but for common Greek as well. If we ignored the evidence from Linear B, only a very complicated set of arguments could lead us to attribute to any stage of Greek a labiovelar before an *o*-vowel, given that all dialects have replaced it with a labial stop. The difficulty here is a serious one but must be discussed in the context of an analysis of the concept of common Greek.

(20) The best brief account of the problems and of the possible antecedents of these forms in Mycenaean is found in E. RISCH, « Die mykenischen Personennamen auf *-e*», *Tractata Mycenaea*, p. 281-298, esp. p. 293 f.; for the Arcado-Cyprian data and the earlier literature cf. also A. LILLO, *Glotta* 61 (1983), p. 1-4. As Risch points out, the suggestion that forms like Myc. *e-re-(de)* and *ma-se-(de)* represent accusatives of *-eu-*stems is doubtful (the nominative is not available). Mycenaean nominatives like *ka-ke* could indeed be the equivalent of Arcadian ἱερής, but they are found in personal names and not in *Appellativa*. In general this evidence is very difficult to use. Moreover those who have discussed it tend to forget that, outside Arcado-Cyprian, inscriptions on Greek vases offer a number of *-es* nominatives where *-eus* would be expected; these too call for an explanation (cf. P. KRETSCHMER, *Die griechischen Vaseninschriften* [1894], p. 191 f.).

TABLE II

Second millennium features

(1)   *-si < -li*
(2)   *-u* from *-o*
(4)   *-ao* or *-au* genitives
(5)   *-ois, -ais* dative plurals
(8)   *hoi* nom. pl. of the article/pronoun
(9)   Λ *honu* demonstrative
(10) *-toi* verbal forms
(12) *-(e)nai* infinitives
(13) (14) (15) (16) (18) The prepositions *apu, in, on*(V), *pos*, and the conjunction *kas*
(19) Temporal adverbs in *-te*
(21) A form *(h)ieros*
(22) Ablatival prepositions (*apu, ex,* etc.) construed with the dative

7. How does this reconstruction match with Mycenaean? What we are aiming at is *not* a demonstration that reconstructed Arcado-Cyprian is Mycenaean but rather the confirmation (or otherwise) that a reconstructed Arcado-Cyprian is compatible with the almost contemporary Mycenaean in terms of its developmental trends. If this is the case we obtain a positive indication of the value of our reconstruction; if it is not it does not follow that the reconstruction is wrong. An alternative interpretation would be that the dialect of Linear B does not belong — in terms of dialect classification — with the ancestor of Arcado-Cyprian. This is not simply a way to say that we cannot lose; a comparison of reconstructed Arcado-Cyprian and Linear B *either* provides us with positive evidence in favour of the validity of our techniques *or* leaves us at the *status quo,* i.e. at the stage of legitimate agnosticism favoured by the moderate wing of the «separatists».

In a number of instances the evidence is simply not there : we do not know about the Mycenaean forms of the article/pronoun (8) or the demonstrative pronouns except for a few (9), or the athematic infinitive (12) or the forms of καί (18) [22]. We find, however, the correspondences listed in Table III :

TABLE III

Mycenaean and Arcado-Cyprian correspondences

(1)   *-si* from *-ti*
(4)   *-ao* genitives
(10) *-toi* verbs

---

(21) For a general discussion see L. DUBOIS, *Recherches sur le dialecte arcadien* I (1986), p. 154 ff. (with earlier bibliography).
(22) The regular conjunction in Mycenaean is *-qe*, Gr. *-τε*. We do have, of course, compounds of *ka-si* which must be related to κάς.

(13) *a-pu* preverb/preposition

(16) *po-si* preverb/preposition vs. Arc.-Cyp. *pos.*

(19) *o-te* «when»

(21) *i-je-ro*

(22) Perhaps the construction of a preposition (Myc. *pa-ro*) with the dative instead than with the gen. (?)[23].

7.1. The next question is whether there are Arcado-Cyprian isoglosses which would lead us to reconstruct for the second millennium forms which differ from, or are incompatible with, the Mycenaean forms.

The answer is «yes», but a caveat is necessary : of the remaining unlisted items of table II, no. 5 need not be relevant here. Admittedly, the Arcadian and Cyprian dative plurals of the *-ois, -ais* type contrast with the Mycenaean *-o-i, -a-i* type, if this is correctly read *-oihi, -aihi*; the Mycenaean forms are far closer to the later *-oisi, -aisi* of other dialects. And yet there is sufficient evidence for a Mycenaean instrumental plural in *-o,* i.e. *-ois,* which is identical to the form preserved as *-ois* in the first millennium. The *-ais* dative of Arcado-Cyprian may or may not correspond to an *-ais* of Mycenaean, but in any case must owe its existence to analogy with *-ois* and causes no problems[24]. If we then leave no. 5 aside, we are left with the forms of Table IV :

TABLE IV

Mycenaean and Arcado-Cyprian divergences

(2)   Mycenaean seems to preserve final *-o* (*su-qo-ta-o, de-ka-sa-to, de-ko-to*) without changing it into *-u* as Arcadian & Cyprian do

(14) Mycenaean has *en* (*e-ne-e-si*). where Arcadian & Cyprian have *in*

(15) Mycenaean has a preverb *an(a)* (*a-na-ke-e, a-ne-ta-de*) where Arcadian & Cyprian have *on-* or *un-* (as well as *an-*) for Attic ἀνά

Absence of evidence may conceal other divergences (or, of course, other correspondences). Of those that I listed above the most enigmatic is (15), the form of the preverb/preposition ἀνά which in Mycenaean has the *an-* form while in Arcadian and Cyprian we find *an-* as well as *on/un-*. Given the presence of *an-* in Arcado-Cyprian too perhaps the form should not be listed here, but *on-/un-* are puzzling. The problem is that quite simply we do not know the etymology of the attested forms nor do we see how they can be related[25].

The other two «divergences» concern two phonetic changes of Arcado-Cyprian not matched in Mycenaean : *a)* final [o] becomes [u] in Arcado-Cyprian with the first evidence

---

(23) This is a very doubtful item; we have no clear evidence for the construction of *a-pu* in Mycenaean and ἐξ is not attested in prepositional function. Admittedly *pa-ro* (Att. παρά) is always construed with the dative but we cannot be certain that these datives would correspond to the genitive of a non-Arcado-Cyprian dialect.

(24) The interpretation of Myc. *-o-i* as *-oihi* is not accepted by everyone; cf. e.g. C. J. RUIJGH, *SMEA* 20 (1979), p. 82 ff. The best case for it is still that made by *Mémoires* III, p. 255-266, who also find in Mycenaean one example of *-a = -ais* (*ibid.*, p. 264). See below for the Arcadian datives σφεις and σφεσιν and their importance for the interpretation of the *-o-i* datives of Mycenaean.

(25) For the evidence and the nature of the problem there is little to add to the statements by DUBOIS, *op. cit.* I, p. 23-25 with the relevant footnotes which also discuss the Cyprian data.

appearing in the eleventh century B.C. (*o-pe-le-ta-u* in Cyprian : *ICS* 18g); *b)* the [en] sequence becomes [in], first of all in the preverb/preposition but elsewhere as well. Phonetically both changes are common enough and we would not be surprised to find oscillations (determined by geographical or sociological factors) even in the same dialect ; hence the divergence between Myc. final *-o* or Myc. *en-* and reconstructed Arcado-Cyprian final *-u* and *in* need not be disturbing. However, there may also be reason to believe that the position of reconstructed Arcado-Cyprian has been somewhat overstated[26].

   To start with the first change. Contrary to what is often said, neither in Arcadian nor in Cyprian all instances of final [o] turn into [u] : in Cyprian next to *e-u-we-re-ta-sa-lu*, *o-na-sa-ko-ra-u*, etc., we have a few instances of final *-to* in e.g. *teles(s)ato, ekhrato* (*ICS* 306 : fifth/fourth century), *ku-pa-ra-ko-ra-o* (*ICS* 357), etc. Also cf. *po-ro* if = *pro* (*ICS* 264) and the forms of the article/pronoun *o* (*passim*) and *to* (e.g. *ICS* 92 add., 140, 318 B V 1, etc). In Arcadian next to the *-τυ* (ἐξόλοιτυ, etc.) and *-αυ* endings and the neuter ἄλλυ, we have ὁ, τό, αὐτό, δέκο, δυόδεκο ; other *-o* forms may be due to koine influence[27]. Dubois (*loc. cit.*) assumes that the change is limited to unaccented final [o] and seems inclined to find together with Risch and Lillo an explanation of the *-o* of δέκο in the fact that this vowel derives from a vocalic nasal[28]. In his view the Arcadian change goes back to the period which preceded the Cyprian migration. In fact the change may have started then but it is likely that it developed slowly and by degrees. If it was generalized early — even if limited to unaccented position[29] — it would be impossible to understand the Arcadian and Cyprian genitives of the *-ω/-ō* type ; we would expect an earlier *-oyu* or *-ou* (from *-oyo* or *-oo*) to yield an [ou] diphthong and not a long vowel[30]. It is also likely that after [u]/[w] final [o] would have been preserved

   (26) What follows (and indeed what precedes) was written before I read Claude BRIXHE's article about «Morphologie ou morphographémie en grec ancien», *BSL* 84 (1989), p. 21-54, where the problem of the *-o > -u* change (or alternation) is thoroughly discussed (p. 39-48). Since I agree with large part of Brixhe's conclusions and of his criticism of his predecessors, I thought it might be useful by way of support to leave my text in the original form though I have added in square brackets the relevant references to Brixhe.

   (27) For the evidence in Arcadian and Cyprian cf. DUBOIS, *op. cit.* 1, p. 26-28 with the footnotes and references to earlier literature. One hesitates to add to the dossier Arc. ὑπο-, that may be due to the *koina*, ὀπύ, which may or may not be the dialect equivalent of ὑπό, and κατυ-, which may or may not be based on *κατο-. [Cf. also BRIXHE, *loc. cit.*, who mentions for Arcadian too προ- found in compounds].

   (28) For the different ways in which the link between *o*-vocalism and the vocalic nasals has been explained see A. LILLO, *Studia philologica salmaticensia* 4 (1980), p. 165-167. [BRIXHE, *loc. cit.*, offers a convincing criticism of both hypotheses].

   (29) I am not convinced that the evidence warrants a distinction between the treatments of final accented and final unaccented [o]. DUBOIS, *loc. cit.*, refers to Arc. τό and αὐτό as contrasted with ἄλλυ, but it is likely that τό was proclitic, possibly even when used pronominally (as were the nom. ὁ and Cyprian *pro*) ; αὐτό may well be analogical on the article/pronoun. [Cf. BRIXHE, *loc. cit.*, for a convincing statement about the non-relevance of accent].

   (30) LILLO, *loc. cit.*, believes that the genitive form is analogical on the article ; this, being proclitic, would not have undergone the *-o > -u* change. Yet, the influence of the article (*qua* article) must be relatively late and we might have expected some traces of an *-ou* genitive — all the more so since this would have been supported by the *-au* genitives. If we establish our chronology on the basis of Mycenaean (which may of course lead us into a vicious circle) we could argue that in the second millennium final [o] is not raised after [y] and consequently *-oyo* is preserved. However Arcadian has ἄλλυ which must go back to a reconstructed Arcado-Cyprian *alyo(d), since Cyprian has *ailos*. This may either show that the rule is wrong or that the [u] pronunciation of a final back vowel became more widespread at a later stage.

e.g. in δύο and perhaps in an early *en(n)ewo* «nine», if such a form existed in Arcado-Cyprian as well as in Mycenaean [31]. In other words, the *-o* > *-u* change may have started in the second millennium in well defined but limited environments and have then acquired a wider distribution at a later stage, when the two dialects were already separated [32]. Pamphylian, where the change has spread even further (e.g. to final *-os* > *-us*), may provide support for this view. We cannot of course be certain that the *-to* and *-ao* forms of Cyprian are archaisms rather than innovations, but this possibility should not be excluded.

The *en* > *in* change does not seem very different in nature. The data have been collected by Dubois, *op. cit.* 1, p. 17 ff. Both in Arcadian and in Cyprian the preposition/preverb ἰν regularly has this form in contrast with the Myc. preverb *en-* [33]. In addition both Arcadian and Cyprian show examples of an internal change of [en] to [in], though in neither dialect is this fully generalized or fully predictable. It is possible that the oscillations reflect a neutralization of /e/ and /i/ before nasal and consequent spelling hesitations. Once again it is likely that originally the preverb/preposition shifted to an [in] pronunciation in specific environments and the [in] form was generalized from there (in spelling and/or pronunciation).

8. It seems at this stage that our exercise in reconstruction has been successful. We reconstructed a series of forms which we want to attribute to the Peloponnese of the last quarter of the second millennium. When the evidence is available these forms to a large extent overlap with the Mycenaean forms actually attested in that region approximately in that period or slightly earlier.

It may be objected to this particular type of reconstruction that it does not contribute definitive results for the purposes of classification. Indeed the objection has been raised (Chadwick, *op. cit.*, p. 61f). In Mycenaean and Arcado-Cyprian there is no feature that belongs exclusively to these three dialects and is not an inherited Common Greek feature. The only isogloss of this type is the *-toi* ending of third pers. sing. middle which is now widely believed to be an inherited ending replaced by *-tai* in the other dialects. If my aim had been to show that there was a special branch of the Greek family tree which included Mycenaean, Arcadian, and Cyprian only, then the objection would have been valid (though even this would require further discussion). However, my aim

---

(31) If *en(n)ewo* did indeed exist in early Arcadian we could explain the final vocalism of δέκο as analogical on that of the numbers two, nine and possibly eight. This could explain both the choice of a back vowel and the retention of [o]. [BRIXHE, *loc. cit.*, rightly draws attention to the importante of δέκοτος and to the fact that in the ordinal the [o] was not final and consequently not subject to raising].

(32) We might want to argue that, in spite of what was said earlier (p. 424), Myc. *a-pu*, Cypr. *apu* and Arc. ἀπύ represent the first onset of the change. A rounding of the final vowel after labial stop would not surprise and the fact that ἀπύ occurs in Aeolic too need not speak against this view. In other words, final [o] could have changed to [u] after labial in a wide range of dialects, including Mycenaean, but, except for Arcadian, Cyprian and Pamphylian, the change would have been limited to this environment. [A similar suggestion is made by BRIXHE, *op. cit.*, p. 39, but without reference to phonetic conditioning]. On the other hand the existence of Mycenaean *u-po* speaks against this view as of course would that of Arcadian ὑπο- if this were the real dialect form (see above).

(33) For the Mycenaean evidence for ἐν see J.-L. PERPILLOU, «ἐν, ἐς, ἐξ en mycénien?», *Tractata Mycenaea*, p. 267-279.

was simply to show that, in some instances at least, it is possible to use the comparative evidence of more than one dialect to reconstruct an earlier dialectal phase and that the reconstructions reached in this way have some validity. I do not see that this can now be denied.

9. I argued earlier that reconstruction can be *a)* valid, *b)* fruitful. I shall conclude with an example of *b)* which the «separatists» may perhaps find more congenial than what I have been discussing so far. Both conclusions and data are well known but I hope to use them to carry my point : reconstruction does actually help to understand attested forms, their links and their background.

In an Arcadian inscription of the fourth century (Tegea, *IG* 6, 10.18) there are two unique instances of the dative of the reflexive pronoun of third person plural, σφεις :

l. 18 ...ζαμιόντω οἱ ἐσδοτῆρες ὅσαι ἂν δέατοί σφεις ζαμίαι
«let the *esdoteres* fine them with whatever fine seems to them suitable».

The context is clear and it is not difficult to establish that the form is related to Attic σφεῖς, dat. σφισίν. But how do we explain it? The etymology of the reflexive is not altogether clear and among the various hypotheses it has been suggested either that the basic form is **sphe* to which the endings have been added or that the basic form is a dative **sphei* (cf. Oscan *sifei*, Lat. *sibi* < **s[w]ebhei*) which was remodelled into a plural-looking form[34]. Yet the very isolation of the form was puzzling. It was only after the decipherment of Linear B that a parallel was found. This is the pronoun dat. pl. *pe-i* of Pylos which occurs as a dative of advantage in **PY Na 395** and after a preposition in the *o-ka* tablets, always in formulae of the type :

PY An 654 *me-la-qe pe-i e-qe-ta , ⋮ a-re-ku-tu-ru-wo e-te-wo-ke-re-we-|i-jo* ,
«and among them (is) the ἑπέτας Alektruōn son of Eteoklēs».

There is, however, a difficulty ; in Linear B normally the second element of a diphthong is not written and consequently a form like *pe-i* should be disyllabic ; a hiatus is warranted if something has been lost and the obvious candidate is an inherited *-s-*. If so, the parallelism of the Arcadian form with Mycenaean may not be perfect and its origin is even more obscure[35]. A recently published inscription from Mantinea of the beginning of the fourth century helps us to reach a solution.

Mantinea, Treaty with Helisson (Te Riele, *BCH* 111 [1987], p. 167-190) :

l. 15 ...κύριά σφεσιν ἦναι κὰ τὸς νόμος...
«let (the agreements) be valid for them according to the laws».

---

(34) Cf. *Griech. Gramm.* I, p. 601.

(35) Some of the suggestions found in the earlier literature are discussed by DUBOIS, *op. cit.* I, p. 123 f. Mycenaean *pe-i* has been read as *spheihi* or *sphehi* or *spheis* ; the last two interpretations go back at least as far as the first edition of *Documents*, p. 87. A case for *sphehi* was made in *Interpretation*, p. 50 and reiterated by MILANI, *Aevum* 39 (1965), p. 408-413. To explain the Arcadian σφεις Dubois, before the discovery of σφεσιν, thought of the agglutination of two particles *i* and *s* to *sphe*. The new data make Waanders' suggestion quoted below more plausible and speak for a *sphehi* reading of the Mycenaean form.

Paradoxically the existence of three equivalent forms (Myc. *pe-i*, Arc. σφεις and Arc. σφεσιν) does not confuse but clarify the picture. Mycenaean *pe-i* must be read *sphe(h)i* as was suggested long ago : we are dealing with an early dative/locative plural formed by adding the ending -*si* to a basic stem *sphe* (which is also found in the nom. σφεῖς). At a pre-Mycenaean stage intervocalic -*s*- was changed into -*h*-. Mycenaean may or may not have preserved this *h* but in any case the Linear B syllabary would not have written it. As was suggested by F. M. J. Waanders in his appendix to the first edition of the text (*BCH, loc. cit.*, p. 190), the Arcadian pronouns can be explained if we assume that they derive from a form similar to the Mycenaean one. After the loss of intervocalic *h* the dative **sphehi* must have become opaque and was remodelled in one of two possible ways. On the one hand the -*si* ending of the athematic dative plural was reintroduced analogically into the pronoun's inflection ; on the other the inherited form (**sphei* < **sphehi* < **sphesi*) was preserved and remodelled with a plural termination -*s* (for which see e.g. the -*ois* ending of the thematic declension). In other words if we accept, on the basis of our previous discussion, the possibility of deriving some of the Arcadian forms from earlier forms that are similar to those of Mycenaean we succeed in clarifying a few grammatical oddities of the alphabetic period which otherwise would remain incomprehensible. At the same time we throw some light on the Linear B data.

But we have not yet come to the end of the story. If the origin of σφεις is clear this may help us to clarify the question of the datives plural of the first and second declensions which I mentioned earlier. Mycenaean in all likelihood had datives in -*oihi* and -*āhi* and instrumentals in -*ois* and -*āphi*. Arcadian and Cyprian know only dative plurals in -*ois* and -*ais*. Previously we argued that the form of the Arcado-Cyprian datives was due to a choice between the two sets of forms available in the second millennium. The history of σφεσιν/σφεις now confirms our original hypothesis and gives a new slant to the whole discussion. It is likely that in the immediate post-Mycenaean period two forms were available for the dative/instrumental plural of the second declension: **-oi* or **-oyi* (after the loss of intervocalic *h*) and -*ois*, the original instrumental form which probably was no longer semantically distinguished from the dative and locative. If a choice had to be made there was little doubt that the best characterized form -*ois* was bound to prevail : a monosyllabic **-oi* (< **-oyi* < **-oihi* < **-oisi*) was unacceptable because it was homonymous with the locative singular. On the other hand, the alphabetic -*ois* may also have arisen, as σφεις did, from a simple redetermination of the plural **-oi* (< **-oihi*) with a pluralizing -*s*. Whatever its actual origin (and we shall never know) the «winning» -*ois*, if it ever competed with **-oi*, was bound to be interpreted as a recharacterized variant of it. At the same time the existence of σφεσιν in Arcadian may show that the reintroduction of an -*s*- into the inflection could still happen in the post-Mycenaean era, something which has often been disputed and has sometimes been used as an argument against the -*oihi* interpretation of the Mycenaean datives [36].

(36) Forms like the early Argolic or Ionic -*oisi* must be due, as normally assumed, to the reintroduction of -*s*- into the earlier and expected -*oihi* ending or into a later version of this form after the loss of aspiration. O. SZEMERÉNYI, *Cambridge Colloquium*, p. 222 ff. argued that -*ois* was preferred to -*oi* < -*oï* for the obvious reasons, but also suggested that -*oisi* was due to a remodelling of -*ois* based on the alternation between the athematic -*si* endings of the dative plural and the shortened forms of the πύξ or λάξ type. In view of the new evidence provided by σφεσιν I wonder whether this is necessary.

10. In the example (however trivial) that we have been discussing comparison and reconstruction not only lead to the clarification of etymological facts but also account for the coexistence in the same dialect of two different forms with the same function. At the same time they oblige us to reconsider the inflectional history of the dialects. The lesson to be learned is that, whatever their personal preferences, those interested in a historical approach to the Greek dialects cannot dispense altogether with reconstruction and prehistory. Priority will always have to be given to the actual data, to fact finding, to a philological inquiry and to an analysis of a dialect as a whole but comparison and reconstruction are powerful tools and cannot be ignored — all the more so, if the Mycenaean data now allow us to test some at least of our results against real evidence. Does this mean that the Hoffmanns and Kretschmers of this world now win against the Thumbs and the Bechtels? Not really. I have made a case for the validity of dialect reconstruction and I have argued that this is a necessary precondition for the respectability of classification, but I have said nothing about the reliability of the further inferences that lead to classification and from classification to ethnic and historical conclusions. That is another question and one for which some scepticism may well be in order.

<div align="right">Anna MORPURGO DAVIES.</div>

DISCUSSION

García-Ramón : Je suis bien entendu d'accord avec la validité du modèle de reconstruction comparative interdialectale que vous proposez. Or, je ne peux que regretter que l'état actuel des études dialectologiques fasse qu'elle soit la bienvenue et qu'elle puisse être dans le cas de l'arcado-chypriote considérée comme «traditionnelle». Faudra-t-il rappeler que ce ne sont pas les concepts «traditionnel» et «innovateur», mais plutôt «probant» et «non-probant» qui ont une portée dans le domaine scientifique? La reconstruction comparative interdialectale se heurte, certes, à des difficultés : mais on ne peut ignorer sérieusement que la connaissance des *corpora* offre souvent une série de concordances (de véritables *lectiones difficiliores*) entre des dialectes fort éloignés géographiquement (e.g. entre l'arcadien et le chypriote, entre les dialectes éoliens, etc.) qui ne peuvent relever que d'une origine commune à une époque antérieure aux textes dont on dispose. Si vraiment on veut faire des progrès dans nos études, il est préférable d'oublier la querelle méthodologique entre ce que vous appelez «classifiers» et «separatists», que l'on s'efforce de renouveler de nos jours dans un débat stérile ou l'on déforme souvent les positions que l'on traite de «traditionnelles» (ainsi, le «postulat néo-grammairien de la régularité des lois phonétiques» est systématiquement faussé : on ignore la nuance «dans les mêmes conditions») et on présente comme du nouveau ce qui ne l'est pas. Faut-il vraiment s'attaquer à des questions de méthode plutôt que laisser juger une approche par les résultats concrets que chaque auteur en obtient? Tout le monde est d'accord sur plusieurs points qui n'ont d'ailleurs rien de nouveau. D'un côté, le besoin de l'effort pour une bonne philologie : qu'on y arrive ou pas, cela dépend des

personnes, non pas des écoles ; et on trouve des erreurs partout, il faut le rappeler. D'autre part, personne ne doute que la connaissance exhaustive des dialectes est une condition préalable à toute considération ultérieure. Mais on oublie souvent que ce qu'on connaît de la plupart des dialectes, une fois le *corpus* contrôlé, est malheureusement très peu : que connaît-on de la morphologie (surtout verbale), de la syntaxe, du lexique du thessalien ou de l'éléen, pour me borner à deux *corpora* dialectaux qui me sont familiers ? C'est le degré de pouvoir connaître des dialectes qui est fort limité et il serait sage de renoncer non seulement à reconstruire entièrement leur préhistoire mais surtout à l'optimisme naïf concernant la possibilité de reconstruire chaque dialecte dans son ensemble à l'aide de spéculations sociologiques indémontrables sur un matériel (sauf en attique) qui ne les permet pas. Ce dont on dispose pour chaque dialecte ne permet pas de connaître ni tout le dialecte, ni toute sa préhistoire, ni toutes ses variantes sociales.