

inscriptions do not (generally),²⁶ and one cannot therefore confidently affirm that the κ- forms are generally Ionic: that they were at least sporadic and genuine is of course proved by their use in elegiac and lyric poets. They would appear, however, as West's evidence seems to show, to have been utilized by poets in their more local utterances, elegiac or iambic, in poems composed for local audiences. Even Hipponax, for instance, though he knew that κ- was allowable in trimeter and epode, used π- in hexameters (128.3, 129 West), burlesque though they be. The κ- forms, in short, were a parochial phenomenon, one not suited for the pan-Aegean and even pan-Hellenic epic. Given the choice—if indeed he was—between a local dialectal feature redolent of a specific time and place, and a more general, more widespread form, the Homeric poet chose the latter.

Homeric epic was not a poem of local derring-do or local political concern. It was a poem which celebrated Hellenic heroism against the Asiatic foe, and which recalled mighty deeds of mighty warriors united in a Greek overseas expedition. In the service of this poem the poet utilized a pan-Hellenic language, a language full of (heroic) archaisms and the various forms of Greek known to him from around the Aegean Sea and possibly elsewhere. Though basically Ionic and hence basically his own dialect, or an archaic form of his own dialect, Homeric epic nonetheless admitted influences from outside that dialect. A national epic required a national language, and Homer forged that language, as he forged his poem, out of resources drawn from the entire Greek world.

WILLIAM F. WYATT JR.

Brown University, Rhode Island

²⁶ Cf. Buck (n. 17) 63, F. Bechtel, *Die griechischen Dialekte* iii (Berlin 1924) 87-89. Bechtel holds that -k- develops regularly in the indefinite relative between two /o/'s. And it may well be that this is where the development began. See now J. Chadwick, *JHS* cx (1990) 174-77.

The descent of the Greek epic: a reply

In *JHS* cx (1990) 174-7 Dr John Chadwick expresses scepticism about certain lines of argument followed in my article 'The Rise of the Greek Epic' (*JHS* cviii [1988] 151-72). He will not expect me to be heartened by his remarks. But I am. If this (I reflect) is the worst that the linguistic establishment can throw at me, there cannot be too much wrong with my approach.

His paper consists largely of a rehearsal of elementary facts and principles familiar to me and to everyone in the field. We differ, evidently, in our assessments of the bearing of these facts and principles on my reconstruction of the main phases of the epic tradition. I will try to explain succinctly why his representations leave me so unabashed.

His first point is that the spelling conventions of our text of Homer (use of εἰ and οὐ for ē and ō, elimination of qoppa, etc.) cannot go as far back as the seventh century. The text therefore underwent 'various modernisations' before reaching the Alexandrian editors. In this process, he claims, forms such as κῶς, δίκου, if

Homer had used them, would have been replaced by πῶς, δίκου, so that there is no force in my argument that the absence of the κ forms aligns epic Ionic with Central or West Ionic as opposed to the Ionic of Asia Minor.

Chadwick fails to distinguish between orthography and phonology. To replace Φορε by κούρη, χσενος by ξεῖνος, etc., is merely a matter of spelling. There is no doubt that modernisations of this sort must have occurred in the Homeric tradition, even though we do not know what kind of alphabet was used in the original written text. But to replace κῶς by πῶς is not a matter of spelling—π is not another way of writing κ—it is a substantive change from one dialect form to another. That a change of this sort was effected in the pre-Alexandrian written tradition is an unsubstantiated hypothesis, and one that raises awkward questions. Why should κῶς and κού be changed to πῶς and πού, when they were perfectly familiar as 'Ionic' forms from Herodotus and other authors? Why were they *not* changed in Callinus, Mimnermus, Semonides, Hipponax, and Anacreon?¹ If we did not know where these poets came from, we could infer correctly from their use of κ forms that they came from the East Ionic area,² by contrast with Archilochus, Tyrtaeus, Theognis, and Solon, who have π forms. Why should this criterion be deemed inapplicable to the epic dialect?

Chadwick writes: 'West might have supported his thesis of a Euboean origin for the Homeric text by pointing to the aspiration which is guaranteed by consonant changes resulting from contact with initial aspirate (type τῖφθ' οἰτῶς). Psilosis is normal in East Ionic, but the aspirate is partially preserved in Central Ionic and fully in West Ionic'. I must point out firstly that I was not arguing for 'a Euboean origin for the Homeric text',³ but for 'Euboea as the area in which the epic language acquired its definitive and normative form' (my p.166). Chadwick then proceeds to attack the argument I did not use in support of the thesis I did not propound: 'But aspiration in Homer is quite obviously the consequence of editorial interference with the tradition, so that this proves nothing about the origin of the text'. Actually the question of aspiration and psilosis in Homer is more complex than he implies. I dealt with it briefly, and I think sufficiently for my purposes, on my p.163.

The usefulness of linguistic features as pointers to the past would indeed be diminished if, as Chadwick supposes, some generations of oral transmission intervened between 'the monumental composer' and the establishment of a complete written text, which he thinks only happened towards the end of the sixth century. This is of course the theory of G. S. Kirk, and it was courteously demolished by Adam Parry a quarter

¹ Cf. 166 of my article.

² Amorgos belongs here as a Samian colony.

³ I did suggest that the *Odyssey* might be a Euboean poem, but I made it clear that I believe the *Iliad* to have been composed in Asia Minor (172).

of a century ago.⁴ I do not see why Chadwick finds 'further evidence' for it in the phenomenon of diectasis (ὄρω, ὄραν, φῶς, etc.). I agree with him that these 'spellings', as he calls them, represent the actual pronunciation of rhapsodes; but why not of Homer?

It is well known that metrical and prosodic anomalies in Homer often disappear when we replace linguistic forms of the transmitted text by the older ones from which they evolved. Early verse inscriptions such as the Ischia cup⁵ show that many such anomalies had already come into being by the late eighth century. Chadwick thinks it reasonable to assume that all previous eras were equally tolerant of 'loose metrical practices', and he infers that it is not justified to argue back from metrical-ly irregular formulas to older forms that would have been regular. Thus he disputes the usual assumption that Ἰλίου προπάρουθεν and Αἰόλου κλυτὰ δώματα go back to Ἰλίoo and Αἰόλοο. He says that this view 'has been somewhat damaged by the revelation that Mycenaean knew only a genitive in -οιο'. I cannot detect any damage. No one, I think, maintained the -oo genitive to be Mycenaean. It is the logical intermediate stage between earlier -οιο and later -ου. It might have happened that when the intervocalic ι disappeared, the two short ο's immediately merged into a long one. But the Homeric evidence points clearly to the existence of a phase at which poets used uncontracted -oo. Chadwick regards Ἰλίου and Αἰόλου as examples of a licence to treat names scanning — as —. There is evidence of this in inscriptions where an awkward name had to be accommodated. But anyone who seeks to account for Homer's Ἰλίου and Αἰόλου in this way must explain

(a) why the licence is only used in the genitive singular (eighteen instances, not only proper names but also various nouns and adjectives); why never molossic Ἰλίωι, Δαρδάνων, θυμηνίς, etc? And

(b) why the following word almost always begins with two consonants, if not to lengthen the second ο of -oo. In two of the three exceptions the following word begins μεγα-, which is equally capable of lengthening a preceding short open syllable. The remaining case is *Iliad* ii 731 Ἀσκληπιοῦ δύο παῖδε.

It cannot be fortuitous that metrical irregularities in formulas constantly resolve themselves when antecedent forms are reconstructed. In some cases we have to go back a long way, even to forms earlier than those represented in the Linear B documents. We should welcome this evidence for the age of the epic tradition, which is consistent with the findings of Homeric archaeology. Chadwick himself seems to accept that the tradition goes back to the Mycenaean period.

He is more doubtful about the thesis that an Indo-

European tradition lies behind it. He emphasizes that the hexameter, so far as our evidence goes, appears to be a Greek invention. I never suggested otherwise. The hypothesis of a continuity of poetic tradition from Indo-European times is not understood by its many adherents as excluding changes of metre. On the other hand, when Chadwick writes, 'To prove that poetry existed among the undivided Indo-European peoples we should need to demonstrate the existence of similar metres, or at least similar metrical phrases in at least two traditions', he is stating a requirement that has long been satisfied. Only a few lines earlier he has cited Meillet's monograph *Les origines indo-européennes des mètres grecs*, where the strong similarities of Vedic and Aeolic metres were pointed out. Meillet's work has been successfully extended by Roman Jakobson, Calvert Watkins, Gregory Nagy, and others. Perhaps Chadwick, like G. Zuntz, is unimpressed by Meillet's comparisons.⁶ If so, he should try to convince us that they are invalid, not speak as if nothing of the kind had ever been published.

As for the comparison of poetic phrases in different national traditions, he again ignores most of what his fellow philologists have done in this field. He mentions only the 'hoary example' of κλέος ἀφθιτον ~ Vedic *śrávo...áksitam*, calling it 'almost unique'. 'The coincidence is striking', he concedes, 'but we should surely demand a string of such coincidences before accepting...' Anyone acquainted with the literature cited on my p.152, nn.6-7, will know that such strings are available.

Chadwick's concluding remarks on dialectal developments in Greek are somewhat elliptical, and it is not entirely clear to me what in my arguments is being criticized. If the implication is that I have assumed the dialect groupings of the first millennium to be equally valid for the second, or that I am unaware that in identifying regional elements in the epic language one must distinguish between inherited archaism and innovation, I plead not guilty.

A second paper written in reaction to my article appears in the present volume: W.F. Wyatt, 'Homer's linguistic forebears'. By courtesy of the author and the editor I have been granted a preview and the opportunity to respond. I will content myself with a few points and, for the rest, leave it to our readers' discernment to judge between Professor Wyatt's interpretation of the facts and mine.

My view of the development of the epic tradition is not quite so compartmentalized as Wyatt makes it appear. I do not conceive of Mycenaean culture and Mycenaean epic as being confined to the Peloponnese, and while I speak of an accretion of epic themes from the south to a late Mycenaean Thessalian poetic tradition, I do not think in terms of a specific migration of bards in the context of some refugee movement. Nor do I imagine a 'translation' from Achaeans into Aeolic. The differences between Achaeans (if this is taken to mean the language of the pre-Dorian Peloponnese) and Aeolic

⁴ 'Have we Homer's *Iliad*?', *YCS* xx (1966) 175-216, reprinted in J. Latacz (ed.), *Homer. Tradition und Neuerung* (Darmstadt 1979) 428-66, and in A.M. Parry, *The language of Achilles and other papers* (Oxford 1989) 104-40. Cf. M.L. West, 'Archaische Heldendichtung: Singen und Schreiben' in W. Kullmann and M. Reichel (ed.), *Der Übergang von der Mündlichkeit zur Literatur bei den Griechen* (Tübingen 1990) 33-50.

⁵ *CEG* 454; see P. A. Hansen's addenda and corrigenda in *CEG* ii (304), where Risch's supplement (favoured by Chadwick) is refuted.

⁶ G. Zuntz, *Drei Kapitel zur griechischen Metrik* (Österr. Akad. Sitzungsber. ccccxliii [1984]) 12 n.10. Zuntz asserts that the Indian metres adduced are 'so variabel, dass sich für jede denkbare Form eine Parallele finden dürfte'. I cannot agree. They have characteristic rhythms which make a verse easily recognizable as such; and Meillet's comparisons are based on standard, not aberrant forms.

(the language of Boeotia and Thessaly) will still have been quite small at the period in question. Such regional differences as existed in speech would naturally be reflected in local uses of the epic language, and in Thessaly the epic language naturally took on Aeolic features as they developed.

Wyatt maintains that there never was an Aeolic epic, on the grounds that it 'has not left a trace', that we do not hear of any pre-Homeric Lesbian poets (but then, we do not hear of any Ionian ones either), and that there is no hint of any epic poetry lying behind Sappho and Alcaeus, 'save reminiscences of Homer'. Even if the screen were as blank as he suggests, it would prove nothing: there would be no difficulty in the hypothesis that a Lesbian epic tradition flourished in the eleventh, tenth, or ninth century but had died out by the seventh. But I must once again draw attention to the Lesbian poets' Πέρραμος and Πέρραμος, forms that could only have developed in a Lesbian tradition of poetry about Troy, the first by the regular Lesbian sound-change /CpɪV/ > /CεppV/, the second by accommodation of the new form to formulas requiring the original scansion with short initial syllable.⁷

Wyatt does not deny that there are Aeolic and even Doric forms in the epic language. The question is how they got there. His notion is that Ionian poets brought

them in deliberately, partly to replace 'difficult' archaisms (but why should an Ionian audience have found ἄμμεξ and ἄμδξ easier than *ἤμέξ and ἤμδξ?), partly for thematic reasons (Thessalian Achilles, action situated around Lesbos, Lesbian spoken not far from the Troad). This is to attribute to the bards a measure of linguistic self-consciousness and artifice worthy of the Alexandrians. We are asked to imagine an Ionian poet who, in retailing a story of Thessalian Centaurs, takes it into his head to call them Φήρεξ instead of Κέντρουροι without there having been any Aeolic poetic tradition to supply him with this gloss. No. Thessalian mythology, reference to Lesbos in the context of the Trojan War, and interest in Troy itself are not merely contingent phenomena that encouraged an Ionian epic tradition to sprout a few Aeolisms: they themselves point to prior Aeolic epic, and the linguistic Aeolisms point with them. Why fight it?

M.L. WEST

All Souls College, Oxford

⁷ Cf. *CQ* xxiii (1973) 191; *JHS* cviii (1988) 163 n. 79, where I point out that this sound-change must have been completed relatively late, after Πρταμος had become established in Ionian poetry.

Papers of Professor A.W. Lawrence

At the time of his death in 1991 Professor A. W. Lawrence was re-working the valuable notes which accompany his revision of Rawlinson's translation of Herodotus, published in a limited edition by the Nonesuch Press in 1935. Scholars who wish to consult this material should contact Dr Judith Priestman, Dept. of Western MSS., Bodleian Library, Oxford OX1 3BG.