David on West on DAVID (A. P.) **The Dance of the Muses. Choral Theory and Ancient Greek Poetics.** Oxford UP. 2006. Pp. xi + 284. 9780199292400

M. L. West's attempt to dismiss and discredit my book in the *Journal of Hellenic Studies* (128 (2008) 182-3), which does not publish author's responses to reviews, is here answered with the useful and the substantial.

' ... the syrtós*, supposed to be of immemorial antiquity.'*

The national dactylic dance of Greece no doubt *is* of immemorial antiquity, but the memorial that I in fact cite is an inscription from Boeotia of the 1st century CE, at which time it was called the 'dance of the forefathers' (104).

'The ring of dancers revolves making one longer and two shorter steps, with regular pauses and retrograde movements, from which David derives the hexameter with its caesurae.'

I actually make no claim about the dance having a pause, since it need not have had one (108-9), and the association of 'caesurae' with 'pauses' in this ostensible summation is misleading. West is confusing the dance and its accompaniment. From the perspective of the dance—and catalogue poetry was in my view composed, and only justified aesthetically, as a summoning agent in revenant dance ritual (138-41, 208-9)—the verbal phenomenon which produces caesura marked a point of turn which was the beginning of a retrogression. The diaeresis, a conjunction of new word and new foot—as at the beginning of a line—marked the end of the chiastic retrogression (abc-cba), and a resumption of rightward movement. West nowhere mentions the diaeresis or my interest in it (15-16, 94-5, 114-15, 125-6), unless he has mistaken all such word-divisions for 'caesurae'. Regular diaeresis near the end of a stichic line is in fact a curious anomaly; why after all an inceptive cue, a 'kick-start', just before the closing cadence? No other stichic line shows this. The caesura-diaeresis interval in the midst of an hexameter line in fact marks a closed circle of retrogression within a revolving hexameter dance.

S. G. Daitz has argued that there should not be a pause in recitation at the caesura (*American Journal of Philology* 112: 2 (1991) 150-60), and I agree when one is considering danced performance, or recitation that is true to dance. The caesura is in origin a point of orchestic *turn*, not rhythmic *pause*, and it is possible to demonstrate the effective performance of even non-catalogic verse without a mid-line pause. But within Homer there is on occasion the depiction of heroic song sung independent of dance, and in Odysseus' lyreless tale of wandering, perhaps even the depiction of a rhapsode. I think it likely that in the development that led to the histrionic use of the Homeric texts by rhapsodes, and perhaps to begin with in the scripting of these texts for them, mid-line and interlinear pauses were expected, cheated, enjambed and rewarded, just as they are in Shakespeare. Classical music derived from dance also takes pleasure in the rest and the rubato. But it is increasingly clear to me that one *could* have danced to the whole texts of Homer, without pauses, if there was enough sap in the legs and bronze in the voice. (Please listen to demonstrations of rhapsodic performance—that is, with pauses—at http://danceofthemuses.org. Also to be found posted there is a video record of a danced *syrtós* accompanied by Homeric verse.)

‘The structure of the dance, he claims, can also account for such stylistic features as recurrent phrases, ring composition and narrative inconsistency (41-2, 47-8) ...’

If one looks at the passages cited, there is no claim attributable to me, or to anyone else, about ‘narrative inconsistency’. I myself and many others do not find it in Homer, in such a way as to cause unease. In one of the cited passages I requote a phrase of D. M. Shive’s, which I had earlier mentioned in the following way: ‘“Formulae are repetitions”: in its origins, oral theory presumes to apologize for what some modern *littératteurs* perceive in Homer as his “characteristic inconsistencies and inconcinnities”.’ (15) It is oral theorists who begin from a perception of ‘narrative inconsistency’ in Homer, and a number of other blemishes besides, which they used to justify in comically patronising ways. (Nowadays they tend to be *nouveaux littératteurs*, deploying the word ‘tradition’ where it suits them—in interestingly patronising ways, but at the expense of any logically defensible oral theory of actual composition-in-performance.)

‘Parry’s theory of oral composition ... is accordingly redundant, a pernicious ‘fantasy’ (48, 208).’

The ‘pernicious’ is all West, none David (as his quotation marks passively suggest). The lady doth protest, methinks.

‘... he does not claim that Homer necessarily intended his poems to be danced, and is vague about when and how versification became independent of the dance.’

Homerists discredit themselves before an intelligent public, if not before each other, by responding to questions about dates. As long as classicists in general depend upon the house of cards that is the Egyptian chronology, supplied by exogenous and theoretical archaeologists, they will have to live with a ‘Dark Age’ of Greece; whereas the facts on the ground are that Mycenaean objects are found contiguous with archaic ones, and sometimes above them. (The ‘heirloom’ theory of such objects already has this public nodding, without, thankfully, meeting our eyes.) So much for ‘when’.

But the questions of ‘when’ and ‘how’ are indeed subtle when it comes to a question of versification, or more generally, ‘music-making’, where there is a departure in performance practice from accompanying a dance. We have ample experience of this: to this day we sit in a concert hall, and call a seated ensemble an ‘orchestra’. But the fact that J. S. Bach may have sat alone at a harpsichord when he composed, did not prevent dancers’ feet from animating both his fingers and his sentences, when he wrote a minuet and gigue. When and how did modern classical music lose its directly generative connection to dance?

Development in a genre, which implies at least a relative chronology, is not so difficult to discern. An increasing sophistication in metre makes it easy enough for a beginner to be able to distinguish between Stesichorus and Pindar, or even early Pindar and late. West and other oralists, playing in the shadow of A. Meillet, attempt to ‘derive’ the hexameter from smaller lyric units. The hexameter line is not conceived of as a whole; it is, rather, a sort of amalgam of lyric segments, whose lengths happen to generate the familiar caesura and diaeresis points of the line. Here are the facts: lyric texts followed epic ones, and primitive lyric preceded complex lyric. So where did Homeric epic, the fully fledged chicken rather than the egg, come from? This is admittedly a mystery. But let us call it a happy, or a profound mystery. How does it help to put lyric first, in some proto-, ultra-primitive form with no conceivable exemplar—and make historical judgements that are not so much inverse, as literally perverse? Were the lost lyric forms complex and sophisticated enough to produce the panoply of epic rhythms and diction, only to revert to simplicity in the face of Homer, and recomplexify over time in the hands of Pindar and Sophocles? And why did these prehistoric lyric cola not agglutinate into other forms than the dactylic hexameter?

Extant lyric cola do not display the extravagant phonological adjustments, such as metrical lengthening and shortening, that epic lines do. As P. Chantraine concluded, 'il apparait que le rhythme naturel de la langue grecque s'adaptait mal à la métrique rigide de l'hexamètre dactylique.' (158) There is a deep fallacy in the notion that language-driven metres like Aeolics could be used to generate a metre whose extant poetry ubiquitously displays extravagant distortions of language. The extant texts of Homer cannot be composed of traditional formulas, if it was combinations of formulas well-adapted to the rhythm—lyric cola—which originally generated the hexameter.

*'D. describes [W. S.] Allen's work as 'unimpeachable' (16, cf. 68 ff., 264), but it was in fact convicted long ago of being based on circular reasoning (*Gnomon *48 (1976) 5-8) and is generally ignored by specialists.'*

West appears to have made a career out of descriptive arguments—or better, descriptive judgements—but he does not seem to know how they work. In this case he seems to think that they are deductive. Perhaps he has not thought through the nature of descriptive accounts of phenomena, whether in linguistics, where the results have of course been admirable, or in description generally. One first intuits a pattern in the phenomena; one then *looks* for it, and for evidentiary consequences of it. There was nothing deductive about the original claim that Sanskrit, for example, may have been cognate with Greek. The more and more disparate phenomena that seem to answer to a proposed rule, the more persuasive the rule will be, to the users, academic or otherwise, who validate it. The process and the argumentation are therefore *necessarily circular*. (The circularity can be seen in the very concept, 'descriptive rule', if one unpacks it. Obviously that does not mean that there are not descriptive rules, or that our grammars are disqualified. The circle is indeed a divine figure.)

What distinguishes genuine descriptive accounts in linguistics from normative ones, masked or otherwise, is *demonstration*; not by logical deduction—where a charge of 'circular reasoning' would naturally carry its weight—but by example. Individual and specific example is the only recourse for a descriptive account, and there is no substitute for judgment in this kind of analysis. But analysis it is. The new theory of the Greek accent, after its historical and synchronic exposition, is ultimately demonstrated by samples of epic and lyric poetry—where the quantitative patterns are overlaid by positions of stress—which disclose, for the first time in modern history, that they are musical (115-37). Ancient Greek, alone of all known languages, living or dead, is supposed to have displayed no relation between its prosody and the performance of its poetical texts. (This has not made modern classical scholars shy, all the same, when they interpret performance texts from Homer to Aristophanes.) Unfortunately there are no set criteria for what constitutes a 'musical' pattern, but the charts I drew up in graduate school for this book seem to show something obvious, and more importantly, something compelling. A new tonal theory of the Latin accent (75-9) is also based on the analogy of the Vedic *svarita*, and so buttresses convincingly the only sort of demonstration possible in all such descriptive claims: a breadth of cover for the account shown by individual and specific example. My account vindicates those ancient grammarians who described the Latin accent in the tonal terms with which Greek was described. I hope that readers will look to these demonstrations, unrecognised as such by the reviewer, if they wish to look to the heart of the matter.

As for the reception of Allen's work on stress, the true specialists have never ignored it: it is highly respected among linguists, if not among what remains of the inheritors of classical philology, and their echo chamber. As you may have guessed, the review in *Gnomon* 1976 of Allen's stress theory is in fact *West's own*. His prosecution of 'circular reasoning' need not in fact be wrong; it is instead pointless, and rather childish. Thankfully West is not always successful in his attempts to discredit others' work. I rather endorse the judgment published eight years later by A. M. Devine and L. D. Stephens (70), where the stress theory in Allen's *Accent and Rhythm* is described as 'the first work in the field of Greek metre that can truly be said to understand the requirements of scientific method and theory construction'.

'In D.'s version pitch and stress are brought together in one system: the most prominent syllable in a word may be the one on which the high tone falls, but if it is succeeded by a long syllable, the latter, which carries the falling pitch after the acute, wins the greater prominence.'

The reason for this victory is that the downward pitch-glide combines in this case with longer duration. When the succeeding syllable is short, the rising pitch-glide (acute) predominates.

'Like Allen's theory, this is not supported by any phonological evidence or ancient testimony (indeed, D. gives a badly distorted acount of what the ancients meant by 'barytone'), but is devised to humour the Anglophone hankering after a stressed ictus.'

West seems to have skipped a whole chapter in the book replete with ancient testimony, about the nature of the harmonic accent that was described by authoritative native informants as *barus*, or 'heavy' ('The Voice of the Dancer: A New Theory of the Greek Accent', 52-93 *passim*). Sources include Glaucus of Samos, Plato, Aristoxenus and the Thracian Dionysius. All confirm that the *barus*, one of the twin components of *harmonia* along with the *oxus*, was not, as now, understood to signify 'low-toned' or 'unaccented', but to refer to a 'leveling' or downward pitch-glide, where the *oxus* referred to a 'tensed' or rising one. This evidence is univocal and uncontroversial, albeit badly neglected. I show that Allen's claim that there was a down-glide in Greek cognate with Vedic, is heavily supported by history, and not just common sense. Unlike Allen I extend the analogy fruitfully also to Latin and to classical Sanskrit, in such a way as to reformulate their accent rules in terms of tonal stress (75-9, 83-4). One likes to think that there is a place in philology for 'cognates', in the context of reconstructive history, alongside the far more dubious class of provenance claims (3-6).

My historical argument for Greek is corroborated by the synchronic accounts of A. H. Sommerstein, P. Sauzet and C. Golston (80-2), all of whom call attention to a falling glide as a separate accentual feature from the rise, and two of whom suggest that it is accentually prominent. This sort of corroboration obviously lends credence to a non-standard historical claim.

It is therefore not at all clear what West could mean here by 'phonological evidence'. It is true that classical Greek does not show some of the characteristic features of stress, such as weakening or shortening of unstressed vowels and syllables. But neither does Latin! And moderns almost universally suppose that classical Latin had a stress accent. Hence there is no 'Anglophone hankering' (!) in my work. Both the Greek and Latin accents do show these characteristic features historically (83), or prehistorically in the lay sense; but in the recorded period, they show a culminative contonation, which has a dynamic property that can reinforce metrical ictus, without diminishing the value of neighbouring syllables. This lack of diminution in unstressed syllables is the key feature that allows classical Greek and Latin metres to be 'quantitative', like dance metres, but quite unlike English and other poetic metres. (The classical period of the culminative contonation in Greek and Latin appears to have been both preceded and succeeded historically by periods of a stress prosody which does exhibit the characteristic diminishing effects on unstressed syllables. The nearly simultaneous appearance and disappearance of such a contonation in Greek, Latin and classical Sanskrit, constitutes an historical-linguistic puzzle.)

'[D.'s theory] yields, for example, a stress on the third or sixth thesis of the hexameter whenever the word before the caesura or at line-end falls into any of a range of accentual patterns.'

This is in fact the demonstration in relation to a descriptive account to which I earlier referred. The phrase 'before the caesura' is highly misleading. The caesura, unnoticed and unheard of in the ancient world prior to the writings of Aristides Quintilianus, is not a structural feature of epic verse, but a 'side effect' of other forces in the verbal accompaniment of a danced hexameter. It is in fact the accentual pattern of Greek, according to the new theory, that *causes* the two types of mid-line caesura (111-12, 118-19).

'However, the scheme frequently fails to produce this happy result, and when that happens we should admire the poet's skilful art of variation, his mastery of counterpoint and syncopation (121, 135-7, 249-51).'

This is simply silly. I doubt I could have been clearer: 'my claim is for a *musical* reinforcement of ictus by prosody in Homer, not an *automatic* one ... in effect, the musical prediction is for *variation*, while variation itself presumes a predominant pattern.' (121) If there is no variation, there is no music—never mind 'skilful art' or 'mastery of counterpoint'. Perhaps classics schoolchildren trained to 'scan' each line with a stress on every thesis could confuse the sound they make with music or poetry, but no-one else on the planet would. In other words, if there were no syncopation demonstrated in the relation of word dynamics to the dance pattern, the new accent theory would surely be wrong. On the other hand, if it turned out that syncopation predominated, the theory would also surely be wrong. (There are exceptions to this rule: a sarabande shows a regular syncopation on the second beat, responding to a step in the dance, 94-5.) Where there is variation, perhaps in particular when it is *words* rather than pure tones that are reinforcing the metrical pattern, one should of course look for *significance* in the variation; and I do. But variation is a local phenomenon, dependent on a local creation of expectation (150-1). There is no rote code here for the critic of Homer.

For the record, in the passages I analyse, a prominence according to the new theory occurs in the third thesis 39/52 times (75%), and a prominence in the sixth foot 49/52 times (94.2%). By contrast, the figures for the written accent in the third thesis and the sixth foot are, respectively, 16/52 (30.8%) and 34/52 (65.4%). According to the orthographic accent, therefore, more than a third of the Homeric lines show no prosody at all in the final foot, and only a minority at mid-line. Such numbers make no sense, if one assumes that Homer's lines had a musical purpose. The former numbers, however, bespeak the sense and presence of music.

'D. thinks that his system has some applicability to Latin too, and that in arma uirumque cano *there was not, as we all suppose, any clash on* cano *between accent and ictus—that would 'spoil' the caesura (77-9).'*

The new theory for Latin, formulated in terms of the Vedic contonation, says that where possible the voice must rise in pitch on the second mora before the ultima. The thing that simplifies the Latin rule in relation to the complexities of Greek is that there is no regard in Latin to the quantity of the ultima. When one applies my prescription for Greek—that when the down-glide of the up-and-down contonation happens to coincide with a heavy syllable, it predominates over the rise, but not otherwise—we generate the received rules for stress in Latin, for all shapes of word but one: the iambic disyllable (for example, *canò*). That is, we correctly predict *Cícero*, *Cicerônis*, *níhil*, *côrda*, *râri* (78). Only in this one species in all of Latin, the iambic disyllable, the down-glide must occupy a long ultima. By contrast, in a pyrrhic rather than iambic disyllable (for example, *nihil*) the rule predicts accent on the penult.

The opening half-line of Virgil's epic is itself strong evidence that in classical Latin, the ultima of iambic disyllables was accentually prominent. Iambic shortening in the historical (that is, prehistoric) picture suggests that the old Latin instinct eschewed an ultima accent. But consider the claim that West espouses: he asks us to believe that in the first mid-line cadence of Virgil's epic, the poet (for no reason useful in interpretation) composes a prosodic emphasis on the *second short of a dactyl* (the penult of *cano*). This is patently the weakest and most transient part of the foot. Reinforcement of the *first* short is an acceptable anapaestic sycopation, but rare at this cadence point of the line (161), where one naturally expects reinforcement of the ictual thesis. It is all very well, perhaps even intellectually sexy, to entertain 'a clash between accent and ictus'. But from the perspective of actual rhythm and human performance, this particular proposal is preposterous. It is not impossible in ancient verse to stress the second short of a dactyl; I myself discuss a striking case in Pindar's *Pythian XII* (268). But the claim that the first cadence of Virgil's epic is such a rhythmic malapropism—no matter what it is that 'we all suppose'—is simply, and very simply, absurd.

'The hypothesis about the hexameter's connection with the syrtós *is interesting, but neither new nor verifiable.'*

It is hard to fathom a criticism that an idea is not new. The connection between dance and verse is as old as the hills. If West and his ilk are happy to keep propagating the once-upon-a-time new ideas of M. Parry, there are many of us who might rationally choose the hills, however nouvelle their touristic facade.

Absolutely no aspect of oral theory has ever been *verifiable*, let alone *verified*. **Absolutely no ancient evidence** has ever been reputably adduced in favour of it. When it is understood that there is not really a competing suggestion, and that oralists have simply ignored the evident connection of the isometric hexameter to dance in their theorising, perhaps my argument will be entertained at least with the suspension of incredulity which once greeted Parry's theory, and did not immediately dismiss it as about so many monkeys at typewriters, generating a poetic encyclopaedia over the extensible centuries of a 'Dark Age'.

I take pains in my Introduction (1-21) to establish exactly what sort of argument I am making, and what sort of argument I intend to be replacing. It helps that they are arguments of the same kind: that is, arguments by *comparison*. Repetitions of various kinds in the text of Homeric epic are supposed to have been produced by the same forces as produced allegedly similar repetitions in the stultifying yarns of modern Muslim Bosnian guslars. As I say, 'Parry's theory is in no sense falsifiable. The only possible argument against a particular comparison is a better one, and judgement in such matters is only partially apodeictic.' (8)

But it is reasonable to expect that a successful descriptive argument will spread its wealth through illustrative resonances in the phenomena, which in turn redound upon itself. Nothing at all of this kind happens in the case of oral theory. Obviously there have been many books and articles written in the last 75 years that have contained insights into Homer; as I have said to one of my benefactors, we stand on the shoulders of giants. Not all of us could have compiled the compendia of Professor West. But all of them, it seems to me, reflect what I say about the seminal works of G. Nagy and J. M. Redfield: that nothing insightful in their dictional analyses depends at all upon the faith-based caveat to oral theory appended preemptively to their works (168).

And when the phenomena are asked to respond to the theory, the theory rebels. How else can one interpret an ad hoc proliferation in the definition of a 'formula', the central concept in any oral theory? The multitude of purported definitions, by otherwise credited scholars, is itself a sign of the failure of the theory when applied to Homer. The 'economy' of formulas is not negotiable, it cannot be 'softened', for anyone who adopts a non-literary paradigm—that is, a paradigm where composers do not always *choose* what they say—for the Homeric poems.

In any imaginable story, play, novel, film or TV show, there will always be recourse to 'tradition' in the interpretive act. What must be untangled in Homeric studies is the bewitching and yet soporific tether to the notion of 'traditional diction', provided by oral theory, which has never in fact played a logically direct part in any critic's analysis, but has merely stood there alongside, as the silent witness to substantiate what is, after all, pure speculation—however insightful such speculation may be. Let such critical speculation about Homer, analytic or neo-analytic, stand on its own merits, as criticism of other authors does, for once and for all. And thereby let tradition at last become a serious subject for Homerists. (It is a paradox in the interpretation of tradition as such, that those words and phrases frozen in Homer, which were only conjecturally understood in classical times, were the ones most likely to be traditional.)

'The hypothesis about the hexameter's connection with the *syrtós*', by extreme contrast, rewards us and redounds immediately and profoundly. We understand immediately an otherwise obscure fact: why the hexameter is built upon the dactyl, an isochronous foot born in dance, which is still the basis of modern Greek folk dance. Meillet described it as an 'innovation du grec' (158), but the dactyl is better described as an anomaly rather than an innovation; contrasting time pulses are the rule in Indo-European metrics, and speech-driven metres generally. (Oral theory has nothing useful to say about linguistic dactyls, the thing under its purview most in need of explanation.). We immediately understand why the line, conceived as a whole rather than an amalgam, should exhibit a break defined by the trochaic caesura and the bucolic diaeresis—a break without example in any other stichic line—if these represent the tropic points in a particular circling dance that is still observable in Greece. Oral theory merely accepts these breaks as 'traditional' templates, and some, as I mentioned, suggest that they are lyric construction joints, based in a completely fictitious lyric phraseology that is supposed to have predated and even constructed the dactylic hexameter. There is no such fantasy-mongering in choral theory. Choral theory rests on the assumption, justified historically and within the Homeric poems, that the *dance came first*, and that no amount of academic theorising, with or without intrinsic data or extrinsic testimony, has the power to generate a *still extant folk dance*.

Anyone who has sung verse and chorus of a Christmas carol understands that one does not have to explain repetitions in the accompaniment to a round dance: one should rather have to explain the lack of them. At the level of the syllable, the phrase, the line, in chiasmus and in ring composition, and even at the level of narrative theme, the choral theory of Homeric composition *answers*. Oral theory once saved Homerists from the public smirk that was the 'Homeric Question', but the twentieth century in our field will sadly be remembered for oral theory instead. Once the connection registers between Homeric rhythm and form and that of the round dance, no amount of watering-down will be able to save Parry's theory and its reception from daylight justice, and a historical hangover.

'Even if there is something in it, it cannot support a reductionist theory of the dactylic dance-step as the source of all Greek metre.'

I do not espouse a reductionist theory in my book. The main point about aeolic rhythms is that they represent an attempt to 'rein the dactyl in', for the sake of the lyric, tragic, and comic dances that moved more and more to speech rhythm. West has apparently not grasped this feature of my analysis (236 ff.). Sources are things to divert from, as well as draw from. The epitrite was already a way to cadence a dactylic run; but in the archetypal glyconic, the sole dactyl was always immediately abutted by a cretic, which, in its true and physical sense as a dance movement, short-circuited the dactyl's urge to run. Once upon a time, the dactyl was everything. In the attested historical period of lyric development, the dactyl increasingly became a *foil*, as Greek poetry began to explore its linguistically iambic rhythms, even in fully choral performance, and often sought to confine the dactyl—a dance and not a speech rhythm—within the strait-jackets of the expandable glyconic and the cadential pherecretean. (See 'The Lyric Orchestra', 215-69, *passim*.) Expansions were of course sometimes dactylic, and the Greek tragic and comic experience was richly characterised by extended and virtuosic anapaestic runs. But there was a definite development from a definable origin.

'The accentual theory is without merit and involves much special pleading. Is there then anything of value in the book? As everything in it is based on those two theories, I am afraid the answer is no. OUP was badly advised in this case.'

My dissertation was read and enthusiastically approved by the late A. W. H. Adkins and D. Grene, and chaired by P. Friedrich at the University of Chicago. My outside reader was G. Nagy of Harvard University. The manuscript was reviewed and approved for OUP, with calls for minor revision, by two British scholars highly qualified to judge it on its merits, who, to the best of my knowledge, had no prior acquaintance either with me or my work. I am deeply grateful to Oxford for agreeing with the judgement that it should be disseminated. There is in fact in my book a new and soundly argued theory of the accents of ancient Greek and Latin. Splash the headline! Every century is a new one for Classics departments, and perhaps this one will be a century for performers.

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