A Response to Laywine on Hagel, *Ancient Greek Music*★

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I am sad that as intelligent a reviewer as Alison Laywine was so deeply dissatisfied with my book. On the one hand she has convinced me that there are issues which deserve her harsh criticism, even if part of her disappointment (I hope) might perhaps be ascribed to the fact that the title misled the renowned Kantian scholar to expect an introduction to the study of ancient music, which she apparently came to embrace only recently. On the other hand, I find my argument so severely misrepresented both overall and sometimes in detail that I decided to write this response, in the hope that in this way the readers of *Aestimatio* will know better what to expect to find in the book.

To begin, Laywine could not locate 'a single, self-contained, coherent statement of [my] overall motivations' [168] and therefore embarks upon construing one on the basis of my doctoral thesis, published a decade earlier. Turning the volume around, she might instead have found the statement, 'This book endeavors to pinpoint the relations between musical, and especially instrumental, practice and the evolving conceptions of pitch systems.' This, I think, covers it nicely. I will give one example of the internal cohesion of the argument which Laywine is missing. Towards the end of my book, the necessary ingredients are collected (I trust) for tuning a lyre to the same pitch (within a semitone) and the same intervals (within a small fraction of a tone) as ancient lyres had been in the second century, in order to accompany a tune that had actually been composed for such a lyre. To accomplish this we need to understand the pitch range of the cithara [ch. 2] and how the tunings reported by Ptolemy relate to the keys of the notation [ch. 4] in comparison with other treatises [ch. 3], which in turn presumes a diachronic

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understanding of the evolution of this notation [ch. 1]. On the other hand, it is necessary to understand how the tunings support the specific 'modes' used at the same time [ch. 6], and it is a useful corroboration to see that the reported theatre resonators were in best accordance with music of just this type [ch. 7]. Finally, it is necessary to single out musical documents that were citharoedic [ch. 8] as opposed to *aulos*-accompanied pieces [ch. 9].

None of that was part of my thesis, which focused instead on modulation in making a case *inter alia* for complex, rapid, and remote modulation in the 'Delphic Paean' by Athenaeus. Inevitably, as my book talks about pitch-systems and the development of instruments, modulation is also a recurrent topic there. For this reason, Laywine lightly dismisses the requirement of entangling herself within the interwoven strands of the argument, which builds on concurrent evidence from archaeology, the texts, and the scores, sometimes with the help of statistical testing. Instead, she decides that the topic of modulation would provide a chance for upsetting the whole building with a single blow. Consequently, most of her review is dedicated to countering my arguments for remote modulation in the Delphic Paean [Hagel 2000] and the Ashmolean Papyri [Hagel 2010]. Take Hagel's speculative interpretation of these away, she implies, and the whole edifice collapses. This approach, I am afraid, is sadly flawed from the outset. It is true that my argument throughout assumes the widespread practice of modulation—but quite ordinary modulation between neighboring keys, which is well attested both in the texts and the extant scores, and whose existence nobody (including Laywine herself, as it seems for all practical intents) has ever denied. In contrast, what Laywine tries to refute with considerable knowledge and verve is the presence of remote modulation in the two mentioned fragments, modulation between keys that may even be located opposite each other in the circle of fifths. If the Ashmolean fragments are better explained in another way, I am quite happy to renounce whatever wrong I have said in the 13 pages dedicated to them. In the structure of my argument, however, the interpretation of these fragments does not form the basis of far-reaching conclusions. Laywine has mistaken a twig for the root, pruning for chopping down.

Admittedly, if Laywine is right, my book from 2000 is more seriously compromised. I will, therefore, take the liberty to address a couple of her arguments in detail (which is not to imply that I stand by every idea that I expressed back then). First of all, I wonder how Laywine incorporates within her general rejection of remote modulation in antiquity the fact that the treatises explicitly acknowledge it as part of musical practice.[1] If we take the texts at face value, the discussion about whether anything of the kind is going on in the Delphic Paean or the Ashmolean fragments in particular is of little interest for the general question: it was going on elsewhere, then.

Even so, let us review my claims about the second part of the Paean. Laywine first objects on a very general level that any suggestion about what went on 'inside Athenaeus' head' is pure speculation. Certainly so, but then any musical or textual interpretation is speculation—including Laywine's claims about what 'Hagel thinks' [136], 'believes' [127], 'must be thinking' [158] or 'apparently takes' [153] (the last is the most wrong). I fully agree with her that the whole dispute can ultimately be reduced to the 'question whether we have any good reason to describe the melody this way' [145].[2] I also agree that drawing parallels to the 'modern Western' musical tradition may be a useful tool for understanding what is going on. However, I think that Laywine's parallels do not adequately reflect important aspects of what is happening on the surface of the ancient melody and, therefore, ignore crucial points of my argument. My interpretation was based, firstly, on the fact that the 'odd' note (O) is embedded within the *Gebrauchsskala* of its context by providing the 'link note' (B) that ties it to the rest *via* the circle of fifths, though this note does not belong to the melodic repertoire otherwise but

1 E.g., Aristides Quintilianus, *De mus.* 1.11 [Winnington-Ingram 1963, 22.15–16]:

> All sorts of modulations take place between the keys, according to each one of the intervals, both the composite and the incomposite ones...their forms and structural cohesion can be perceived in terms of modulation from a note by a tone or a semitone, and generally by any interval, odd or even, downwards or upwards. They establish common ground between tetrachords: sometimes these differ by a semitone, sometimes by a tone, sometimes by larger intervals....

Modulation 'by a semitone' is equivalent to modulation across almost (if applied to a whole system) or exactly (if applied to tetrachords) half the circle of fifths, the most remote modulations of key possible in both the ancient and the modern Western system.

2 However, Laywine later takes a much more uncompromising stance [149]: even if it could be shown that only a single melodic interpretation is possible, this would still not reveal the composer's intentions because all the facts may be side-effects of an unknown intention. This is, of course, true and I capitulate before such a degree of philosophical rigor. No, we cannot know anything for sure!

appears only once—which I took to be a plausible signal of the composer's (playful) awareness of harmonic relations. Secondly, I have argued that the melodic and rhythmical structure indicates how the notes are arranged within different scale fragments that are well known from theory and the scores alike, and which an ancient listener would have 'recognized' just as we 'recognize' a major chord when its notes are played in succession. In contrast, Laywine relies on a notion of 'coherence' that is tied to individual notes rather than musical context in a way that strikes me as hardly useful. In particular, she believes that since all the other notes are present in the Phrygian scale—unfortunately she forgets the link note B—and since this is the basic key of the section, it would always be 'more natural' to interpret them as '—well, er, uhmm—Phrygian notes' [145] and to consider the 'odd' one out as—well, er, uhmm—odd ('exharmonic' [148]).[3] This is, I think, tantamount to arguing that if a piece is, by and large, in C major but for a while introduces an f♯ instead of f, it would be more natural to perceive the key as still C major rather than as a modulation to G major, because all the other notes are still the same.[4] Perhaps it depends on how long the 'while' must be in order to speak about modulation? At any rate, even the uncontested parts of Athenaeus' piece show, by means of the notation particular to specific keys, how quickly one can switch to another one and back. But that does not seem to be the issue anyway: Laywine would apparently agree about neighboring keys such as C and G. The preceding example is merely to show that her

[3] Laywine tries to conceal the desperation behind the designation as 'exharmonic' by ascribing it the potential function of providing a semitone stop below a structurally important 'fixed' note, M [148]. This idea hardly stands exposure to the facts of the melody, where M frequently leads over to an emphasized O (emphasized by length and/or repetition), while the opposite is never the case. M, therefore, does not gain prominence from the presence of O. On the contrary, it lends prominence to it, just as is demanded within the framework of my interpretation of the notes in question as a couple of πυκνά a semitone apart where O is the 'fixed' note of the lower one. Where the higher πυκνόν emerges, its lowest note M is given weight in a similar way by reaching it from the note above.

[4] Rephrasing Laywine [147f]:

> Too much is the same for it to be likely that g, a note that has by now so solidly established itself in our musical insight as being the dominant of C major, could be understood as the tonic of G major even be it in the company of f♯, and even if f♯ appear to be the leading note to a G major chord: g–b–d.

general argument, if applied generally, seems to entail absurd conclusions. Therefore, everything reduces to the question of melodic usage.

Here then is my own ‘modern’ parallel to what Athenaeus achieves (though drawing parallels is ultimately hampered by the fact that in the ancient chromatic genus notes may become harmonically ambiguous more easily than in our ubiquitous diatonic). Suppose the following:

> A piece’s first movement starts off in C major with a brief introduction of an f♯ towards its end. The second movement uses f♯ on a regular basis (for instance in the context of g–b–d), while f is also present from time to time. Suddenly, a single odd g♯ appears. A bit later, there is another g♯ but one preceded by a c♯—not really that odd, after all. However, c♯ never turns up again. Instead, g♯ becomes really prominent, especially in the sequence e–g♯–b (upwards and downwards), which alternates with f–a–c (also upwards and downwards). No fewer than nine of such ‘triads’ are found in close succession…

Looking at such a score, I would yield to speculation and say that the composer intended to switch/modulate between two major chords a semitone apart, which theory might term ‘E major’ and ‘F major’.[5] Following Laywine’s argument, I suppose that she would prefer to label the whole second movement as G major with modulation to D major and an exharmonic g♯. I leave it to the reader to decide on the basis of the Paean’s melody which kind of description appears more plausible.

But again, all this is peripheral for the project of the book under review, concerning which I have concluded my general plea above. All I have to add are a few details in which I find that either the evidence or my arguments

[5] Laywine repeatedly implies that I have claimed ‘that we are really in Hyperiastian’ [144] or even that somebody might have ‘heard a modulation from Iastian to Hyperiastian’. This seems to be a misunderstanding. Actually, where I have used the name ‘Hyperiastian’, it is always enclosed in quotation marks, and ‘Iastian’ I used only as a means of clarifying the structure. Moreover, I have pointed out that in Aristoxenian terminology, which may have been more relevant at the time of the composition, the posited remote modulation would take place between ‘high Mixolydian’ and ‘low Mixolydian’, a terminology that ‘may’ have played a role [Hagel 2000, 73]. The intention of modulation by a semitone, however, is independent from the question whether the composer would have had a name for the keys. I only argue for the former.

are misrepresented. First of all, I am not aware of ever having assumed or argued that equal temperament played a role in ancient music-making [e.g., 127n2, 144, 153]. Above all, no actual scale or set of scales in a particular performance needed to be equally tempered: a 'Pythagorean' tuning, for instance, would satisfy all demands. However, Aristoxenus effectively maintained that the octave consists of 12 equal semitones, as was required to set up a full coherent system of modulating scales in theory; ancient notation basically reflects the assumption of a closed circle of fifths.

Related to this issue is Laywine's concern that some of my arguments 'would lead us to expect that the tonic chromatic would at least find special favor with Aristoxenus', while 'the surviving theoretical treatises do not seem to privilege the tonic chromatic' [165] over the alternatives of 'different shades of the chromatic, the enharmonic and its different shades, as well as the diatonic and its variants'. This appears to involve a twofold error. Firstly, my arguments by no means require the prominence of chromatic over diatonic or even enharmonic; rather, they entail the prominence, among diatonics, of a diatonic with semitones and tones (instead of three-quartertone intervals, and so on), and the prominence, among enharmonics, of a quartertone enharmonic. All this is warranted by the sources. Secondly, the notion that theorists do not favor the tonic chromatic over other shades of chromatic is plainly wrong. Among the Aristoxenian sources, some quote the tonic chromatic exclusively and all others, including Aristoxenus himself, treat it as the *typical* variant. Similarly, practically all the non-Aristoxenians who describe intervals in terms of ratios chose the numbers in a such way that they can only reflect the 'tonic chromatic'—from Archytas on, who derives the 'chromatic note' by means of a whole tone, through Eratosthenes and Didymus and the whole 'Timaeus' tradition up until Roman times. Ptolemy is the only one who also provides for a 'tense' version (which he gives as the citharoedic standard in the higher range; in the lower range, however, his χρωματική is still an exact whole tone above the bottom note).

Concerning my treatment of the ancient scores, Laywine [154n17] implies that my sole motive for assuming that the bulk of the notes in Pöhlmann and West 2001, no. 5 is restricted to a fourth is the fact that they are so in no. 6. In fact, no less than 69 out of the 71 preserved notes of no. 5 fall within the fourth in question. To most people, a percentage of 97% might warrant my designation as 'the major part'. Laywine also seems to insinuate that the seemingly crowded notes within the fourth in no. 6 might belong to different pieces after all. A glance at the fragments shows that this cannot be the case: ΥΠΜΛ are ubiquitously coupled with either Τ or Ν, and the latter two also appear in close context.

Against the 'rush to judgment' by Pöhlmann and West, Laywine defends an 'Arabic' interpretation of the Ashmolean scales by observing that her intervals are 'vastly easier to sing than the weird and horrible seventh diminished by an enharmonic δίεϲιϲ in what survives of the Orestes fragment' [160]. Actually, the quoted interval is not part of the melodic line at all but occurs between vocal notes and what has been taken as instrumental notes of disputed purpose (perhaps only to give the accompanist an idea of the intended 'harmonization'?). The 'horrible' intervals, therefore, likely do not indicate successiveness at all. In any case, there is hardly a question of them having been sung. Laywine subsequently proposes understanding the modulation not as one of key but of genus, finding 'nothing in Hagel's analysis that would exclude the possibility'. No wonder, since I have also suggested that, e.g., on page 267 (compare the synopsis on page 271).

Unfortunately, Aristides Quintilianus' 'Wing Diagram', which Laywine cites as a source for ancient notation [135n5], is not preserved in the manuscripts; perhaps she relies on the reconstruction in Barker 1989 [428f]?

More problematic is Laywine's remark about 'the lyre and the cithara' as providing the context for the theory of interval-ratios: 'no great surprise here because string-lengths can be readily compared in terms of musical ratios' [166]. Actually, difference in string-length plays no role on Greek or Roman lyres; and even if it did, it would not warrant any sort of comparison.[6] But Laywine plays the *ʿoud*, which may well explain why she is much more inclined to considering microtonal variants within a single performance than I am: such a practice is intimately connected with instruments with a (fretless) fingerboard and the musical cultures where these play an important role. A lyre, however, has no fingerboard and comparatively few strings; therefore, its notes are too precious a resource to waste it on mere microtonal

6 This is because comparison of length between strings of different length presupposes that similar portions of both sound the same pitch. This can be ensured on an experimental instrument (in ways outlined by Ptolemy) but not on any ancient lyre that we know of.

variation (which however played a role in differentiating the individual tunings). Admittedly, the case might be different for an expertly played *aulos*; however, I still cannot see why Aristoxenian theory would not have incorporated a 'modulation according to shade' alongside the other four types of melodic modulation if it was common in practice.[7]

Finally, it is of course mainly the fault of my user-unfriendly presentation if Laywine sometimes misses the essential connections of the argument.[8] As an example, she complains that the solution of the fundamental riddle expounded and allegedly solved in my chapter 1 'did nothing to advance later discussion in the book' [168]. Actually, it forms the basis for relating Ptolemy's work to the rest of ancient music. Has Laywine missed the point that Dorian eventually turns out to be, in some sense, Lydian (and not Hypolydian as she only quotes), and that this is essential for figuring out why a tuning that Ptolemy describes as instantiating the Dorian key would be called λύδια? Or has she failed to realize that the relation between Ptolemy's system of keys and the keys of notation had not been figured out before? At least, her review never mentions this topic, which I would have considered one of the book's major achievements. But since she never expresses doubts about this point either, I may perhaps console myself with the warming thought

7 Here we cannot really 'conjecture "til the cows come home" [157]. If ancient writers present a list of possible melodic modulations, evidently implying that it is exhaustive, this leaves little room for speculation that another one was 'discussed in treatises or parts of treatises that have been lost'. Nor is it really an option, at least not without specifying a possible motive, to have Aristoxenus exclude from his theory a kind of modulation which was part of late classical music, which was reflected in notation, and which could be described within his framework straightforwardly («πέμπτη δὲ κατὰ χρόαν ὅταν μενόντων τῶν δυνάμεων καὶ τοῦ γένους κινῆται τὰ διαστήματα»). I appreciate Laywine's caution concerning an argument from silence; but sometimes general scepticism may be dissipated by greater familiarity with the evidence.

8 Not always is Laywine herself a model of helpfulness. When she informs us that my portrayal of the presence of lots of notes within a narrow range (such as six of them within a fourth) as a sign of sophistication is 'simply false' [155], simpler minds like mine may crave an explanation or an example of non-sophisticated music with comparable characteristics. Perhaps, though, we have different ideas of 'sophistication', which I do not necessarily consider as laudatory and would probably not apply to the great melodies of three notes that she cites, even though I would almost certainly agree that they are great. My fault then, as the non-native speaker.

that, at the end of the day, even a harsh critic accepted crucial points of my argument, even if I cannot be sure whether she was aware of the fact.

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