

SEVEN FATAL FLAWS IN THE ATTEMPT TO DERIVE THE DACTYLIC HEXAMETER FROM AEOLIC COLA

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SETE FALHAS FATAIS NA TENTATIVA DE DERIVAR O DÁTILO HEXÂMETRO DA COLA EÓLICA

RESUMO: A tentativa de M.L West de desconsiderar e desacreditar meu livro em resenha é aqui respondida no que é necessário e fático. Segue-se então uma crítica de propostas de se encontrar a origem do Hexâmetro datílico a partir de pequenas e tardias frases métricas líricas. Essa abordagem tornou-se uma ortodoxia, captaneada por West e G. Nagy¹.

PALAVRAS-CHAVE: Dátilo Hexâmetro, Cola Eólica, Metro grego, Cesura, Dança grega.

ABSTRACT: M. L. West's attempt to dismiss and discredit my book is here answered with the useful and the substantial. There then follows a critique of attempts to 'generate' the dactylic hexameter from smaller, and historically later, lyric metrical phrases. This approach has become an orthodoxy, whose champions include West and G. Nagy.

KEYWORDS: Dactylic Hexameter, Aeolic Cola, Greek Meter, Caesura, Greek Dance.

1. N.Editor: Neste artigo, A.P.David responde à resenha de seu livro *The Dance of The Muses. Choral Theory and Ancient Greek Poetics* (Oxford University Press, 2006), realizada por M. West no *Journal of Hellenic Studies* 128, 2008:182-183. Em sua resposta em forma de artigo, A.P.David justapõe trechos da resenha, de seu livro e ampliações de sua argumentação. Para conhecer mais sobre o trabalho de A.P.David, v. www.danceofthemuses.org.

... 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 chiasmic 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 orchestric *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 *littérateurs* 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 littérateurs*, 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 apparaît que le rythme 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. I shall say more on this in the epilogue.

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. It was reprinted to considerable professional acclaim in 2009. 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 account 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 predication is for *variation*, while variation itself presumes a predomi-

nant 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 occurs 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*, *Cicerônís*, *níhil*, *còrda*, *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, *níhil*) 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 connec-

tion 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 mon-keys 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 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.

It is time for 21st Century Homerists to ask the following question in the most direct and unblinkered way: what exactly about Homeric poetry, in its broad and fine structure, is a theory about its oral, extemporaneous composition by anonymous bards, as part of an unknown and historically unattested tradition alleged to span uncharted centuries, supposed to be trying to explain? What exactly is meant to be illuminated about the extraordinary Homeric art works that we continue to read, study, and enjoy, by a comparison with stultifying orally extemporized yarns from Bosnia? I am not here going to refute the assertion of 'economy' that is, logically, the central pillar of Milman Parry's output. All it has ever been is an assertion. I learnt the formal refutation twenty years ago from David Shive's book, *Naming Achilles* (OUP 1987), but the latest thing appears to be Rainer Friedrich's *Formular Economy In Homer: The Poetics of the Breaches* (Verlag 2007). Friedrich is led to describe Homer's text as 'post-oral'. But without the economy of formulae that alone makes extemporizing possible, cognitively, there is no reason to connect the Homeric texts to any sort of extemporized origin. To judge by recent opinion, of every stripe, they no longer need an apology for their style. With far better justification would we call the Shakespeare Folio, in relation to historical performance, the composers who produced classical music in relation to dance, and the storytellers who published serialized novels in the 19th century, 'post-oral'.

Most of you will have heard of Homeric 'formulas'. Homer repeats a number of short phrases, and sometimes whole lines and series of lines, in the course of his compositions. Why?

We should note that it is misleading to refer to these repeated phrases as ‘metrical building blocks’, as though we knew what was being built through the hexameter and why. The French alexandrine, for example, divides its twelve syllables right down the middle. Each hemistich is also divided, not always isosyllabically, but isochronously in performance. Hence there is a pendulous symmetry in its declamation. The epic line, however, almost never divides down the middle, through tens of thousands of verses in Homer and others. There is no imaginable linguistic reason that a central diaeresis should be prohibited in a dactylic hexameter. The notion that language generates metre in ancient epic—a central assumption for a number of prominent Homerists—runs foul on this and many other simple and obvious facts of Homeric usage. There must be a musical desire behind this rhythmic choice, based on notions of balance and a pleasing asymmetry, and of the line as a whole event, which determines the shapes of phrases whether they are repeated or not; only this desire can make any sense of the notion of a ‘formula’, as a thing that satisfies it.

Several theories attempt to derive the epic hexameter itself out of smaller lyric units. The prevailing idea is that the allegedly formulaic subunits of an hexameter line began life as shorter lyric periods, which in some inspired prehistory managed to stitch themselves together into the long hexameter lines. One uses the reflexive voice because a poetic or rhythmic motivation for this stitching, and then its prolific imitation over tens of thousands of lines of epic, is not given. It is sometimes even suggested that the formula—not just the Greek language, but some of the actual rhythmic phraseology found in Homer—has an Indo-European heritage. Proponents of these ideas appear to think that they are consistent with the notion of the formula as metrical building-block; whereas the latter in fact assumes the metrical line as a given, in relation to which the formula becomes a compositional convenience. What they are truly proposing is the formula as a *metre* building-block—a miracle, inasmuch as it is a sort of thing that gives birth to its own parent.

There follow six intractable problems for the colometric derivation of the dactylic hexameter, with a seventh for the oralists who misguidedly champion this derivation.

Problem the first for such a derivation is that no such thing has been demonstrated for any other stichic line, an-

cient or modern, Indo-European or otherwise. Poets (and musical composers) have always composed in whole lines, and groups of lines. Testimony for Homer's aesthetic achievement in this regard is not lacking. People who respond to poetry with the sense that the poet is 'filling up his line', ought to have the courage simply to declare that they do not like this poetry, rather than explain it away (as, for example, 'oral'), or become scholars of it. It could be argued that the English free verse of the last century was a poetry of fragments, after five centuries of pentameter rule. The centre did not hold. What we have, at least initially, is 'line segments' freed rhythmically from the context of the archetypal line. Even if this is not the true history of free verse, such a development is at least plausible. But to claim that the English pentameter was itself *built* out of such fragments—precisely the structure of the claim of colometrist ideologues in the face of the historical precedence in Greek of epic texts over lyric ones—is to try to put Humpty Dumpty together again.

Partial quotation in word, rhythm and melody, which is documented by [Gregory] Nagy in the case of Sappho and Homer, is not evidence of an integrity to the parts *prior to the original*, as is assumed in the concept of a traditional formula; partial quotation can be paralleled in other cases of musical revolution through history, whether we look to Renaissance polyphony in relation to plainsong, or to a bebop version of Cole Porter. Such quotation, nested within a new or merely idiosyncratic order, can help persuade an audience to get its bearings, and get hip. Snippets of hexameter phrasing in the mix give the verse legitimacy, by connecting the audience to its traditional music—at the very same time that the chorus seeks a new legitimacy in moving for the first time not so much to dactyls but to the rhythm of natural language. (241-2)

Nagy would like to claim that the curtailed Sapphic quotes are not quotes, but a direct tapping into the store of colometric formulae that is supposed to have predated the hexameter. I shall go on to address the fallacy (in the formal sense) involved here. But here is *problem the first* for a colometric derivation of the epic hexameter: there is not given any typological justification whatsoever for this kind of 'derivation' of a prolific, widespread, stichic line of verse.

It is a sign of desperation to claim 8th Century dates for lyric when no basis for such dating can be given. Here are the facts: lyric texts followed epic ones, and primitive lyric preceded complex lyric. The development in lyric is clear and historical and intuitive, for anyone with the judgement to distinguish Stesichorus from Pindar. But where did Homeric epic, the fully fledged chicken rather than the egg, come from? This is admittedly a mystery. But 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?

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 theorizing, 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 an encyclopaedia over the extensible centuries of a 'Dark Age'.

My criticism of both metrists and colometrists has to do with their falsifying mathematization of symbols that were intended to record rhythmic performance. In the book I address Nagy's derivation of the hexameter from the pherecratean. The pherecratean is a catalectic or cadence metre in relation to a glyconic.

Glyconic:	X X ↓ * * ↓ * ↓
Pherecratean:	X X ↓ * * ↓ ↓

One can hear the cadence effect well enough by ear, but if each syllable represented one step of the foot, the visual effect would perhaps have been even more emphatic. A glyconic that begins on the right foot would end on the left, and so a series of them can be strung together. The pherecratean, however, has one less syllable, and hence it begins and ends on the leading foot. In general, a catalectic metre is short one syllable in relation to its partner, and has an odd number of

syllables; note that both of these properties make immediate sense if one moves to them physically.

It may at first sight make sense to say that a pherecreatean has a synchronic relationship with a glyconic (i.e. it is the catalectic version) and that it has a diachronic relationship with the hexameter (it is the parent). The problem methodologically is that the nature of 'metrical opposition' is not analogous to phonemic or lexical oppositions in a synchronic state. To say that the glyconic/pherecreatean relation is synchronic is like saying that $2 + 3 = 5$ is a merely synchronic relation, or the relation between a fifth and an octave, or between male and female. The pherecreatean does not exist without a glyconic; the catalectic version is derived from the original and exists in a definite musical relationship with it that is not determined by time or circumstance. There is not an opposition between them but a definite harmony. Every verse has to have a cadence: every glyconic or series of glyconics has to have a pherecreatean. This is a fact of dance and rhythm, in no way analogous to a linguistic fact [unless something like 'every verb has to have a subject']. Think about 'shave and a hair cut' (*bum bum ba bum bum*), which *has* to be followed by—'two bits' (*bum bum*). In his diachronic claim, Nagy seems to think that you can isolate the 'bum bum' from its context—in which case it loses all its rhythm—and derive a whole new rhythm backwards from it, in anticipation of it as, once again, a cadence. But the pherecreatean's *being* as a cadence derives from its relation to its original partner; whereas the sequence of dactyls that Nagy grafts on to it would naturally rather seek a rhythmic cadence in cretic-based forms, to judge by lyric practice (as in dactylo-epitrite). (163-4)

This brings us to *problem the second*, which is the problem with comparative metrics: it violates the most basic tenets of comparative historical reconstruction. *It can define no isolable units, like the phoneme, which interact synchronically and maintain their identity through time.* Comparative metrics, insofar as it presumes to wear the mantle and methodology of historical linguistics, is therefore *completely bogus*. Clever professors playing with signs does not a science make:

It is not illegitimate to isolate an element in a diachronic analysis—for example, the feature 'voice' in a consonantal sound change. The question becomes, however: what in fact consti-

tutes an isolable metrical element? If one were to rephrase the question as ‘what is a *rhythmical* element?’ one would immediately see that rhythmic patterns and oppositions occur that are in no way susceptible to a synchronic/diachronic analysis. Rather, the contrasting elements of rhythm remain constant and universal as long as we remain bipedal creatures who draw breath, and as long as numbers retain their properties. (164)

But when we turn to one of the proposed examples, we turn from pseudoscience to simple nonsense. This would be the now iconic comparison of the eight-syllable Gayatri verse form to a Greek glyconic. Here is the comparison which fueled the colometric fantasy. The Sesame Street test fails. (For those unfamiliar with the children’s TV programme, the song goes ‘One of these things is not like the other ...’) These two things,

Gayatri	x x x x c _ c _
Glyconic	x x _ c c _ c _

are not like one another. This is *problem the third*. Even from a mathematical perspective, the invariant part of the glyconic has six elements, the other four. If you think there is nothing worth noticing in the difference between a half and three quarters, why not pay more tax. But it is the reading of metrical elements as individual quanta, rather than integral groupings of feet, that leads to a spurious comparison. A mathematician may be struck by an identity in the final four elements of the invariant portions, and perhaps you are as well. But what are the *rhythms* doing in each case? The gayatri always closes in iambs, an ascending cadence; whereas the glyconic always closes with a descending dactyl, immediately followed by a cadential cretic. One has to cut up this invariant dactyl to make the comparison in the first place. Hence the true comparison is represented this way: x x x x ia ia and x x da cr. As I said, the Sesame Street test fails.

But simple similarity is not necessarily the phenomenon that drives a reconstructive comparison. It took long years of exploitative militarism and commerce before a British magistrate in the Raj noticed something genuinely worth comparing between Sanskrit, Greek and Latin. So ...

... What is the point that drives the comparison? It is claimed that they are both lines of eight syllables with a ‘vari-

able opening and a fixed cadence'. Note that 'cadence' here has become a null term, meaning simply 'ending'; to judge by native descriptions and usage, it was the pherecratean that produced a feeling of cadence, in relation to a series of glyconics that could be extended at the poet's will. But more important to note is that this second feature connects the two patterns with virtually *every known human rhythm*. The English iambic tetrameter, for example, another eight-syllable verse, is also fond of trochaic variation in the first part of the line (think of 'Déserts of vast eternity', from Andrew Marvell's *To His Coy Mistress*). Why should we pronounce any of these schemata 'cognates', and infer a common ancestor, other than the common rhythmic nature of our species as the generator of parallel forms? Disagreement finds its way to agreement; any human being knows this, who has had an experience of harmony or rhythmic satisfaction. It is not possible, or even prudent, to avoid sarcasm in this case. The proposed comparison and inference, and the generations of credence accorded it, discredit the whole project of comparative reconstruction. *There is no rational reason to connect these two sets of symbols at all, let alone as a basis for comparative reconstruction of an eight-legged common ancestor of some kind. Problem the fourth.*

(It is amusing to speculate about the reconstruction: does the dactyl in the Greek exemplum suggest the influence of a non-Indo-European neighbour? Or do we apply the rule that an unusual variant is likely to be original—a survivor in the face of the iambic IE juggernaut, which even took over Greek versifying?)

But we have not yet got to the salient point. In the Sanskrit line, the second and fourth syllables (in the 'variable' part) are usually long. In other words, the Sanskrit line is iambic, like Marvell's tetrameter. The invariant part of the glyconic, however, always contains a dactyl abutted by a cretic (BUM bada bumba DUM).

This suggests that a concrete and distinctive dance step determines the form of the glyconic. It is not a bunch of stuff prior to an iambic punch. That invariant close in the Greek case is a dactyl abutted by a cretic, *descending modulating to ascending*. The gayatri verse ends in simple ascending iambic. The rhythms do *not* in the least resemble each other. They are as different as can be in the realm of the realities of rhythm and rhythmic expression ... (240-1)

I draw on Antoine Meillet, who is generally championed by oralists, for his most important observation in this context. He points

... to the equality in length of arsis and thesis in the dactyl as 'une innovation du grec'. [That is, the strong and weak parts of the foot have the same time length.] This fundamental isochrony in the foot, unique to Greek, is itself evidence of an orchestric origin for Greek metre. A language-derived metre would rather be expected to build itself out of contrasting time pulses, as Meillet well understood. An isochronous foot generates isometric music. Isometry is a prevalent characteristic of dance and of dance music. Neither Greek nor any other Indo-European language appears to have been designed to reinforce isochronous dactyls. (158)

The dactyl continues to be unique to modern Greek folk dance. The descent of Greek populations is admittedly controversial and its history politicised, but to deny a connection between the modern *συρτός* and ancient dactyl-based metres is to claim a sort of spontaneity in Greek soil and air, which mysteriously asserts itself upon its inhabitants, and makes them dance funny.

So apart from the fact that there is no rational basis for comparing the comparanda in order to reconstruct a common parent, the comparates, Greek glyconic and Sanskrit gayatri, are positively dissimilar in definitive ways. The invariant portions of the compared lines are not only of different lengths, but of unrelated rhythms. And in particular, the glyconic's obligatory dactyl is unique to Greek. This is *problem the fifth*.

What we have next to deal with is in fact fallacy in the logical sense. Description is what it is: only extreme errors in historical description can lead to positive fallacy. A reviewer of my book speaks for the would-be orthodoxy:

It is also generally held that the forms of cola in Indo-European meter are derived from the formulae of oral poetics; this observation goes back to Parry and has been developed both within Greek and by comparative observation of other traditions (Lord, Foley, Schmitt). The recurring phrases of Greek epic have particular metrical forms, and the words and the rhythms grew up together. (Anne Mahoney, 'Review of *The*

That words and rhythms grow up together in poetry is a pleasant and natural enough assumption, which may also be generally true. It is absurd, however, to apply this bromide to the peculiar case of ancient Greek, and to archaic epic in particular. Meillet's observation that the dactyl was a Greek innovation is a distinction to be focused on rather than glossed. Pierre Chantraine provides voluminous evidence that the non-contrasting time pulses of the dactylic hexameter forced a violence on the language that could not be expected in a situation where 'the words and the rhythms grew up together.' As I mentioned above, he puts it without either fanfare or controversy: 'il apparaît que le rythme naturel de la langue grecque s'adaptait mal à la métrique rigide de l'hexamètre dactylique.' (see 158-9) The claim, remember, is that phrases that were originally lyric cola became the formulas of epic hexameter. Here is the fallacy:

Epic hexameter phrasing everywhere exhibits phonological and morphological adaptations, necessitated by the metre, while lyric metres depend upon and conform to the native quantities of words: the ubiquitous metrical pressure on linguistic form observed by Chantraine in the hexameter is nowhere to be found in lyric pherecrateans. Consider the implications of this for Nagy's hypothesis. Much of the 'formulaic' material in extant epic, which *does* display phonological and morphological alteration, does not belong to the period that actually produced the metre[, if 'the words and the rhythms grew up together']; it must rather be interpreted as a later product of assimilation to the hexameter, which, most paradoxically, had to have displaced such traditional material as *did* once generate the metre, and so did in fact fit the form euphonically and naturally without phonological alteration. How could such a displacement have occurred? This paradox ought to discredit any attempt to 'derive' the hexameter from smaller Aeolic lyric units (this is also [Martin] West's approach). The paradox is that apparently 'language-driven' metres (Aeolics) are being asked to generate a metre whose extant poetry displays extravagant distortions of language. The maladaptation to Greek is not claimed for her lyric metres. (164-5)

The conclusion that follows from oralist premisses is actually a welcome one to me, and anyone else who finds Homer violently original and even satiric about tradition; but one ought not to accept conclusions from false premisses, and one very much doubts that oralists have thought these things through:

[Anyone] who work[s] from the premiss that the dactylic hexameter is a product of the Greek language, ought to consider how expensive is this assumption for [the oralist] belief in the traditional nature of Homeric diction. Once the facts of Chantraine's description are taken into account, one is obliged to conclude that Homer's language has *displaced* such native formulae as were required to generate the metre. Hence the premiss leads inexorably to the conclusion that Homer is *non-traditional*, that the extant diction of epic is an *innovation*, and that its meaning and implications can give us *no direct evidence of either traditional language or thought* as it bears on the Homeric poems themselves. An investigation into Homeric tradition through its diction must therefore begin by abandoning this premiss. (165)

This is *problem the seventh*, and it arises only for oralists, that Homer's diction is non-traditional. *Problem the sixth* is the fallacy that language-driven metres like Aeolics can be used to generate a metre whose extant poetry ubiquitously displays extravagant distortions of language. The text of Homer cannot be composed of traditional formulas, if it was combinations of formulas well-adapted to the rhythm—lyric cola—that originally generated the hexameter.

In sum: the derivation of the Homeric hexameter out of smaller colometric 'units' fails because 1) there is no typological basis given for such a derivation of a line; 2) the study of comparative metrics, on analogy with comparative reconstruction in historical linguistics, is bogus; 3) the Sesame Street test fails when one compares Sanskrit eight-syllable forms with glyconics; 4) the basis for linking the comparanda (variation prior to invariance) is so broad as to link each of them to most known verse forms; 5) the rhythmic sense of the metres is unrelated and positively dissimilar, hence rendering implausible the possibility of a common parent; and 6) lyric cola, the supposed elemental constituents of the hexameter, do not in extant examples display the phonological and morphological

adaptations characteristic of the Homeric text. The child is a monster! As an absurdist corollary, 7) Homeric diction, and therefore Homeric poetry, is definitively non-traditional.

My own sense is that Homeric scholarship, whether oralist or scripsist, is trending toward the greater recognition of structure, complexity and self-referentiality. (No such development has emerged in the study of Bosnian epic.) Homer's poems no longer need an apology for their style. A number of us, a critical mass, have been humbled by them, and long to speak this experience. Perhaps we have come so far from the spring, that we have forgotten to thirst—or we are being watered by secret springs. Oral theory was intended, after all, to account, divertingly and creatively, by 20th century standards, for a certain primitiveness of style perceived by continental-bourgeois critics in relation to literary epic, and later forms of narrative. In a new, pre-oral world, have we at last forgotten this? Shall we not drink from the waters of Lethe, and clasp hands—and dance?

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