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Response to Anne Mahoney's review of *The Dance of the Muses: Choral Theory and Ancient Greek Poetics* (Oxford 2006)¹

I am grateful to *Versification* for the opportunity to respond to Anne Mahoney's review of my book, *The Dance of the Muses: Choral Theory and Ancient Greek Poetics* (OUP 2006). I especially appreciate the attention she calls to the new theory of the Greek accent. For many readers this contribution will be the most useful from my work. There is also a newly tonal theory of the Latin accent (as well as of Classical Sanskrit); much the most gratifying result of Sidney Allen's applying the Vedic *udatta/svarita* model to Greek is the sense that can now be made of the prosody of all three classical languages, which opens an unexpected door to living performance.

Mahoney is quite wrong, however, to claim that I argue 'that Ancient Greek had a type of stress accent along with the well-known pitch accent.' (Mahoney's *Versification* review, henceforward 'M', p. 1) A double accent would have been so anomalous as to be absurd typologically (see my pp. 70-1, with the analysis of A. M. Devine and L. D. Stephens). There is in fact only one accentual feature in ancient Greek—or rather, one accent-producing feature—the contonation (a combination of rising *and* falling pitch, in that order) which Sidney Allen finds in the description of the Vedic cognate. The textual accent marks in Greek are placed on the mora where the voice rose—that is to say, the beginning of the contonation; the circumflex shows that the voice also dropped in pitch on the same syllable, but its rationale in Aristophanes' system of signs is to indicate that the voice rose on the first mora of a long vowel rather than the second. In other words, our texts do not bear accent marks, so much as phonetic marks of rising pitch (the grave sign indicates the suppression of this rise on the ultima, because the contonation could not be completed within the word.) But accentual prominence could also occur on the syllable following the mark, where the pitch fell. Because the contonation is mora-based, and not syllabic, it can be placed over adjacent syllables, and it is quantity that determines which syllable is the most prominent. The placement of the contonation over different quantitative environments would have produced different emphases, most notably the accentual prominence of an unmarked down-glide in pitch over the two moras of a heavy syllable, following the marked acute. (The rising pitch only ever occupies one mora.) Changing pitch and duration (quantity) are leading indicators of stress, and it is their combination in various permutations that determines the prominent syllable in a Greek word.

The locations of these emphases correspond to the rules for the location of prosodic prominence that Allen induced from his study of word placement at the ends of lines of stichic verse. Allen identified this prominence as 'stress', and did not connect this stress to the location of the contonation. My contribution was to show that there is but one

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system of accent in ancient Greek: stress marked by changing pitch (sometimes rising, sometimes falling), according to definite ‘rules of engagement’ for the lexical contonation and the combinations of syllables it infuses. Different sequences of quantities tended to emphasise either the leading (ὄξύς, ‘sharp’) part of the contonation, or, wherever possible, the culminative (βαρύς, ‘heavy’) part. To judge by the description of Vedic, and by Greek metrical usage, the circumflected vowels in Greek, which contain the whole contonation, were more ‘heavy’ in quality than ‘sharp’, but still audibly distinguishable from the post-acute falling pitch on a long syllable (e.g. ἄνθρωπος), which was simply ‘heavy’.

All the complications in Greek description follow from the fact that, according to the recessive rule for the onset of the contonation, no more than one mora may follow the down-glide (*svarita*). Because there are also exceptions to complete recession, this results in five (or most strictly, seven) permutations of prosodic prominence: oxytone on antepenult [ἐλώρια], penult [λόγος] and ultima [λαοί], and barytone on penult (including properispomenon) [ἄνθρωπος, μῆνις] and barytone on ultima (including perispomenon) [ἰφθίμους, θεῶν]. In Latin the recessive rule for the contonation makes things very much simpler: where possible, the voice must rise in pitch on the second mora before the ultima (regardless of the quantity of the ultima), and drop thereafter. The traditional rules of accent, interpreted as stresses by the moderns but as Greek-style changing pitch by the Romans, follow: stress on the antepenult when the penult is short [*fácilis*], and stress on the penult when the vowel is long [*Cicerônis, râri*] (called ‘flex’ by Quintilian—see p. 77) or when the syllable is closed [*percúlsae*]. The only exception to the received rules is in natural Latin iambs: the recession of the rise and the length of the ultima in these cases induce prominence of the *svarita* there, uniquely among Latin word shapes (as in *canò*, *Aeneid* I.1).

Call it what you will! ‘Emphasis’ is at root a better word to describe the central phenomenon of accent or prosody, than Latin ‘ac-cent’ or Greek ‘pros-ody’, which imply something epiphenomenal. The thing to distinguish stress from other kinds of tonal emphasis is the diminution in value of the vowels in unstressed syllables; this feature is as conspicuous in English and in prehistoric Latin, with its initial stress, as it is absent in classical Latin and Greek. But all known human speech involves the distinguishing in some way certain syllables as against others in a collocation one might call a ‘word’. (More on ‘word’ later.) Unassumingly I presumed to connect emphasis at the semantic level with emphasis at the phonic; certainly in the case of poetry it is evident that words are so arranged that their prosodic patterns are brought significantly into relief. Possibly this is the source of Mahoney’s confusion at my use of ‘stress’, a term which is also used at the levels of both sound and meaning. At any rate, Mahoney makes some truly extraordinary claims, not just about Greek, but French as well. Though she thinks I have made a contribution to accent theory, and she cites Allen (who formulated stress theory for Greek) and Devine and Stephens (who describe Allen’s formulation of prominence rules 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’), she is capable of claiming that the syllable marked with a written accent ‘was acoustically prominent in some way, and as best we can tell, this prominence took

the form of a change in pitch, *not* a change in loudness like the stress in an English word (or a Latin one).’ (M p. 9, emphasis Mahoney’s) (The point about my new synthesis is that under specific circumstances, it is the syllable *following* the marked one that was most prominent.) The claim about English (and Latin) is inept. I myself cite a collection of empirical studies (Hyman) that list the cues for stress in English, in descending order, as ‘changing pitch, duration, and intensity’ (see p. 82). In other words, changing pitch (rising or falling) and quantity supercede loudness as cues for stress *in English*. Also as features of [call it what you will!] in Greek. (And Latin.) The linguistics of English is not something native speakers (or classicists) are automatically qualified to lecture on.

Nor French, it would seem. ‘... [M]any languages have poetic forms that have nothing to do with stress. The most familiar example is modern French, whose meters are purely syllabic.’ (M p. 10) Unfortunately French is the only example Mahoney offers us, to sample this emphasis-free poetry. ‘The French alexandrine consists of twelve syllables, with obligatory word break after the sixth. There is no question of stress or “verbal accent,” because that is not part of the French language’, she says. It is just not acceptable that Anglophobic Francophones get to decide the way that French is described. It is of course exciting that descriptive laws can have political or cultural implications, but it is also extremely irritating, especially when false ones are promulgated in this way. Serbs and Croats, who use different alphabets, are all the same not allowed to be taken seriously by linguists when they claim that they speak different languages. (The case is even more incendiary for Hindi and Urdu.) If you think French doesn’t have stress (call it what you will), try emphasising the initial syllables instead of the final ones, and see where that gets you. You might still qualify to play Inspector Clouseau, but you’ll make a fool of yourself reciting Racine or Baudelaire. Mahoney continues, ‘Indeed, in formal poetry, every syllable counts, even those that would not be pronounced in casual speech.’ This is, of course, false. Unaccented final vowels stay silent in French verse (as does the third plural ending -ent), unless there is an initial consonant following—in which case the convention is to pronounce a feminine ending (as we understand the term in English), without shifting the normal stress (e.g., FEEH-ye for *fille*—note how absurd it would be to claim that the two syllables are equally stressed). Here are where the emphases (call them what you will) fall, on my reading of Mahoney’s example from Racine:

Cet hereux **temps** n’est **plus**. || Tout a changé de **face**
 Depuis *que* sur ces **bords** || les **dieux** ont envoyé
 La **fil**le de Minos || et de Pasiphaé.

The French alexandrine does always have a word-break after the sixth syllable, but the way to understand this musically is that the sixth syllable is stressed (or emphasised, or what you will; it walks like a duck, and quacks *comme un canard*). But each hemistich typically has two stresses; one might quibble with my choices, but not with the fact that there is a first stress in each hemistich prior to the cadential one. (I have used italic rather than bold where I would stress only mildly.) The six-syllable hemistiches can divide 3-3 or 2-4 or 4-2, but they must divide, and the division is marked by

whatever it is the French can describe as ‘emphasis’, without stressing themselves out. There can even be 1-5, as in the final hemistich of Baudelaire’s *Harmonie Du Soir*:

Ton souvenir en **moi** || **luit** comme un ostensor!

Note that there is no feminine ending in *comme* here, so it is effectively monosyllabic; but there would have been if a consonant had followed, and it would have become disyllabic, as here (from *Au Lecteur*):

Comme les mendiants || nourrissent leur vermine.

(*Nourissent* before *leur* also shows the rhythmic feminine ending; *vermine* does not.) The reciter tends to stretch and squeeze syllables in order to equalise in time the segments of the hemistich—even in the dramatic 1-5 case above—*luit!*—and the result is a characteristic pendular isochrony.

Mahoney refers to the new theory of the Greek accent as a ‘lemma’ (M, p. 2) in the book’s argument, but this is formally incorrect. I explain why a theory of linguistic stress derived from a specific metrical database should not be presented in isolation, when it is possible in Greek verse for metrical ictus and lexical accent to determine each other mutually (pp. 19-20). The analyses of Homeric texts that follow the presentation of the theory are not deductions via a lemma. They are in fact demonstrations that a proposed descriptive law is in harmony with the phenomena. Mahoney refers to these demonstrations as ‘twenty-odd pages of “prosodic charts”’ (M p. 12). ‘By “rhythmic-prosodic agreement” (the scare quotes are David’s), he means an *oxus* or *barus* accent on the first syllable of a foot.’ Of course she is wrong about my meaning (I myself, for obvious reasons, use ‘ictus’, ‘arsis’ and ‘thesis’, and ‘step’ as analytic elements of the concept ‘foot’; not ‘syllable’.) It is unfair to simplify the core of my book in this way, apparently in an attempt to trivialise it. Mahoney ignores the discussion preparatory to the presentation of these charts (pp. 111-15), where I take some pains to define exactly what should be meant by ‘agreement’, culminating in a table (113). There is a careful discussion of expectations and even predictions of results, consonant with a proposed descriptive hypothesis.

Mahoney begins her review by claiming that I *deduce* the ‘verse form of Archaic Greek epic ... from a dance step.’ There is no deduction. Deduction is very rare in descriptive work: induction and inference are the modes of operation. From the point of view of logic and deductive science, every descriptive argument engages in circular reasoning. Why is X a law, rather than Y? Because X seems to account for the phenomena more broadly and elegantly than Y. Why do the phenomena behave in this way? Because X is a law. This is the style of argument for ‘David’s Law’ of classical accent, as it is for ‘Verner’s Law’ or ‘Porson’s Bridge’. There is no substitute for judgement in deciding for X over Y, and little or no serious theory about how to educate this thing called ‘judgement’. I would encourage readers to use their judgement when they consider the discussions of the music in the passages from Homer analysed in my Chapter 4, ‘The Form of the Hexameter: The Origins of Caesura and Diaeresis’. There is either

something revealed there, or there is not. Mahoney herself does not so much dispute the results, as ignore them and their implications.

It is hard to make sense of how Mahoney intends these quotations, or what is supposed to be the power of the sting in the tail:

The mid-line caesura, [David] concludes, “must be seen as an automatic consequence of the desire to accent the thesis of the third foot” (p. 119). “Caesura results from the prosodic shapes that produce a sense of agreement in ancient Greek, a concrete musical motivation; not from a need to pour words into metrical moulds inherited from some unknown tradition” (p. 119), he goes on, ignoring the idea that the tradition, the words, and the “metrical molds” grew up together. (M pp. 12-13)

Of course I do not ignore this overtly romantic idea (tradition in epic is exactly what scholars make of it—this is why this ahistorical, penumbral idea is so attractive throughout the academy). I prove the association asserted here to be *fallacious* in the context of Homer. I shall stress the logical flaw in this response, as it is of a different order from erroneous description or comparison.

That first sentence of Mahoney’s is, unfortunately, typical of a kind of logical casualness that it is tempting to associate with entrenched orthodoxy. As I considered how to respond, it began to appear that virtually every sentence in the review stood in need of serious correction. Obviously if I had sat down to respond at this level, I should have produced something extremely tiresome, serving of nobody, not even the self. It appears that scholars who have engaged in colometry and comparative metrics, including the 20th century icons who still invigorate Mahoney and a new generation, show a poor understanding of the methodology that grounds their claims, and hence they are led to make wildly indefensible ones. I shall focus on this broader problem as a way of responding constructively to the review.

Mahoney is an honest colometrist, but she is disingenuous with her readers about the contentious relationship between colometrists and metrists. Despite my expressed opinions on Hephaestion, she appears to cast me as a metrist (M p. 2). One presumes this is because the metrist archenemy is the enemy she knows. Probably the best source still for a discussion of the controversy can be found in Thomas Cole’s *Epiploke* (Harvard 1988). My own opinion is that both kinds of metrician have been

... studying [ancient] metre without regard to accent and harmony—an approach which would be considered self-contradictory in any other context—and with little concern for the practical implementation or performance of their metrical schemes. Of course it is my claim to have rediscovered the essential harmonic constituent (in the nature of the verbal accent), which combines with metre (interpreted as dance) in χορεία. But one wonders sometimes how much metrics since Hephaestion has ever felt the loss of its musical function and aesthetic

content. Turf wars between metrists and colometrists are neither inspired by nor have any bearing on the harmony and the dance. They have no bearing on reality at all. (pp. 247-8)

Mahoney speaks of a 'currently accepted theory of Greek meter', where 'the "feet" identified by Alexandrian and late antique theorists (units of two to four syllables similar to the units used in analyzing English meters) have no independent existence in Greek verse.' (M p. 1) (This is the first of a number of instances I shall point out, where Mahoney requires that we disregard ancient descriptions of the ancient poetic phenomena in favour of the disembodied mathematics of 20th century colometrists.) She concludes, 'All of this is well known and uncontroversial.'

Oh really.

Cole himself is to be commended for bringing a rhythmic sense to the mathematical schemata, and discovering something beautiful: ἐπιπλοκή, the 'interweaving' of complimentary rhythmic movements. Greek periods show, even at the metrical level, a characteristic modulation from descending to ascending rhythm. The feet pair off by the reversal of the 'up' and 'down' elements, the arsis and the thesis—literally by shifting one long from the front to the back: trochee/iamb, dactyl/anapaest, cretic/bacchius, choriamb/ionic, and at the colometrist's scale, iambochoriambic and anacreontic. Cole, like Paolo Vivante, looks at whole lines (or periods) to find their musical sense, following an instinct that well serves poetries other than the antique Greek.

What follows are 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 claimed to have been done for any other stichic line, ancient 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; 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, the development is at least plausible. But to claim that the ancient 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 of Greek 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. (pp. 241-2)

Mahoney offers this chestnut: '[David] goes so far as to try to revive the dreadful old term "logaoedic," "word-song," a term used by later Greek grammarians apparently to refer to meters that seemed closer to prose than to proper verse ...' (M p.15) Here is instance two: we are supposed to dismiss a term produced natively in response to a style of local poetry, in favour of a 20th century academic denomination—despite the fact that 'Aeolic' is not meant to refer either to the dialect of that name, or to the harmonic mode. One is reminded that a crater on the moon was called 'Plato'. Best not leave it to the Moon-Men to tell their own story. (Feel free to suspect that Classics departments have authority issues with their reason for being.)

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 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 stichic line.**

I am accused by Mahoney of arguing 'for almost exactly the inverse of the currently accepted concept of the development of Greek poetry.' (M p.2) The claim that there is an accepted concept of the development of Greek poetry is a simple lie. It is not a misapprehension of subtle scholarly disputes; it is the *lie direct*. There is something to note here about this reviewer's standards. Homeric and Hesiodic poetry came first. Every extant lyric poem came later. It is a sign of desperation for someone to claim 8th Century dates for lyric verse (M p. 4), when no basis for such dating is given. Mahoney sees fit to point out that 'every culture seems to have folk-songs', and mentions the 'well-known Rhodian Swallow Song', quoted in Athenaeus, which, it is claimed, 'must have existed earlier'—one presumes, than the 8th Century—a thousand years (!) before Athenaeus himself. Mahoney neglects to mention that Homer himself depicts the 'Linos' song being performed on the shield of Achilles (*Iliad* 18.569-572), complete with a phorminx-playing child singer and yelling dancers. But it is obscure how Mahoney means to connect folk songs with our authored lyric texts.

We do not know what the precursors of Sappho, Archilochus, and Stesichorus were doing, nor do we know what music filled the halls of Mycenae. What we do know, however, is that the lyric verse forms used

by the archaic poets are strikingly similar to the verses of the Rig Veda.
(M p. 4)

(More about 'strikingly similar' later.) Well, what is the connection supposed to be between undateable folk songs and ancient Greek choral lyric, on the one hand, and the Rig Veda on the other? Here are the facts about our Greek texts: lyric followed epic, 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 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 theorising, perhaps my theory will be entertained at least with the suspension of incredulity which once greeted Milman 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 mathematisation of symbols that were intended to record rhythmic performance. I illustrate in the book in various ways how disregarding rhythmic realities for the sake of metric and colometric mathematics leads to silliness. About Nagy's 'derivation' of the hexameter, for example:

The derivation of the hexameter from the pherecratean ... has more to do with mathematics, than it has to do with either the linguistic quality of poetry surviving in these metres, or with their original, physical, orchestric reality. To begin with, the pherecratean is a catalectic or cadence metre in relation to a glyconic ... It may at first sight make sense to say that a pherecratean 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/pherecratean 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 pherecratean 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 pherecretean. This is a fact of dance and rhythm, in no way analogous to a linguistic fact. 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). (pp. 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.*

Diachronic reconstruction requires the establishment of elementary units. In order to do this, linguists must first isolate those features and bundles of features that constitute the objects of the science and the spatio-temporal particulars of the history. This does not mean that phonemes or morphemes are easy to define; neither are they entirely cognitive phenomena nor are they entirely phonetic. Yet they are, all the same, real and definite, el-em-ents in the root sense: the a-b-c's have been paradigms of intelligibility and trans-temporal identity since the invention of alphabets. (The title of Euclid's famous book, for example, could be translated 'Letters'.) (pp. 6-7)

But there are no 'metremes', and (thankfully) no attempt to define such units on the part of would-be comparative metrists; yet

... without such definition, can one even begin to make sense of their changing over time according to rules—that is to say, of diachronic accounts with respect to them? (p. 14)

So there is *problem the second*: **comparative metrics, insofar as it presumes to wear the mantle and methodology of historical linguistics, is 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 constitutes 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. (p.164)

But when we turn to one of their proposed examples, we turn from pseudoscience to simple nonsense. Mahoney actually reproduces the comparison which fueled the colometric fantasy, but which I reproduced in order to debunk it (see p. 240 and M p. 4). She is right to point out that I should not have saddled it on M. L. West, who was merely a transmitter of the nascent tradition. But it was simply unfair of her not to include my whole analysis. Apparently, ‘to most observers the salient fact about the glyconic is not that “it contains (*must* contain) a dactyl” (p. 240, emphasis David’s) but that it always contains eight syllables, and allows variation only at the opening of the line ...’ (M p. 4). I wonder what such observers would say about eight apples next to eight oranges. If one weren’t actually learning how to count, I suspect one would most notice the differences, rather than their eightness, or the fact that they all had stems. **The Sesame Street test fails: these two things (x x x x c _ c _ and x x _ c c _ c _) are not like one another.** As I mentioned, Mahoney calls these verse forms ‘strikingly similar’. This is *problem the third*. The invariant part of the glyconic has six elements, the other 4. If you think there is nothing worth noticing in the difference between a half and three quarters, why not pay more tax. The country needs people like you.

But simple similarity is not necessarily the phenomenon that drives a reconstructive comparison, *pace* Mahoney et al. It took long years of exploitative 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? That they are both lines of ‘eight syllables with a variable opening and a fixed cadence.’ (M p. 4) But 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 (‘Deserts of vast eternity’). 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 claimed to have 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. (Mahoney also endorses an absurd claim, in passing, that the twelve-syllable Sanskrit jagati is ‘the direct ancestor of the iambic trimeter’. Comparative reconstruction does well enough to establish, mutually, proto-forms and cognates; parental claims are out of its purview. Apparently Ferdinand de Saussure’s criticism of the fetishisation of Sanskrit because of its alleged age has not been heeded. See my pp. 4-7. This unwarranted historical claim is particularly galling in light of Mahoney’s scornfulness in the face of my perfectly reasonable suggestion that Panini’s work may have been available in Alexandria, to inspire Aristophanes of Byzantium to mark the accents in the way that he did—exclusively in terms of the position of rising pitch,

despite the obvious fact that the Greeks, unlike the Indians, also described a ‘heavy stress’ [βαρὺς τόπος]. See M p. 13: Mahoney’s claim that ‘analogy has turned into argument’ is simply bizarre. My suggestion on this matter of Panini’s influence has remained—and remains—a parenthetical suggestion.) ***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 juggernaut that even took over Greek versifying?)

But we have not yet got to the salient point in my own analysis, which is brushed aside without comment by Mahoney. As she points out herself, in the Sanskrit line, the second and fourth syllables (in the ‘variable’ part) are ‘usually long’. There is no subtlety here: the Sanskrit line is iambic, like Andrew Marvell’s tetrameter. The invariant part of the glyconic, however, always contains a dactyl abutted by a cretic.

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 ... (pp. 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’. 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. (p. 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 *syrtós* 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 the 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*.

The climax to Mahoney's review reads, 'David's thesis ... that Greek meter derives not from an ancient verbal tradition but from dancing—is ultimately unconvincing.' This conclusion would itself be more convincing if the colometrists could supply an alternative source for the dactyl. As Aristotle testifies, the natural speech rhythm of Greek is iambic; dactyls are not suited either to Greek or to Indo-European. At least Meillet recognised that it was an innovation. Where did it come from?

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. It is time for all concerned parties to realise that, at least in American Classics, the emperor has no clothes.

Here is Mahoney, speaking 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. (M pp. 1-2)

That words and rhythms grow up together in poetry is a pleasant and natural enough assumption, which is also generally true. It is, however, absurd 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 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 pp. 158-9) He gives

numerous examples of numerous forms of phonological and even morphological adaptations to metrical constraints in Homer ... It is therefore appropriate to speak not just of the alteration of forms (lengthening or shortening of syllables) due to metrical pressure, but also of poetic neologisms and the creation of forms—some of them redundant—and hence of the creation of an epic diction in Homer. The redundancy in some cases of Homeric coinage with respect to unmetrical equivalents [e.g. forms in -ιζω to substitute for forms in the

paradigm for -εω verbs] distinguishes it from the Shakespearean kind of coining: the Homeric creativity is, at least in part, necessitated by the verse form. Such a diction—as well as the very notion of metrical constraint—arises immediately and naturally from the adaptation of speech to a fixed dance rhythm.

The claim 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; 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 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. (pp. 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:

Oralists who work from the premiss that the dactylic hexameter is a product of the Greek language, ought to consider how expensive this assumption is for their 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. (p. 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 can no longer 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; 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 supposed child is a monster! As a corollary, 7) Homeric diction, and therefore Homeric poetry, is non-traditional.

Mahoney claims that 'David's discussion of the relationship between formulae and metrics (p. 163-167) is simply confused.' (M p. 14) This is all that she has to say about these pages. The reader will observe that I have quoted extensively from them above. Of course I must take the blame for any lack of clarity in my presentation. But when a review dismisses out of hand what in fact constitutes a critique of the orthodoxy that the reviewer, apparently, intends to protect from criticism, there has occurred too blatant an abuse of academic discourse, to avoid the charge of intellectual dishonesty. I cannot simply require a charge of 'confusion' about this.

Mahoney follows her announcement of the paternity of the Sanskrit twelve-syllable jagati over the Greek iambic trimeter, with: 'The idea that the dactylic hexameter is the fundamental verse form of Greek, then, is ahistorical.' (M p. 4) The 'then' has no inferential value. She goes on: 'David is overly influenced by the theories of Plato and Aristotle, even though he realizes they are not contemporary with the poetic tradition he is trying to re-construct (p. 26).' This would be instance number three: we are supposed to disregard direct and compelling evidence of eyewitnesses of both the Greek language as a living thing, and of the performance of Greek poetry as a still living thing—Plato *and* Aristotle, for God's sake!—in favour of the theories of twentieth century theorists, who, apparently, Mahoney recognises as somehow meta-contemporary with the poetic tradition they are 'trying to re-construct'. She quite misrepresents what we know about the reported reality of epic performance, by the way. Plato writes a wonderful dialogue about Ion. He was a *rhapsode*. This word does not occur in the vocabulary of Mahoney's review. He did not perform Homer with a lyre, or with dancers. Such showmen were usually depicted with a staff as their only prop. All the same, Mahoney asserts: 'As far as we know, epic is a narrative form, performed by a

single singer, accompanying himself on a stringed instrument of the lyre family.' (M p. 3) In fact there is no evidence whatsoever for this kind of epic performance outside of the epics themselves. (And alongside these depictions in Homer, are of course depictions of the same phorminx-playing bards [Phemius and Demodocus] accompanied by dancers during their story-telling.) My claim that Homeric poetry is 'dance music' does not require that it be performed by dancers, any more than does such a claim about the works of Bach or Mozart, who composed in a variety of isometric dance rhythms.

In point of fact, there are no 'theories' about the practicalities of Greek music and poetic performance in either Plato or Aristotle. Plato defers such issues to an expert sophist, Damon, in the *Republic*. Of course he draws on Pythagorean theses, but this is not the same thing as to originate theory. Aristotle's *Poetics* should really have been called 'Mimetics': almost immediately in that treatise the question of 'making' devolves onto the notion of 'imitation'. There is far more of interest to be found about 'making' in his *Physics* than his *Poetics*. What is precious about the evidence I draw on is that it is meant to be paradigmatic for other purposes: facts and theories about music are presumed to be well known, and hence readily illustrative for these authors about more abstract kinds of knowledge. Hence there is, designedly, nothing controversial about the descriptions. On the other hand, precisely because the givens are well-known to the prospective audience, there is an elliptical quality to the allusions, and a modern must do his best to fill in the gaps. But it is because Plato and Aristotle do not trouble themselves with *theories* about music and performance, that their illustrative *data* on music and performance are likely to be uncontroversial. (The same cannot be said, of course, for their metaphysical views.)

Mahoney takes me to task on my interpretations of passages from the *Epinomis* and Aristotle's *Metaphysics*. These are both involved philosophical texts, and her approach to them will be seen to be wanting. But this does not excuse the ways in which I also have misled my readers, and she does a service in pointing this out. It is indeed misleading to imply that the writer of the *Epinomis* himself adds the numbers 9 and 8 to total 17, and so establish beyond doubt that he is referring to the epic hexameter when he refers to the 'dance of the Muses' (see M p. 5). It is Aristotle who does this, when he says that the 'means are (on the one hand) nine and (the other) eight, and the *epos* is seventeen, equal in number to these'. It could also be disputed that Aristotle's comment is a direct reference to the *Epinomis* passage (although Mahoney does not explicitly take issue with my claim on this). Both passages are elliptical, because both authors assume that the reader will know what is being talked about with only a minimal prompt. But I shall stick by my conclusion about the 'dance of the Muses' in *Epinomis* 991b:

... this dance had a significant division in the sense of its steps, yielding sub-periods of nine and eight. Only if this were so could the writer have linked the double ratio and the Muses' dance through the mysteries of nine and eight, and the reader have taken his allusion.

The point about the means is that 9 and 8 are, respectively, the arithmetic and harmonic means between 6 and 12, the two numbers that are explicitly mentioned in the passage. But 6 and 12 have no intrinsic significance except that they express the ratio of doubleness; the sixth multiple became well known because it is the first that allows the means to be interpolated as whole numbers. The sequence 6, 8, 9, 12 had come to acquire a near-mystical significance among Pythagoreans, in part because they express the ratios of string lengths that produce musical consonances (the octave, fifth and fourth). But for the *Epinomis* author, the octave itself is only one of the expressions of doubleness that impress him. Astoundingly, Mahoney claims that ‘the whole passage in the *Epinomis* is about the number twelve.’ (M p. 5) It is difficult to see how such a sentence survived the proofreading. One has to hope that she was not serious. The passage is manifestly about the double, both the powers of two and the internal means within the ratio of two (which is the ratio of the octave). She reports:

Instead of dance, the passage is generally taken to refer to divisions of the octave into smaller intervals ... Moreover, the Greek word that Taylor translates as “choir” and David as “dance” is χορεία, a broader word than merely “dance” and certainly not specifically referring to any one dance form. Finally, the idea that the phrase “dance of the Muses” must refer specifically to the epic hexameter, as opposed to any other form of song, poetry, or dance, is speculation, not to say fantasy. (M p. 5)

There is a willful surge of rhetoric here, worthy of the *Epinomis* writer himself. The translation ‘choir’ is highly misleading for χορεία, because the English word no longer has associations with dance, but does have specific associations with the multi-part music of modern tonal harmony. I quote Giovanni Comotti in the book:

‘the Greeks and Romans did not know harmony, in the modern sense of the term, or polyphony; their music expressed itself through pure melodic line alone. The accompaniment faithfully followed the development of the song, either in unison or at the interval of an octave. Only after the fourth century B.C. do we know of songs accompanied at an interval of a fourth or a fifth.’ (see p. 92)

But the most misleading aspect of the translation ‘choir’ is that it connotes a group. The word *never* refers to a group. Χορεία rather refers to an activity. It is true that there is some breadth to the term—Plato uses it to describe the movement of the planets, for example, presumably because they show periodic retrogression, like the modern Greek *syrtós* round—but the reader may satisfy himself, by consulting Liddell and Scott’s entry, that, *pace* Mahoney, there is no sense to it broader than ‘dance’. (There is no such thing as ‘mere’ dance.) Context ought to be crucial in a classicist’s judgement: there is no question in either text of a ‘theory’ of the hexameter, but rather the use of the hexameter as a most obvious exemplar to the intended audience. The author of the *Epinomis* connects *specific numbers* to what he calls the ‘dance of the Muses’. (Surely the genitive *should* most naturally be taken to specify the term ‘dance’ in some way.) Either these numbers relate to any (and hence every) Greek dance—a notion that Mahoney

does not find fantastic—or they relate to a particular dance so widely known to belong to the Muses that it needs no further specification. The second option is the only one that takes seriously the illustrative intention of the writer.

But it is true that in my own reasoning it was the passage from Aristotle that sealed the issue, that we were in fact dealing in both cases with a previously unrecognised description of the epic movement. Had I to write things over again I should have made this clearer—that if there were any doubt about the import of the *Epinomis* passage, Aristotle resolves it. Mahoney says:

In this passage, however, Aristotle is quoting the Homerists who “observe small similarities but overlook large ones” (David’s version, p. 96), and saying that it is *they* who divide *epos* into nine and eight and connect that to the means between six and twelve. (M p. 6, emphasis Mahoney’s)

But there is no quoting of anyone, and especially not the Homericists of old, who are not in fact the subject of the relevant sentence. It is dishonest to suggest that my reading could be taken in this way. Here is the translation that Mahoney herself quotes:

These people are like the Homeric scholars of old, who see small resemblances and overlook important ones. Some people go on to describe many such cases ...

Even in Julia Annas’ version, it is evident that ‘these people’ are not the ‘Homeric scholars of old’, but *like* them; and that ‘some people’ (or better, ‘some’, *τινες* in a partitive sense) are a subset of ‘these people’, *not* of the old Homerists. As if Homerists have anything to say about arithmetic and harmonic means! W. D. Ross suggests that these ‘Homericists’ were allegorisers (see pp. 96-7). At any rate, the project of *Metaphysics M* and *N* has nothing to do with them: it is a debunking of the mathematical metaphysics of contemporary Platonists and Eleatics. The general pattern of the critique has to do with how their theories descend into what we would now call numerology. But there is also direct critique of their first principles as such. In point of fact, in the comment here there is a backhanded praise of Homer, as the obverse of the denigration of the Homericists; that is, one should dearly like to know what these neglected—but, presumably, true, and presumably large—similarities (or ‘important resemblances’) are, in Aristotle’s opinion, within the Homeric poems. But Aristotle’s critique of his contemporaries, and his analogy comparing them to the Homericists, is, in my opinion, correct: after a number of genuine observations of the double and its marvelous powers and roots in nature and the world, the writer of the *Epinomis* concludes with this mumbo-jumbo about a division in a dance. There was no question in my book of vouching for either author, but of mining for data. This is what Mahoney sees fit to make of this passage (note the ‘in context’): ‘In context, what Aristotle says is roughly “some Homerists divide the *epos* into nine and eight, but this is trivial or silly.”’ (M p. 6) This is not a possible reading of the Greek, let alone a careful or a scholarly reading. It is difficult to name exactly who the intended Platonists are—as

difficult as naming the author of the *Epinomis*—but if Aristotle is not referring to this author, he is referring to this author’s student.

At least Mahoney recognises what the passage is describing, despite the fact that she misattributes the source, and appears to be unaware of any of the context of *Metaphysics* M and N. As I have said, Aristotle is not out to assert or even to report anything original about the ‘epic hexameter’ (tr. Annas, see M p. 6), but to illustrate a kind of intellectual wantonness on the part of contemporary metaphysicians, who would cite the mystic Pythagorean numbers 9 and 8 as constituents of the *epos*, because they add up to 17, when their real significance is as means in the double interval 6:12. But along the way we find out (as though overhearing) that the *epos* is seventeen, stepped on the right with nine syllables, but on the left with eight. Mahoney makes an original suggestion, that perhaps the counting is of alternate feet, and so at the end of a ‘dance line’, the right foot has fallen nine times and the left eight. But this does not well accord with the resemblance to means, which are, in essence, divisions. And there is more still to say about this later, in favour of the *Epinomis* author.

But let us note that the *epos* is a seventeen part thing that one ‘steps out’. Mahoney comments, ‘David is entirely correct to note that this is far more likely to refer to physical movement than to scansion: it’s a perfectly concrete verb.’ (M p. 6) Earlier she had asked,

What is epic? The Greek name for the form comes from ἔπος, “word” or “utterance” (cognate with Latin *vox* and English “voice”). That is, to the Greeks epic is a form made of words.

She does not, however, see fit to revisit this statement in light of Aristotle’s testimony. (The cognate relation across languages does not allow one to infer, by the way, what a particular form might mean to a speaker in a given language at a given time; consider *attendre* in French versus ‘attend’ in English.) In the book I draw an opposition between *epos* and *muthos*, two words often translated ‘word’ in Homer’s usage:

An ἔπος is therefore *numerable*, internally as to its metricality, externally as an element in a catalogue. Most generally, ἔπος is a speech act viewed in all its qualities, such as its syllables and its metricality, that do not touch on its meaning. Hence words are characteristically winged, not specially so; they are all and equally substances that direct themselves through air. (pp. 185-6)

This is not the opposition that Mahoney wishes to draw, however; it is, after all, synchronic in Homer. She rather draws a later contrast (but contemporary with Aristotle’s statements) with μέλος, a term said to be synonymous with lyric poetry (M p. 3). ‘Earlier, in the *Iliad* and the *Odyssey*, poetry is called ᾠοιδή, and the word ἔπος refers to words in general ...’ Hence Mahoney is lining up ἔπος / ᾠοιδή as a contrast within Homer, and ἔπος / μέλος as a contrast in ‘classical Greek’. It looks as though she is reaching for Nagy’s speech/song distinction; see my p. 44, where I comment:

The substantial empirical basis of this distinction can be expressed as a formula: whereas the epic, elegiac, and spoken tragic verses were such as could *accompany* dance, lyric verses were *danced*. Such a distinction accounts in particular for the lack of ‘metrical substitution’ in the lyric forms, which would necessitate a different physical movement; whereas the stichic forms could indulge in the syncopations of an accompanist.

The fact is that the concept ‘word’ remains extremely hard to define. It does not help matters to say that ἔπος means ‘word’. Even if it is true that it is cognate with *vox*, this in no way decides a question of speech versus song. Is ‘lexeme’ a word? Or is *lex* (leg-) the word, and the rest a suffix of some kind? When Dionysius of Halicarnassus attempts to describe word-level accent, he uses this periphrasis for word: λέξις ἢ καθ’ ἐν μόριον λόγου ταπτομένη (see p. 61). (I leave it to the industrious to try a translation; we are lucky to be able to trace the style to a definition in Dionysius Thrax. The trick, I think, is not to try to cover up the near-circularity in the use of λέξις and λόγος.) Not only does Aristotle describe the *epos* as a thing one performs by stepping; in the *Poetics* he observes that when we speak hexameters, we ‘transgress’ (ἐκβαίνειν) the normal ‘conversational *harmonia*’ (see pp. 160-1). It is natural to conclude that there was something peculiar about the *epos* as both song *and* dance, and that it was possible to perform it in ways which suggest that it does not fall neatly under an opposition between speech and song (see my discussion of passages from Hesiod, pp. 162-3).

It is fair to say, however, that there is an aspect of the *Epinomis* passage that Aristotle does not appreciate. I have made the complete argument elsewhere, at some length. These papers (‘Plato and the Measure of the Incommensurable: The Paradigms of Theaetetus’ and ‘The Mathematical Meaning of the Indeterminate Dyad’) have been published only in the *St. John’s Review*, which is not widely available; I would be happy to e-mail a .pdf file (repackaged as ‘Plato’s New Measure: The Indeterminate Dyad’) to interested readers. The point is that the arithmetic and harmonic means were not the most interesting means within an interval for Pythagoreans and other geometers. The geometric mean, a ‘forbidden’ or ‘irrational’ (ἄρρητον) length in the double interval, as well as in the ‘golden section’, lay in between the other two. Geometrically this length is the side of a square double the unit square. The modern designation for this entity is $\sqrt{2}$. As one interpolates series of arithmetic and harmonic means within the 6:12 interval, one generates closer and closer over- and under-estimates of the geometric ratio. The process can be shown to be exhaustive—that is, that the approximations approach closer than any given difference to the true quantum (see Euclid X.1); but of course the geometric mean always stays in between the other two. One can readily see this when one considers that in a proportion, the product of the means equals the product of the extremes: $6*12 = 8*9$. Hence the rectangle contained by the means equals the rectangle contained by the extremes, but it is much more square (or ‘like’ as opposed to ‘unlike’). If one then interpolates the arithmetic and harmonic means between 8 and 9, which are $17/2$ and $144/17$ ($6*12 = 8*9 = 17/2*144/17$), one is already producing a figure that is virtually square, of sides 8.5 and approximately 8.47. Because of the original terms of the proportion, these are approximations of what we would call

$6\sqrt{2}$. The never ending series of paired arithmetic and harmonic means that exhaustively approximate the (sometimes) irrational geometric mean in an interval, was, I argue, the referent for the term ‘indeterminate dyad’, associated with Plato, as a new and fundamental principle in measurement science alongside the ‘unit’, one which can be shown to contain a solution to the problem of incommensurability. I show in detail how *Metaphysics* M and N demonstrate that Aristotle was not familiar with this construction, and that this leads him to throw out the genuinely insightful formulations of Plato along with the befuddled numerology of the later claimants to Platonism.

At the opening of the passage that culminates in the ‘dance of the Muses’, the *Epinomis* assigns pure arithmetic to the education of the young, and then a science ‘ridiculously’ called ‘geometry’, but which is ‘really a manifest assimilation [ὁμοίωσις, ‘making like’] to one another of numbers which are naturally dissimilar, effected by reference to areas.’ (990d, tr. Taylor) This appears to be an explicit reference to the construction I have described, where a rectangle (of dissimilar sides) becomes more and more square. The novelty of the thing seems to drive the writer’s zeal—even ‘geometry’ is yesterday’s news in relation to this mensural arithmetic:

Now to a man who can comprehend this, it will be plain that this is no mere feat of human skill, but a miracle of God’s contrivance. (tr. Taylor)

The key phrase in our passage is this:

τούτων αὐτῶν ἐν τῷ μέσῳ ἐπ’ ἀμφοτέρα στρεφομένη

Taylor takes the referent of the demonstrative to be 6 and 12, and hence ‘the mean between these terms [6,12], with its double sense’. But the immediate references for this demonstrative are the means that have just been mentioned, the arithmetic and the harmonic. The mean which ‘turns about to one and the other’ (this is literal, Taylor haplessly glosses, ‘with its double sense’) would be the geometric mean in its successive, paired, over- and under-approximations. One of the gifts this mean is said to grant is commensurability (σύμμετρον—the side of the double square, $\sqrt{2}$, is famously incommensurable with the unit side). Aristotle is right to see a disanalogy when the geometric mean (square root) of the double interval is compared to the division in two parts, nine and then eight, of an *epos*; but it is also the case that he did not know what all the fuss was about.

Mahoney persistently confuses syllables and feet. She actually claims that I try to ‘get round’ a supposed problem that a Homeric verse almost never breaks between the third and fourth foot—that is, between the first nine and second eight steps (M p. 7-8). (For a discussion of the aesthetic motivation for this phenomenon, see p. 120.) What Aristotle offers (and the *Epinomis*) is a *dance* that divides between the first three feet and the last three—not a *line* that does this:

We do not know why the dance of the Muses should be divided in this way, because our informants do not tell us, nor do these passages in any sense paint a picture of the performance. But the fact of this reported division in the dance, which does not correspond to any observed caesura or diaeresis, is indeed valuable for our analysis of the dynamic structure of its verbal accompaniment. (p. 100)

If a dance divides between the first three feet and the last three, how will a verbal accompaniment reinforce this prosodically? By stressing the thesis of the third foot. Where can you find a Greek barytone for this purpose? Only on the ultima, or on the penult with a short ultima. Hence if the Greek language is used to reinforce the movement of this dance, there will usually be a word break either immediately after the third thesis, or after the first short in the arsis. (This would in turn introduce a dynamic in the verbal accompaniment, in that its rhythm would tend to modulate from descending to ascending—a rather simple notion that Mahoney doesn't seem to grasp; see M p. 7). What this model, of a fixed dance being accompanied by Greek, helps to explain, are: a) why there is a word break in the accompaniment during the third foot and b) why this word break did not register in the ancient world as a caesura. There was no 'cut' in the dance step! The first mention of the caesura is in Aristides Quintilianus (3rd Century CE). Were the older ancients deaf and blind? Obviously not. The notion of a 'cut' in the foot would have made no sense to people still familiar with the hexameter dance. The foot was performed seamlessly by the feet: the words did their own thing, matching the dance's emphases, moving from syncopation to agreement and back again. Word breaks in mid-foot, at emphatic points, were an automatic consequence of verbal stress on the foot's thesis. They would not have been seen as 'cutting' the foot. Rather, prosodic emphasis would have been felt to be *reinforcing* the foot. The situation is not like the familiar one in English and most other poetry, where accent and metrical ictus are both generated by language; it is what we find in operatic settings, where emphatic syllables are placed after the bar line, without thereby splitting off the rest of the measure. I quote from a 1906 description of the Faroese heroic poetry danced in the round:

'What, moreover, should be well looked after in the ballad singing is to "get the word under the foot", as the old ones used to say. One gets the word under the foot when one stresses one word or syllable at the same time that one steps along with the foot.' (p. 43)

Mahoney says, '[t]he line does not divide into nine positions followed by eight, but into seven plus ten or eight plus nine.' (M p. 7) This is obviously correct. It is the *dance* that the ancient testimony describes as dividing between nine and eight. A consequence of the verbal line reinforcing this dance prosodically, given that the line is in Greek, is that *it* must divide after seven or eight; but it is for good reason that the ancient world did not recognise this division as a 'caesura'.

On to miscellaneous misrepresentations. Ostensibly paraphrasing me, Mahoney says:

In other words, the repetition of “fleet-footed Achilles” or “Diomedes of the great war cry” is a patterned recurrence exactly analogous to the recurring dance steps, which are already reflected in the recurring rhythm of the lines (p. 142) (M p. 13)

I cannot find anything like this on the cited page; there is no use of ‘exact analogy’ and I honestly do not understand what she is supposing me to claim. What sense would it make for noun-and-epithet phrases to repeat, just because the dance steps repeat? How could anything new get said? Here is the closest passage:

The recurrence of lines and phrases is a spontaneity in the accompaniment of a round dance that is continuously repeating in its very structure. One does not need to explain the recurrence of words and melodies in the accompaniment of a round (such as a Christmas carol): *one would rather have to explain the lack of it.* (p. 142)

My paragraph goes on to develop the notion that recurring noun-and-epithet phrases become melodic ‘signature lines’ for their referents. This is the analogy I do draw, to a similar spontaneity in the compositional practice of modern opera.

Next: ‘David does not discuss other formulae, such as those that introduce speech.’ (M p. 13) I refer the reader to my critique of Parry’s article on ‘winged words’, pp. 148-50.

Next: apparently I ‘contradict the well-established idea that the strophes and antistrophes were sung to the same melody.’ (M p. 14) Pöhlmann and West only claim this for late settings (see my citation, p. 225). This is just after I demonstrate accentual rhyme (not just of prominence but of pitch contour) for the final periods of the strophes and antistrophes of Pindar’s *Olympian I* (see pp. 223-7, esp. 225). Mahoney neglects to mention this demonstration. Uniform melodic settings for all the strophes would of course negate this striking prosodic feature of the final periods of each of the paired ones.

Mahoney says: ‘Given that David derives epic from dance, and lyric from epic, it is perhaps not surprising that he claims that dance is not as important to lyric *choreia* as it was to epic.’ (M p. 14) This is as choice a piece of pseudo-logic as one could ever hope to see. ‘She came in a huff, and left in a carriage.’ I prefer not to summarise my longest chapter, ‘The Lyric Orchestra’. (Perhaps the title speaks for itself.) What apparently was too subtle for Mahoney was that epic and lyric had different relationships to dance performance, which bespoke a change in attitude. Mahoney had earlier (M p. 3) misleadingly quoted me as speaking about ‘the development of lyric out of epic’. This was a mid-paragraph shorthand for an opening sentence which read:

The development in relation to the round dance of so-called lyric composition out of epic has therefore nothing to do with the lyre or the singing voice, which are features of performance common to both; it has to do with the transcendence and mastery of the word over the dance’s

rhythm and repetition. (Note that this mastery does not involve the imposition of linguistic rhythms on the dance, but rather the control and sequencing of orchestric rhythms through the word.) (p. 221)

See especially my discussion of the different compositional vantage of the epic and the lyric poet in relation to dance movement, and the implications both for critical interpretation and performance (pp. 217-22). The following paragraph, a summary in the course of the discussion, should give the lie to Mahoney's bizarre distortion:

The solid principle underlying this analysis is that it is possible to reconstruct the metres of Greek lyric in terms of the simple dynamics of folk dance steps. Dactyls and trochees are living presences today in dances that are the direct descendants, perhaps the re-enactments, of the orchestras of the ancient world. In the re-enactment of music—as against the preservation of texts, and the transmission of languages—time itself stands still. There may therefore be a future for the science of ancient Greek metrics in the analysis of data from modern folk dance. What must remain speculative for now is my hypothesis about the seminal relationship between the dactyl and the cretic. I have suggested that all the complexities of Greek lyric, the multiplicities of form, depend ultimately upon the structures created as solutions to this question: how can one bring a dactylic (or anapaestic) run—a Greek round dance—to a rhythmic resolution? The epic hexameter, the dance of the Muses, represents only the first and most open-ended answer to this question. The greater physical and aural climax of the cretic and the bacchius and their expansions can be seen to be called forth by the intensity and the closure required of the round by the lyric poet and his mastery. (pp. 242-3)

I am breaking my promise not to get tiresomely into details about a myriad errors. So, finally: Mahoney claims that I over-interpret Thrasybulos Georgiades, in citing him in support of the idea that the 'original Homeric dance still survives in the modern world.' (M p. 8) Apparently, '[f]or Georgiades the idea that dance may have accompanied epic is a small side issue in a discussion of lyric poetry.' It is true that the book in question is devoted to an analysis of Pindar's *Pythian XII*, a poem that was known to have been danced. In the modern era, Homer is not commonly thought to have been connected to dance. But all the same, the chapter on Homer is no 'small side issue'; it is in fact the concluding chapter of the work. Mahoney excerpts Georgiades to claim that,

what he says is simply "The two phenomena, the old and the new, the rhythm of Homer and that of the round dance of modern Greece, testify to identical attitudes and have common roots". (M p. 8)

The gloss and the excerpt are dishonest. Here is the last sentence of the full paragraph; let the reader judge for herself:

What a concentration of meaning and of vigor this dance must have possessed in the time of its blossoming to persist with such tenacity *to this day*, to exercise such power over millennia! (emph. added; see my pp. 105-6)

Mahoney continues:

[Georgiades] then suggests that “tracing the hexameter to the rhythm of the *kalamatianós* poses the question as to what extent the epos was originally related to dance” (p. 140, a passage David does not quote)—if the rhythm of the poetry is similar to the rhythm of a dance, could this have been danced poetry? Georgiades does not answer this question ...

The claim that I do not quote the passage hints at subterfuge. Perhaps for my reader this will be the last straw, and she will reject and excoriate the attempt to pass off technically inept, self-serving misrepresentation and sophistry, as a review of honest work. Please read my book on its merits. Of course I quote the passage, and in point of fact, I quote it in full. Could the *epos* have been danced poetry? Does Georgiades not answer the question? Let the reader decide. Here is the rest of the paragraph (see my p. 173; bolding added):

‘This rhythm comes to us today as the rhythm of the round dance, designated as such by the ancients and used for the performance of the Homeric epos. It is indeed possible that the original manner of performance was conceived in relation to the dance, and that it persisted as a popular practice alongside merely recited performances adopted in later centuries. (A remnant of this would be the modern Greek *syrtós kalamatianós*.) Thus we can imagine the dancing of the Phaeaces to the chant of Demodokos in the *Odyssey*, 8th song, in epic hexameter. It would mean that it was possible to sing the epos and at the same time to dance it. At all events, even if the Homeric epos itself was no longer danced, it is clear from what has been discussed that its rhythm, and therewith its connection with the human being in his entirety, originated from the round dance. **The one direction of development, the intellectualization of a corporeally bound rhythm into a purely musical or merely recited rhythm of presentation, is a plausible process which is confirmed historically, whereas the reverse procedure is scarcely conceivable.** Thus we find in the very beginning of Greek ‘poetry’—that is, *musike*—in the time of Homer, and quite possibly even earlier, the impressive unity of music, verse and dance.’

What is at least conceivable is that a number of conventional formulas may have developed and circulated among bards who attempted to sing along to a dactylic round. (The six-foot grouping itself may have been an imposition of the accompaniment upon an eternally circling dance movement.) These formulas may have become ensconced in the vocabulary of hexameter story-tellers, who came to compose and perform neither

as, nor with, accompaniment. But Georgiades points (perhaps unintendedly) to the reason why those who believe, inversely and perversely, that verbal formulas generated the hexameter itself, can *never* accept that Homeric poetry was connected to dance, or even that depictions of danced performance of bardic poetry within Homer are depictions of the hexameter. I have put Georgiades' exposition of the fallacy that has recently prevailed in Homeric scholarship, in boldface. The necessity of such denial should be weighed when considering the merits of Mahoney's review.

One of the intriguing results of a comparison between the Homeric hexameter and the modern *syrtós* is an account of the bucolic diaeresis. Mahoney makes no mention of this account, although I announce its importance in the Introduction (pp.15-16) and at several further opportunities. Unlike the caesura—midline breaks are typical—the diaeresis is a typological mystery. The coincidence of a new word and the dactylic thesis is the cue for the start of a line. Why should an inceptive cue be deployed near line end? The *syrtós* resumes its rightward motion, after a leftward retrogression, at precisely this point. The inceptive gesture in the verbal accompaniment is perfectly in place, if it were meant to accompany this dance. But even without the comparison, the typological mystery of the bucolic diaeresis ought to be a clue that there are extrinsic pressures on the way that language is deployed within this metre. It is another piece of evidence for the extralinguistic origin of the hexameter.

The aesthetics of retrogression that I discuss at length are also ignored by Mahoney. It will not do to reproduce my book here. But there is no level of Homeric art that this concept does not inform, from the line, to ring composition, to the episode, to architectonic narrative. The chart on p. 115 illustrates how the retrogression in the *syrtós* corresponds to the segment of the hexameter between caesura and diaeresis. There may even be room to explore semantic reflections of this movement in the verse (see my discussions of πολύτροπον, and πάλιν, pp. 125-6).² The *Iliad* sings reversals in battle, great and small (see pp. 181-2)—and the *Odyssey* circuitous returns home (see esp. pp. 204-7). 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 Muslim epic.) 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 to account for Homer's 'characteristic inconsistencies and inconcinnities' (David Shive), to apologise divertingly and creatively for a certain primitiveness of style in relation to literary epic

² Πάλιν ('backward') with forms of τρέπω ('turn') is always placed between caesura and diaeresis—in addition, *all* compounds of πάλιν (παλίωξις, παλιμπετές, παλιμπλαγχθέντες, etc.) occur so as to place πάλιν within the retrogression, in both epics. The figures for πάλιν appearing between caesura and diaeresis in the *Iliad* are 29/39 (74.34%); in the *Odyssey*, 17/18 (94.44%). A correspondent (Oliver Thomas, Oxford University, p. c. 2008) points out, however, that according to the tabulations of Stefan Hagel (following E. G. O'Neill), *all* words of this phonological shape appear disproportionately between caesura and diaeresis. Before one considers a 'semantic appropriateness' for the placement of πάλιν in certain portions of the hexameter, one evidently needs to account for this strange and more general fact—without resorting to 'colometric tradition', that bottomless pit for things explained away without proof or reason. (Given the possibility of metrical lengthening, words of the shape of πάλιν could be placed virtually anywhere in a hexameter.)

and later forms of narrative. Have we forgotten this? Shall we not drink from the waters of Lethe, and clasp hands—and dance?

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