
Ancient Greek Music: A Technical History by Stefan Hagel

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Ancient Greek Music is an ambitious new book by Stefan Hagel. Its title happens to coincide with that of an ambitious book published by M. L. West with Clarendon Press in 1992. But the two books differ considerably in perspective. West set out to study ‘the various elements that go to make up ancient Greek music as a performing art, as an object of theoretical inquiry, and as a cultural phenomenon’ [1992, 327]. Hagel’s book is more narrow in scope. This is perhaps signaled by its subtitle, ‘A New Technical History’. The history that Hagel offers is not only technical in nature, it is also a history of technical problems and innovations in the actual music making of ancient Greece—at least to the extent that such music making is known to us. There seems to be no one technical issue that gives focus to the book, which ambles and rambles in a not especially direct or obviously coherent way. But there is one such issue that gives the book its point of departure: it is the one indicated by the Greek word «μεταβολή» and its affiliated forms.

In the most general sense, «μεταβολή» just means change, as when we speak of a change of fortune (τῆς τυχῆς) or changes of constitution (τῶν πολιτειῶν). Music can undergo change in many different respects but the change relevant here occurs in melody. We find this association of ‘change’ and ‘melody’ in book 2 of Aristoxenus’ *Elements of Harmonics*:

Ἐπει δὲ τῶν μελωδομένων ἐστὶ τὰ μὲν ἀπλᾶ τὰ δὲ μετάβολα, περὶ μεταβολῆς ἂν εἶη λεκτέον, πρῶτον μὲν αὐτο τί ποτ’ ἐστὶν ἢ μεταβολή καὶ πῶς γιγνόμενον—λέγω δ’ οἷον πάθους τίνος συμβαίνοντος ἐν τῇ τῆς μελωδίας τάξει—ἔπειτα πόσαι εἰσὶν αἱ πᾶσαι μεταβολαὶ καὶ κατὰ πόσα διαστήματα. [Macran 1902, 38.3–8]

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In English, this passage might be rendered as follows:

Since some melodies are simple and others changing, it is needful to speak of change, i.e., to say first of all what change is and how it arises—I mean when a certain effect is brought about in the order of the melody—and then how many of these changes there are and at how many intervals.

Aristoxenus is speaking at such a high level of generality that the passage as such tells us only what any untutored listener knows already, namely, that melodies can change. Again we ask: ‘In what respect? Is a change in dynamics or volume relevant?’ If we consult the introduction to the *Harmonics* of Cleonides, a follower of Aristoxenus, we learn that a melody can undergo four kinds of change: that of genus (*κατὰ γένος*), ‘system’ (*κατὰ σύστημα*), *τόνος* (*κατὰ τόνον*), and musical composition (*κατὰ μελοποιΐαν*) [von Jan 1895, 13.20.1–2]. Change of *τόνος* is what Hagel takes to be relevant for his purposes (and what he ultimately takes to have been at issue for Aristoxenus as well).¹ But then the question is what is meant by ‘change of *τόνος*’, or, more generally, what is meant by *τόνος* as such?

If we consult Cleonides again, we learn that the word «*τόνος*» can be used in four ways: either to mean a note (*φθόγγος*), or an interval (*διάστημα*, i.e., presumably the interval of a whole tone as the difference between the fifth and the fourth) or the ‘range of a voice’ (*τόπος φωνῆς*) or pitch (*τάσις*) [von Jan 1895, 12.19.6–8]. The most obscure gloss, from our point of view, is the third. ‘Range of voice’ could be taken to mean register, as when we say that a little boy’s vocal register is significantly higher than that of an adult man. But Cleonides cannot have anything quite so simple in mind because we could get away, for most purposes, with a crude distinction of four vocal registers: the very high, the very low—at the extremes of human singing—and then the not so high and the not so low, somewhere in between. Following Aristoxenus, however, Cleonides distinguishes 13 ‘vocal ranges’ and he mentions them by name, starting with Dorian, Phrygian, Lydian, and so on. If it were just a matter of register, one would wonder why so many fine discriminations were necessary: they are indeed fine, since the 13 ‘ranges’ are successively some kind of semitone apart from one another. But once the discriminations are

¹ Unfortunately Aristoxenus’ treatment is lost. This is why we depend on Cleonides.

made, context indicates that melodies can move from one of these ‘ranges’ or *τόνοι* to another. Such movement will count as the kind of *μεταβολή* of special interest to Hagel. To signal its significance and peculiarity to melody, it is more usual to translate it into English not as ‘change’ but as ‘modulation’. (Needless to say, ‘modulation’ would be an apt translation of «*μεταβολή*» in the other three senses, as well.)

If we try to get closer still to what modulation involves, we will be struck by the fact that, though the discriminations between *τόνοι* are fine and numerous, they are not indeterminate in number. It might be thought that the simplest melody, at least for the sake of argument, consists of two notes of different pitch. Since pitch is a quality of sound, whether musical or not, that is registered by the ear along a continuum of higher and lower with no apparent gaps, nothing prevents us from continuously modulating a simple two-note melody, i.e., such that the melody, *as a whole*, passes from a lowest given pitch to a highest without skipping any pitch along the way. As a matter of fact, this is what happens as a result of the so-called Doppler effect. Emergency vehicles in Germany today—whether ambulances or police cars on the chase—have sirens that repeatedly emit two notes exactly a fourth apart. If you have the misfortune of living on a busy street like Friedrichstraße in Berlin, you will hear such sirens racing towards and away from your domicile all day long. As siren and vehicle approach you, the two-note pattern will not only get louder, it will also rise in pitch—by about a semitone; as they get further away from you, the two-note pattern gets softer and lowers in pitch—again, by about a semitone. But though this semitone sets a limit on how high and low the two-note pattern can rise and fall, the shift in pitch within that semitone is continuous in both directions: it never appears to your ear that the two-note pattern misses a possible pitch along the way. If, then, the Doppler effect is a species of modulation, one might say that it produces ‘fine’ discriminations but that they will be indeterminate in number. By contrast, the fine discriminations associated with ‘modulation’, as presented by Cleonides, are exactly 13. That number is significant—all the more so, if we take the liberty of rounding it down to 12. Let me explain.

Given that we are accustomed to thinking of the octave as if it were a unit of length divisible into 12 equal lengths a semitone apart, it is natural to suppose that, for Cleonides, modulation involves taking

a melody through each of the 12 semitones from one end of an octave to the other: or at least that it potentially could do so, if we really wanted our melody to make a stop at all 12 stations.² But having gone that far with the thought, we might just as well take Cleonides to be speaking of the circle of fifths. Dialing through the circle of fifths is equivalent to starting from a given note, rising an equal-tempered fifth and descending an equal-tempered fourth—a total of six times. If we keep our risings and settings within the compass of an octave, we will pass from one end of the octave to the other, making a stop at each of the 12 semitones in between. But if we now think of each of the stops we make, in precisely the order in which we make them, as a ‘key’ in our sense of the word, and if we think of our point of departure as the ‘natural’ key, then, with each subsequent stop we make, we will reach a key with one more accidental: from the natural key, we will move to a key with one sharp, then two sharps, then three, then four. If we continue in this way, we will pass through keys that are ‘enharmonic’ in our sense, i.e., the ones that we can think of indifferently as either having numerous sharps or numerous flats: five sharps-seven flats, then six sharps-six flats, then seven sharps-five flats. If we continue further still, we will reach keys with fewer and fewer flats, starting with four and moving successively to one, until we finally reach our point of departure, the natural key [see [Figure 1](#)]. The implication of invoking the circle of fifths is that, when Cleonides construes *τόνος* as ‘range of a voice’, he can be understood to speak of keys; and, when he speaks of ‘modulation of *τόνος*’ in this sense, he can be understood to mean change of key—as we do. At least, this is how Hagel understands Cleonides. But, what is more important, he takes the musical practice that Cleonides is responding to as having exploited modulation in just this sense [2000, 33–38].

² For the purposes of the exposition that follows, I am assuming equal temperament. Hagel himself believes that equal temperament was at the basis of modulation in ancient Greek music. At least, he takes Aristoxenus to have been operating with equal temperament, and he takes Aristoxenus to have been responding to the practice of modulation in ancient Greek music. See, for example, his earlier book *Modulation in altgriechischer Musik. Antike Melodien im Licht antiker Musiktheorie* [2000, 18–20]. I myself think that the question of equal temperament remains open.

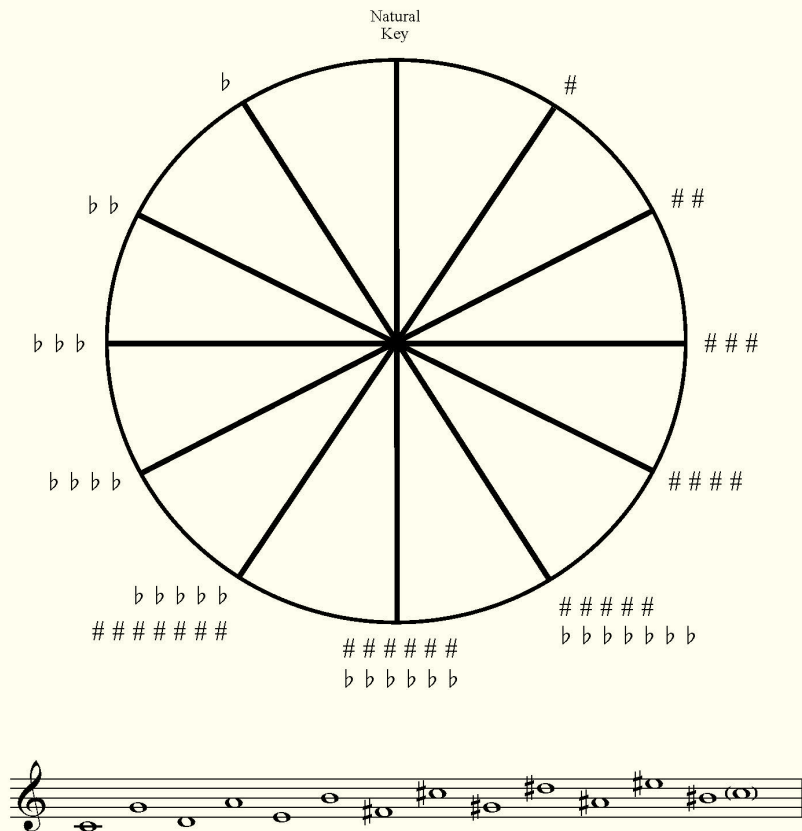


Figure 1 The circle of fifths

Having said all that, I should add that the modulation of a whole melody from one key to another is seldom of musical interest. Imagine a singer dialing ‘Frère Jacques’ through the circle of fifths. Since only the key is changed, the melody itself remains internally the same. From the listener’s point of view, the singer might just as well pick one key and sing the tune in it. This very natural thought suggests that modulation of key is of interest chiefly *within* a tune. As listeners, in other words, we will be more interested to hear a tune that starts in one key and switches midway to another. Imagine now that our singer sings a melody that starts in a natural key like our C-major. Imagine that the singer starts on the note that we take to be the tonic of the C-major scale, namely, C, ascends stepwise to the dominant

G but then modulates at G to the neighboring key of G-major by introducing the sharp that is characteristic of that key. She might do this, for example, by passing from G to F \sharp , only to return to G and rise to A. Perhaps she then descends stepwise from A back to C, but avoids F \sharp in favour of F \natural . If she does that, she will have modulated back to C-major. But though the modulation to G-major will have been short lived, it will have been enough to signal a break from the prevailing C-major environment and thereby introduce a little bit of variety. The distinctive mark of this escape will be that we can aggregate the relevant pitches so as to get three neighboring semitones where we would normally get only one. Had the singer remained in C-major, we would have got a semitone only between E and F. But having modulated from the one key to the other, we get semitones between E and F, then between F and F \sharp and also between F \sharp and G—though obviously not in that order, as I am imaging the tune. For Hagel, modulation of key in ancient Greek music was a phenomenon internal to melody, in the way I just characterized it. (He finds evidence for it in the surviving scraps of annotated music by looking for otherwise unexpected, implied sequences of successive semitones—like the relatively short, implied semitone train between E and G that I just found in the melody I imagined above.)

At this point, any reader interested in the subject and even minimally tutored in what, for lack of anything much better, I will call ‘music of the Western World since the age of all those guys whose tunes we had to learn for childhood music lessons’, will wonder what any of this could possibly mean. Does it mean that the Greeks had major and minor keys, as we do? Did they like to modulate from a given major key, say, to its related minor, or from a given minor key to its related major, as we do? Did their keys, whether major or minor, have degrees, as ours do? If so, did the first degree of their keys serve as the tonic and the fifth degree as the dominant such that if you played a tune in a certain key for a while and then ultimately moved from the dominant to the tonic, your listeners (if any) might well have the sense that you had come to the end (at least for now)? Did ancient Greek musicians employ openings and cadences that exploited the structure of their keys? The answer to these very reasonable questions remains (so far as I can tell): ‘Who knows?’

What the evidence allows us to say is this. The theoretical writings that survive testify to the recognition in ancient Greek music of an ideal scale spanning an octave but indeterminate in pitch, and built up out of two tetrachords spanning a fourth and separated by a disjunctive tone.³ The tetrachords of the ideal scale could be internally arranged in three different ways. From lowest to highest pitch, you could have intervals of quarter-tone, quarter-tone, ditone: this would give you ‘enharmonic’ tetrachords, in the Greek sense of the word. You would get ‘chromatic’ tetrachords, if the sequence of your intervals were semitone, semitone, tone-and-a-half. You would get diatonic tetrachords from semitone, tone, tone. The fact that there were three ‘genera’ of the tetrachord went hand in hand with the fact that the two notes bounding a given tetrachord were always fixed in pitch (whatever the pitch register of the scale as a whole), whereas the two inner notes could, in principle, vary in pitch, depending on the genus. The lower of the two moveable notes could vary by no more than a quarter-tone, whereas the higher could vary by a whole tone. The notes of the ideal scale all had names. The only one I will trouble my readers with is that of the lowest note of the disjunctive tone. It was called ‘mesē’ (‘the one in the middle’).

The scale as a whole was called the ‘Greater Perfect System’ [see [Figure 2](#)]. There was a second ideal scale called the ‘Lesser Perfect System’ [see [Figure 2](#)]. It was distinguished by the absence of the disjunctive tone. Hence, it spanned an octave less than a tone. The higher fixed note of the lower tetrachord was identical to the lower fixed note of the higher one. This note too was called ‘mesē’. But to distinguish it from the ‘mēsē diezeugmenōn’ of the Greater Perfect System, it was referred to as ‘mesē synnēmenōn’—the ‘one in the middle of the conjunctive notes’ as opposed to the ‘one in the middle of the disjunctive notes’. The interest of these two ideal scales is

³ The ideal system that I have in mind here and later in this review is the so-called Greater Perfect System. But the Greater Perfect System had more than two tetrachords, as did the other ideal system which will also come into play in what follows. I should like to clarify that whenever I speak of the Greater Perfect System, I really mean the central octave of that ideal system; and that when I speak of the Lesser Perfect System, I really mean the center of that ideal system, i.e., the two central conjunct tetrachords that are missing the disjunctive tone characteristic of the central octave of the Greater Perfect System.

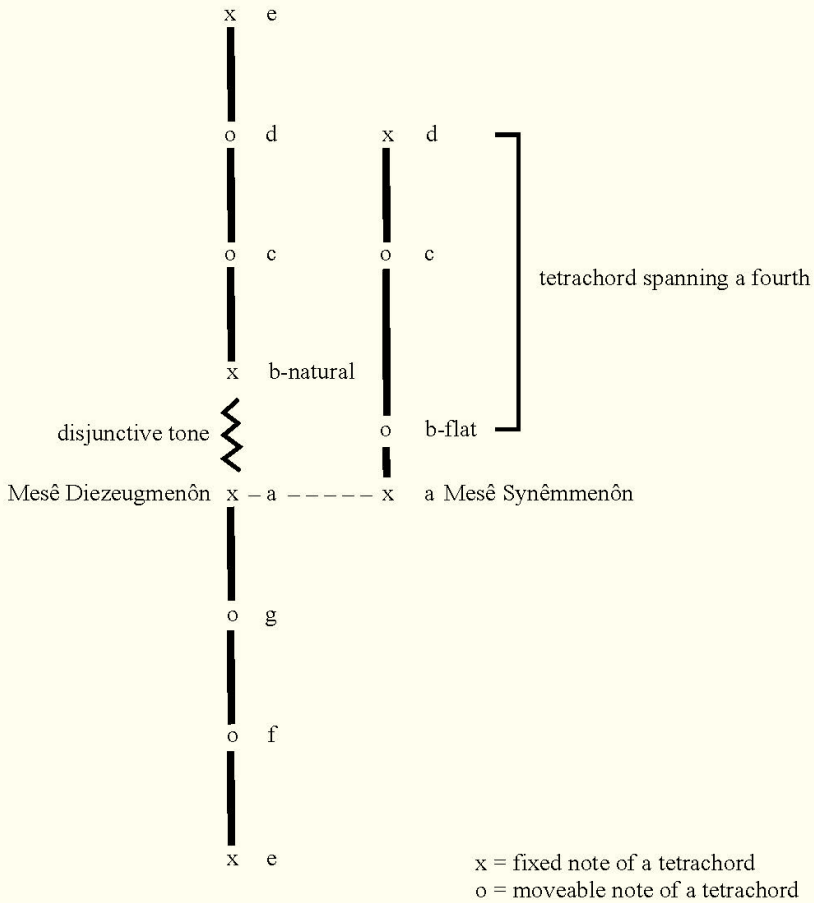


Figure 2. The Greater and Lesser Perfect Systems (given in the diatonic genus of the tetrachord)

that we can think of them as sharing a lower tetrachord and as thus forming a single path in the upward direction, until we get to *mesē*, at which point there is a fork in the road: the melody can either travel into the upper disjunct tetrachord of the Greater Perfect System or into the upper conjunct tetrachord of the Lesser Perfect System [see [Figure 2](#)]. For that matter, it can travel up the one, retrace its steps and turn at *mesē* in order to travel up the other as well. Such a turn may be regarded as a ‘modulation of key’ in our sense of the phrase.

This can be seen if, say, we take the whole forked path, stipulate a genus for all three tetrachords—preferably the diatonic for convenience—and assign modern note names to the common lower tetrachord and the disjunct upper tetrachord of the Greater Perfect System in a ‘natural’ key. That will give us, in order of lowest to highest: e, f, g, a (*mesē diezeugmenōn*), b, c, d, e’. But in order to notate the conjunct upper tetrachord of the Lower Perfect System, we will need one accidental, namely bb: e, f, g, a (*mesē synnēmenōn*), bb, c, d [see [Figure 2](#)]. This will be like modulating to a key with one flat from a natural key. If a singer or some other musician were to perform the modulating turn on the forked path, analogous to the modulation from C-major to G-major in the little tune that I imagined earlier, there would be, in the neighborhood of a, that is, of the *mesē diezeugmenōn* and *mesē synnēmmenōn*, two extra semitones that would not have come into play had the melody remained either in the Greater Perfect System or in the Lesser Perfect System: a-bb, bb-b \sharp and b-c. Had the melody remained in the GPS (as I will refer to it henceforth), there would have been only the semitone between b and c. Had it remained in the LPS, there would have been only the one between a and bb.

Here is an important implication of all this. Suppose that, after traveling upwards into the GPS, the melody explores the upper conjunct tetrachord of the LPS. If it advances to the upper fixed note of this tetrachord, a fourth above *mesē synnēmenōn*, it might naturally come to treat the disjunctive tone of the GPS as having now been shifted to this higher perch [see [Figure 3](#)]. In that case, we may regard the note *mesē diezeugmenōn* (MD) as having been raised a fourth. For convenience, let us refer to it as MD*. This is indeed equivalent to a 30° turn of the circle of fifths in the flatward direction and, hence, to a modulation to a key of one flat from the natural key [see [Figure 1](#)]. This is all to the good because it confirms the idea we have been exploring, namely, that the relation between the GPS and the LPS is such as to provide opportunities for ‘modulation of key’ in something like our sense of the word. But having gone this far with the idea, there is no reason not to push it as far as we can. For let us now regard the note a whole tone below MD* as the higher fixed note of the upper conjunct tetrachord of the LPS in a second appearance. The effect of this move will be to give us a *mesē synnēmenōn* a whole tone below its first sounding. For convenience, we may refer to it as

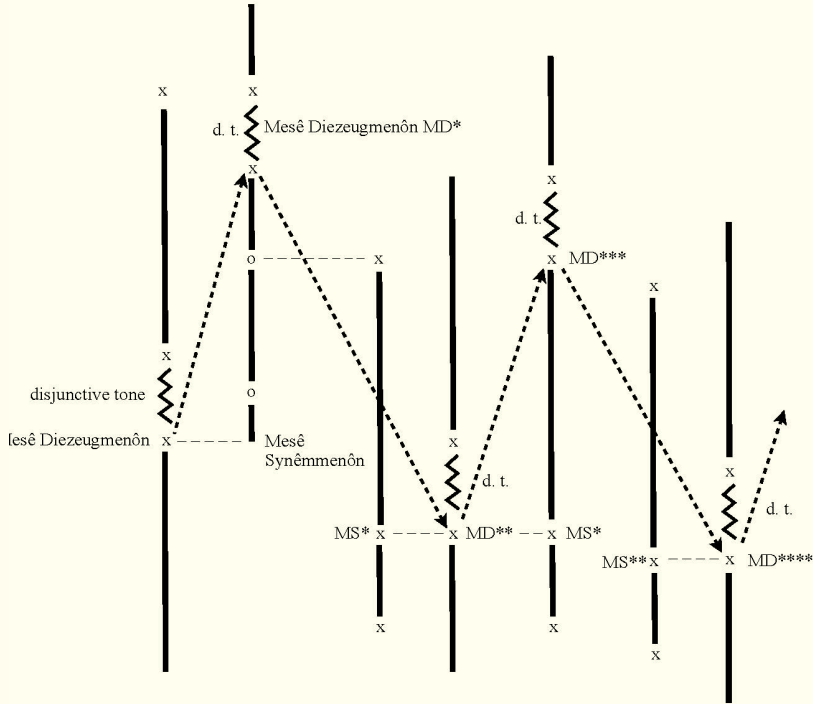


Figure 3. A sample modulation. Follow *mesê diezeugmenôn* (up a fourth, down a fifth, and so on) in the flat direction

MS*. If we now choose to think of this note instead as figuring in the GPS as MD**, we will have lowered MD* a perfect fifth—another 30° in the circle of fifths in the flatward direction, so that we now find ourselves in a key notated not with one flat, but two. We can keep shifting MD up a fourth and down a fifth. But each shift will bring us another 30° in the circle, i.e., another flat away from our point of departure in the natural key. We could conceivably shift MD up and down six times. If we did so, then, provided we took the octave to be divided into 12 equal semitones, we will have closed the circle of fifths.⁴

⁴ Again, let it be noted that all of this is presupposing equal temperament for the sake of exposition alone.

By spelling out, as I have, the implications of the fork in the path at the intersection between the GPS and the LPS, we get no closer to an answer to the reasonable questions about modulation in ancient Greek music that will come even to the mind of those minimally tutored in ‘music of the Western World since etc.’. Sketching out the forked path and where it might lead to tells us absolutely nothing about how ancient Greek melodies might actually have moved along that path. All it tells us is that, in principle, we could assign a melody that travels far and wide a changing key signature with sometimes more or sometimes fewer accidentals. But knowing the number of sharps or flats in a key signature does not, all by itself, indicate anything about mode (major, minor, or what-have-you), much less what the musical conventions might be for establishing in the listener’s ear melodic presence in a given key or departure for some other key. Still, even this much is interesting because it seems to support the hunch that ancient Greek music might be thought—perhaps even by ancient Greek theorists themselves—to ‘change key’ in a way that we would now recognize as dialing through the circle of fifths.

‘Might be thought’ is one thing, though; ‘really was so thought by the players of the game’ is another. Hagel tried to secure the second modality against doubt by looking for evidence of changing keys in the surviving musical fragments as they had been collected and presented by E. Pöhlmann in *Denkmäler altgriechischer Musik* in 1970. That project formed the basis of [Hagel 2000](#), a stimulating and imaginative book. Its results are the point of departure for the book under review. For that reason, I cannot avoid discussing it in some detail.

The program of [Hagel 2000](#) is worthwhile. Its hope is to learn *something* from the surviving musical fragments about how real melodies behaved, to see whether they behaved as the surviving theoretical treatments say they should and whether the theoretical treatments are illuminated by them in turn. (But our hope is that the shuttling from melody to theory and back again will not be such as to spin us in a vicious circle). The program faces the very serious obstacle posed by the paucity and fragmentary nature of materials that span almost 1000 years (fifth century BC to the fourth century AD). Still, something is better than nothing at all. For the purposes of [Hagel 2000](#), the surviving scraps are enough to warrant a spirited stab at

the question whether we can find evidence of deliberate modulation in them. The answer to the question is supposed to be ‘Yes’.

The most interesting fragment for Hagel’s purposes is Athenaeus’ ‘Delphic Paean’ (128/7 BC). It gives us lengthy stretches of continuous melody, mostly—to relatively—free of lacunae until the end where it peters off into oblivion. Like all other surviving scraps of music, it follows the system of notation peculiar to ancient Greek music, which is quite different from ours. One way in which that system differs from ours is that it assigns a different set of signs for each *τόνος* or key (as we will suppose). Keys may share signs but no two keys have precisely the same set of signs.⁵ From the notation of Athenaeus’ ‘Delphic Paean’, we can tell that the first eight lines of the piece are in the *τόνος* or key called Phrygian. We can think of the melody as based on a scale with two notional tetrachords separated by the disjunctive tone, the lower note of which is Phrygian *mesē diezeugmenōn*. We cannot determine the genus of the lower tetrachord, because it is conspicuously defective. It is missing its upper moveable note—the note that normally disambiguates genus, since it occupies a different place in each of the three genera. From the listener’s point of view, the lower moveable note could all by itself equally well figure in any one of them, either as the first moveable note in either the chromatic or the diatonic, or as the second moveable note in the enharmonic. The upper tetrachord is notated unequivocally as diatonic. It has

⁵ I must beg forbearance of the reader. I can find no more perspicuous way to discuss the melodies of interest to Hagel—and others—than by reference to the ancient Greek system of musical notation. That will seem alien to anybody at home in modern note names and staff notation. But part of the problem is precisely how these melodies should be transcribed into our notation. So I will provide a diagram of the relevant keys, in their ancient notation, and their relationships to one another; and I will provide modern note names but only after I have provisionally (i.e., for the sake of argument) accepted certain assumptions. This means that I will provide modern note names only in my discussion of Athenaeus’ ‘Delphic Paean’. In the case of the Ashmolean fragments which I discuss later, where the note signs will, by then, be familiar to the reader, and where I will argue everything is up for grabs, I will not provide modern note names. One important source for the system of ancient Greek musical notation is the tables of Alypius, which can be found in [von Jan 1895](#). I will frequently refer to these tables as the ‘Alypian tables’. Our other source is Aristides Quintilianus, *De musica*, 1.11—more specifically, the so-called Wing Diagram in that chapter.

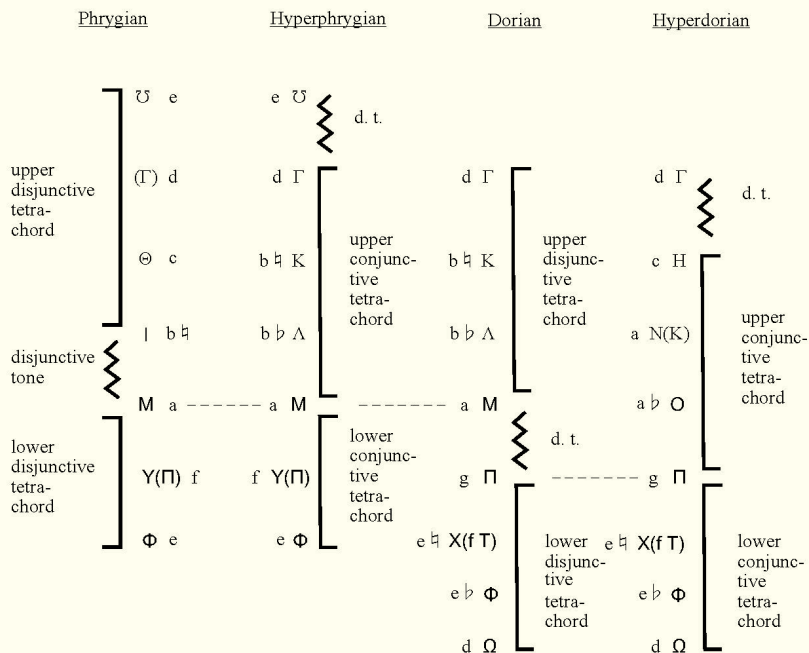


Figure 4. The scalar systems presupposed by Athenaeus' 'Delphic Paean' in Greek vocal notation (given in the chromatic genus of the tetrachord with alternative diatonic notes in parenthesis)

both lower and upper moveable notes. The upper moveable note is indicated by a sign that is specially reserved for the diatonic genus of this tetrachord. That sign looks like this: Γ .⁶ Hagel thinks that Γ serves as a pivot point for modulation. Indeed, by the beginning of the second part of the paean, the melody has clearly migrated into the neighboring $\tauόνος$, the one called Hyperphrygian. This is plain from the notation. Is the change of $\tauόνος$ here really a modulation in the sense that we have been discussing?

⁶ That sign looks like the Greek letter « Γ » because it *is* the Greek letter gamma. Athenaeus' 'Delphic Paean' is transcribed in the notational system reserved for singers rather than instrumentalists, which uses the letters of the alphabet for the central vocal register.

The answer depends, at least in part, on the way the change is brought about and on the relation between Phrygian and its Hyper next-door neighbor. It is easy to see that the two *τόνοι* might be thought of as relating to each other as GPS and LPS [see [Figure 4](#) for what follows]. Phrygian *mesē diezeugmenōn* and what we might conceivably regard as Hyperphrygian *mesē synnēmenōn*⁷ are indicated by the same sign—M—and coincide in pitch. They would normally share a common lower tetrachord, reflected in the notation by the use of the same signs. But the upper tetrachord in Phrygian proceeds in the upward direction from the disjunctive tone, whereas its Hyperphrygian counterpart does not. This means that, though Γ is a fourth above M in both *τόνοι*, it is only in Hyperphrygian that they both serve as the two bounding notes of a tetrachord. Next door in Phrygian, Γ falls within the tetrachord set immediately above the disjunctive tone. For, as I mentioned earlier, Γ is the upper diatonic moveable note of this tetrachord according to the conventions of the notation. Passing through M would be one way for a melody to move from the Phrygian equivalent of the GPS to the Hyperphrygian equivalent of the LPS. But another way would be to have the melody travel upwards in the Phrygian equivalent of the GPS and then get off at Γ and descend from there through the Hyperphrygian equivalent of the LPS to M. Something like that is what we find in the transition from the first part of Athenaeus' 'Delphic Paeon' to the second part.

I have to be cautious and say 'something like that', because the state of things here is messier than my remarks might otherwise suggest.⁸ But it does seem on balance that Γ, rather than M, is the point of transition from Phrygian to Hyperphrygian. This means, however, that we have a shift from GPS to LPS. Assuming that the conjunctive tetrachord of the LPS, whose lowest bounding note we are regarding as Hyperphrygian *mesē synnēmenōn*, is chromatic and,

⁷ In fact, strictly speaking, it turns out to be Hyperphrygian *hypatē mesōn*, the lower bounding note of the tetrachord whose top note is Hyperphrygian *mesē diezeugmenōn*. But that detail and its implications do not affect the point here.

⁸ For one thing, there is a lacuna at the beginning of line 9—the first line of the second section—and because the last note of the first section is a fourth below M, there is a significant downward leap, after the upwards noodling in the Phrygian equivalent of the GPS in the first section and before the upward noodling resumes at the beginning of the second section.

given that the tetrachord set immediately above the disjunctive tone in the Phrygian equivalent of the GPS is unequivocally notated with diatonic note signs, the effect of this shift is to present the listener with three neighboring semitones, where he or she would otherwise have heard only one, or perhaps two. Had the melody remained in Phrygian, the listener would have heard a semitone only between the lowest bounding note of the upper disjunctive tetrachord and its lower moveable note. Had it been confined to Hyperphrygian, there would have been two semitones: the two lowest intervals—the so-called chromatic *πυκνόν*—of the *synnēmenōn* tetrachord bounded by Μ and Γ. But the movement from Phrygian to Hyperphrygian gives us all three, though not in succession. Not surprisingly, it becomes convenient as a result of this shift to transcribe the second section with one flat more in the key signature than the first section.⁹ Indeed, this is what we find in the transcription of Athenaeus' 'Delphic Paean' printed in *Documents of Ancient Greek Music*, the new and improved version of Pöhlmann 1970, edited by Pöhlmann together with M. L. West and published in 2001, a year after Hagel 2000.¹⁰

Phrygian and Hyperphrygian are next-door neighbors. Modulating to the one from the other takes us no further than modulating from C-major to F-major. But the remarkable thing about Athenaeus' 'Delphic Paean' is that it modulates to more remote keys. At least that's the claim of Hagel 2000.

Everybody agrees that something is afoot here. The problem is how to understand what is going on. No sooner has the shift from Phrygian to Hyperphrygian taken place in line 10 [Pöhlmann and West 2001, no. 20] than a note is added to the mix that belongs to neither of the two original *τόνοι*. The note in question is indicated by the sign that looks like this: O. Pöhlmann and West characterize O as 'exharmonic' [2001, 73]. Hagel argues instead that we should take it as a modulation two doors down to Hyperdorian, i.e., at three flats removed from Phrygian. (In fact, he argues, at the end of the day,

⁹ I want to insist that it is a matter of convenience, assuming that, for the purposes of transcribing the piece into modern notation, we want to avoid cluttering up the staff with lots of accidentals.

¹⁰ I should note too that, for the purposes of my discussion of Athenaeus' 'Delphic Paean', I am referring to Pöhlmann and West 2001 rather than to Pöhlmann 1970. The paean is Pöhlmann and West 2001, no. 20.

that it is an ‘honest-to-God’ modulation to a key even more remote. But we will come to that in due course.)

The idea might be stated as follows. There is, in principle, a path that one can take from Phrygian to Hyperdorian, the *τόνος* in which *O* normally appears, simply by repeating the shift in the flatward direction that took us from Phrygian to Hyperphrygian the appropriate number of times [see Figures 4 and 3]. Yet another effect of modulating from Phrygian to Hyperphrygian was to transpose Phrygian *mesē diezeugmenōn* up a fourth. So when Athenaeus’ melody went up the Phrygian upper disjunct tetrachord from *M* to *Γ* and prepared to modulate from *Γ*, it began to treat *Γ* as the lower note bounding the disjunctive tone. For since Phrygian *Γ* is the upper diatonic moveable note of its tetrachord, there is a whole tone between it and the note above it. That interval is preserved in Hyperphrygian. But since Hyperphrygian *Γ* is the top bounding note of a tetrachord, the whole tone above it becomes the disjunctive tone, which means that *Γ* becomes Hyperphrygian *mesē diezeugmenōn*. This is, as I say, to transpose *mesē diezeugmenōn* up a fourth. We could, as we did earlier, refer to it now as MD*. Suppose that we subsequently transpose it down a fifth. Then, as it turns out, we will have modulated from Hyperphrygian to a key with one more flat. That key is Dorian. In Dorian, *mesē diezeugmenōn* lies a whole tone below *M*. It is indicated in the notation by the sign: *Π*.¹¹ We could, as we did earlier, refer to this note as MD**. To get to our intended destination in Hyperdorian, a key with one flat more than Dorian, we would have to raise MD** a fourth. This gives us MD***, which falls a whole tone *below* *Γ*. It is notated by *H*.¹² The note indicated by *O* is the lower moveable note in the Hyperdorian tetrachord whose upper bounding note is *H*, taken to be MD***. If we assume that this tetrachord is chromatic, then *O* is a semitone in pitch above *Π* and a semitone in pitch below *M*.

That there is a modulating path from Phrygian to Hyperdorian is a trivial consequence of our reflections on the forked path defined by GPS and LPS taken as a single system. The question, which Hage

¹¹ Note, however, that *Π* does not appear anywhere in what survives of Athenaeus’ melody.

¹² Note, however, that *H* does not appear anywhere in what survives of Athenaeus’ melody either.

wants to answer in the affirmative, is whether Athenaeus took this path—or, at any rate, something like it: as I say, the modulation Hagel takes Athenaeus to have carried out is, in fact, to a much more remote key.

Let me be clear about what is at stake. We can distinguish two different questions in the case at hand. The first is whether we can characterize Athenaeus as taking the multiple forked path to Hyperdorian (or perhaps beyond). The second is whether he deliberately took it, i.e., whether he would be willing to characterize himself as doing so. I take Hagel to be answering the second question in the affirmative (though ultimately this answer will pertain to the more remote modulation for which I have been sending up these red flags). Since Hagel takes Athenaeus' self-understanding to be correct, he—Hagel—necessarily answers the first question in the affirmative as well. But it is the second question that exercises him and upon which he expends his considerable energy and ingenuity.

The plausibility of Hagel's argument gets a boost from the notated melody itself. The first modulation of the piece—the one that occurs in line 10 of [Pöhlmann and West 2001](#), no. 20, in the transition from the first section to the second—is brought into focus by the contrast between the Phrygian upper disjunct tetrachord and the Hyperphrygian upper conjunct tetrachord. The former tetrachord sits on top of the disjunctive tone, indicated in ascending order by these signs: M and I. The tetrachord itself is notated in ascending order by these signs: I, ϑ, Γ, Ū. For convenience, we can notate the whole sequence, from lowest pitch to highest, using modern note names in a natural key. That would give us: a, b, c, d, e. The Hyperphrygian upper conjunct tetrachord is indicated by these signs: M, Λ, K, Γ. In modern note names, that gives us: a, bb, c, d. We can find unequivocally Phrygian strands of melody in lines 1 through 9. They stand out by their inclusion of b♯, indicated by ϑ. Thus, in the middle of line 6, we have ...M, I, ϑ, I, ϑ, Γ.... (In our notation, that would give us: a, b♯, c, b♯, d.) We can find an unequivocally Hyperphrygian strand of melody in line 10 at the beginning of §2. It stands out by its inclusion of bb, indicated by «Λ». (Note: assuming that the Hyperphrygian conjunct tetrachord is chromatic, and assuming equal temperament of the whole business, it will turn out that Hyperphrygian K and Phrygian I coincide in pitch, namely, b♯, as I am notating

it.¹³ This is one respect in which the ancient Greek notation may have an advantage over our modern notation: it can indicate difference of *τόνος* even when, as here, we might well have two notes of the same pitch.) It is in the middle of this otherwise Hyperphrygian line 10 that the seemingly ‘exharmonic’ *O* (*ab*) is introduced for the first time. Thus we have *K, Λ, M, O, K, Λ, K, Γ*... (In our notation, that would give us: *c, bb, a, ab(!), c, bb, c, d*.) How is Hyperdorian *O* possible in this decidedly Hyperphrygian environment?

Just as Phrygian and Hyperphrygian have a tetrachord in common on the way up to *M*, so Hyperphrygian and Dorian share a tetrachord in common on the way down from *Γ* [see Figure 4]. In descending order, it is the familiar: *Γ, K, Λ, M*—the tetrachord distinguished by *bb*, as indicated by *Λ*. In both Dorian and Hyperphrygian, *Γ* and *M* are both bounding notes of a tetrachord, namely *this* tetrachord. The difference between the two *τόνοι* is the placement of the disjunctive tone. In Hyperphrygian, the disjunctive tone sits on top of this tetrachord, and hence *Γ* is *mesē diezeugmenōn*. In Dorian, the disjunctive tone is suspended from *M*, so that *mesē diezeugmenōn* is a whole tone below *M*, as indicated by *Π*¹⁴—a note we may therefore render as ‘g’ in our notation. The path to *O* from Hyperphrygian is through this shared tetrachord with Dorian. For if we think of this shared tetrachord as Dorian, and if we think of it, moreover, as the GPS part of the forked path, then getting to *O* is no more complicated than hopping off at our intended LPS stop.

Now none of this sheds any light on Athenaeus’ self-understanding. We know that his ‘Delphic Paeon’ can be understood to travel along a path, from Phrygian to Hyperdorian, equivalent to a turn through the circle of fifths that leaves us with three flats more than we started with. But we do not know how Athenaeus himself understood what he was doing. All that anyone can do is to comb the evidence

¹³ Having said that, I think I should add that it is an open question in my mind whether *K* and *I* *do* coincide in pitch. That will be so, if all the relevant tetrachords are chromatic and the semitones are all the same ‘size’. But I do not think that we should take this for granted. I address this sort of issue below in my discussion of Hagel’s analysis in his new book of the melodies from the Ashmolean fragments [Pöhlmann and West 2001, nos 5–6].

¹⁴ But note, again, that *Π* does not appear anywhere in what survives of Athenaeus’ melody.

for clues and think again about his melody in light of them. This is what Hagel does. The result is both imaginative and stimulating. But it is also totally speculative. Speculation can be a very good thing when it invites others to think hard and long about a question. But it can also lead into temptation. Hagel could not resist; we can and should.

Having made such a pronouncement, I am now obliged to discuss the problem of what can and cannot be said about Athenaeus and his ‘Delphic Paean’. Though I did not set out to review both Hagel 2000 and the present book, Hagel 2010, I believe that this discussion is unavoidable, not just as a matter of fairness to the author but because of the intrinsic interest of Athenaeus’ melody, because its analysis is so important to the project of Hagel 2000, and because this analysis is not reprised in Hagel 2010—not at length or in detail—which means that the reader of the new book who is innocent of German will be missing an important presupposition that the later book seems to rely on. Then, last of all, both books exhibit the same weakness for speculation.

The best way to convey my unease about Hagel 2000 is to focus on lines 13 through 16 of Athenaeus’ paean [Pöhlmann and West 2001, 64]. The interest here is that the Hyperdorian O appears four times with Y, a note that, as I see it, may only be construed as the lower moveable note of the tetrachord shared by Phrygian and Hyperphrygian. Thus, we get:

YOM	line 13: f, ab, a,
OYO	line 14: ab, f, ab,
YOM	line 15 and
AMOYOMA	line 16: bb, a, ab, f, ab, a, bb.

By the time we get to line 13, we will no longer be surprised to hear O in succession with M because we will already have heard a pairing of these two notes three times since the very first introduction of O in line 10. If we understand O as a modulation into Hyperdorian, we will understand M to be the note that facilitates this move since it is common to Dorian, Hyperphrygian, and Phrygian alike: it is the natural jump-off point to O from these more remote keys. (Note, however, that M does not appear in Hyperdorian: O is the only note in the whole piece from this key, which is part of what makes it so

distinctive). By the very same token, it is not surprising to hear M and Y in the same melodic context since they are both shared by Phrygian and Hyperphrygian. The surprise is to hear Hyperdorian O sandwiched between them.

Hagel has an explanation for this. He says that Y can be understood as indicating an even more remote modulation—with the aid of O—into Hyperastian (also known as Hyperionic), a key one semitone lower than Hyperphrygian and so much further in the circle of fifths that it has left the flats behind and has four sharps (assuming for the sake of convenience that Phrygian, our point of departure in all this, is the natural key). He says, moreover, that this modulation to Hyperastian is clearly intended by Athenaeus. The issue is not merely how we can characterize the path taken by Athenaeus's melody; it is rather what is—or was—going on inside Athenaeus' head.

Hagel tries to support his claim with the following argument. It begins with the observation that O was first introduced in line 10 as the last note in a descending sequence of three semitones starting with the higher moveable note of the tetrachord shared by Hyperphrygian and Dorian: K, Λ, M, O (b \sharp , b \flat , a, a \flat). The next observation is that this is the last time in the piece, as it has survived, that we are treated to that many semitones in succession. This leads to the most important observation of all: in lines 12 through 16, O appears seven times in either an ascending or descending sequence of two and only two semitones, and indeed, the very same two semitones: O, M, Λ (four times: a \flat , a, b \flat); Λ, M, O (three times: b \flat , a, a \flat). The fact that these two patterns are repeated so often in such a short time suggests—to Hagel, at least—that they might be taken to be a 'chromatic pyknon' in their own right, i.e., the bottom of a chromatic tetrachord, with O serving as its lowest, fixed note and M and Λ serving as the lower and higher moveable notes respectively, such that O and M form some kind of semitone, and M and Λ form the next higher semitone.¹⁵ If that is the case, then Y has a new function once it is introduced in line 13. It can no longer be taken as the first moveable note of a Phrygian or Hyperphrygian tetrachord with M as its higher fixed note (*mesē diezeugmenōn* in the first case; *mesē*

¹⁵ Note that the ascending three note sequence of two successive semitones—M, Λ, K (a, b \flat , b \sharp)—is the chromatic *πυκνόν* of the tetrachord shared by Hyperphrygian and Dorian; it appears in line 12.

synēmmenōn, as we have been thinking of it, in the second). It would now have to be treated as the higher moveable note in a tetrachord whose higher fixed note is a semitone lower than M, namely O. A quick look at the Alypian tables of keys shows that the tetrachord of this description can only belong to Hyperiastian, eight cranks of the circle of fifths away from our point of departure in Phrygian or five cranks away from the tetrachord shared by Hyperphrygian and Dorian.

It might be objected that, if the interval OY (*ab*, *f*) indicates a modulation to Hyperiastian, we would expect Y to be noted with a sign at home in this key and not in Phrygian-Hyperphrygian. For, again, a quick look at the Alypian tables shows that the higher moveable note of this tetrachord in Hyperiastian is normally signaled by T. Hagel anticipates this objection. He points out that the presence of T would have caused hopeless confusion. For T also appears in the equivalent tetrachord of Phrygian and Hyperphrygian with the same function—that of a higher moveable note—but a semitone higher (*f♯*). (This is one of the disadvantages of the ancient Greek notational systems: notes of different pitch sometimes have the same sign.) Actually, it is worse than this in the case at hand because the two tetrachords also have the sign Φ in common. But while this note would be at the same pitch in both keys (*e♯*), if we are assuming equal temperament, as Hagel does, it clearly has a different function (as we saw above for Phrygian I and Hyperphrygian K): in the Phrygian-Hyperphrygian tetrachord, Φ is the lower *fixed* note; it is the lower *moveable* note in the Hyperiastian tetrachord. The result is that we cannot expect Athenaeus to have transcribed his Hyperiastian notes with Hyperiastian signs without risk of totally confusing his singers. Hagel’s point here is perfectly cogent, so far as it goes. But it raises the question: ‘How can we be so sure, just from the appearance of O and Y, that we are really in Hyperiastian?’

Hagel’s answer is to appeal to the immediate melodic context in which Y is introduced for the first time in line 13 [Pöhlmann and West 2001, no. 20]. In the lead up, we find melodic strands that are Phrygian with a Hyperphrygian twist of K and Λ. Thus, from the last note on line 12, we have

K, Λ, Γ, M, Ὠ, ϑ, Ι, ϑ, Γ, ϑ, Υ, Ο, Μ, Λ, Μ
(b♯, bb, d, a, e, c, b♯, c, d, c, f, ab, a, bb, a).

For Hagel's purposes, the sortie back into Phrygian (Γ , M, \mathcal{U} , \mathfrak{D} or d, a, e, c) is significant because the Phrygian note immediately preceding Y is \mathfrak{D} , which means that the interval between them is a downward leap by a fifth (namely, the fifth from c an octave above middle c to f). That is supposed to be significant because we can take \mathfrak{D} to be equivalent to the Iastian H, the second chromatic moveable note in the Iastian tetrachord above the disjunctive tone, whose lower bounding note is O. Hence, the downward leap of fifth of $\mathfrak{D}Y$, though notated in Phrygian, is equivalent to a leap from Iastian into a tetrachord it shares with Hyperastian. The effect of this modulation between neighboring keys is to modulate from Hyperdorian to a key a semitone lower, i.e., to Hyperastian, which is at a far greater remove in the circle of fifths—by no less than five cranks, as we saw. That this really is Hagel's argument can be seen on page 74 of his monograph. That it is supposed to get us inside Athenaeus' head is clear too. Hagel speaks explicitly of 'die Absicht des Komponisten' ('the composer's intention') and of 'die geplante Modulation um einen Halbtonschritt' ('the planned modulation by a semitone'). On page 73, in the introduction to this argument, he leads off with this:

Daß der Komponist nun tatsächlich eine solche entfernte implizite Modulation um einen Halbtonschritt im Sinn hatte....

Now that the composer did indeed intend such a remote, implicit modulation by a semitone....

The argument, however, establishes nothing of the kind. It shows only that a stretch of melody, whose notation indicates melodic travel from Hyperphrygian to Phrygian and back, with a seemingly out of place O in the middle of it all, can be redescribed as melodic travel from Hyperphrygian to Iastian to Hyperastian by way of notes that the latter two keys have in common with Phrygian, Hyperphrygian's immediate neighbor and the key we are taking as 'natural'. But this raises the question whether we have any good reason to describe the melody this way. Since what remains of Athenaeus' 'Delphic Paean' begins unambiguously in Phrygian and carries on in this key for the first nine lines with a single Λ pointing ahead to the Hyperphrygian bits on the horizon, it is more natural to characterize the notes in lines 12 through 16 that are indicated by unambiguously Phrygian signs as—well, er, uhmm—Phrygian notes. For this will ensure something valuable in music, and that is coherence. If you sing the piece to

yourself, you will still have the Phrygian notes in your memory when you get to line 13. You will almost certainly hear the Y introduced in the company of O from line 13 to line 16 as a Phrygian note (or possibly as Hyperphrygian, since Y appears in the tetrachord shared by Phrygian and Hyperphrygian whose top note is *mesē diezeugmenōn* in the one case, and *mesē synēmmenōn*, as we are thinking of it, in the other). Even if you, or your listeners (if you have any), do not know Phrygian from ‘Schmygian’, you will recognize, in lines 13 through 16, notes familiar to you from the opening. Though there is no way of knowing for sure, I would venture to say that even a real *Gelehrter* of Athenaeus’ day would not have heard a modulation from Iastian to Hyperastian in these lines, even if he could be brought to see that the bookkeeping could be understood to work out that way.

Aristoxenus says something relevant to all this:

One should not overlook the fact that musical insight is at the same time insight into something that remains the same and something that changes, and that this holds for almost the whole of music and in each branch of it. [Macran 1902, 2.33.27–32]

If we possess musical insight, we can discern what remains the same in a melody and what changes. That presupposes, of course, that we can retain what we have heard in memory and recognize it as the same when we hear it again. Of course, we do this all the time when we listen to music: with greater or lesser insight, depending on how well informed we are. Aristoxenus gives a number of examples, one of which—not surprisingly—concerns modulation:

And so too when, the same interval being put forth, a modulation comes to be in some cases but not in others. [Macran 1902, 2.34.9–11]

That is, we can discern, by musical insight, when a given interval occurs again in a melody and whether it has introduced a ‘modulation’, i.e., for the purposes of argument, a change of key in the relevant sense.

If we apply Aristoxenus’ idea to the problem at hand, we will be interested to see if there is some interval that occurs before the introduction of O and then in the context of the piece where O, Y, and M allegedly play a Hyperastian role. Then, we will want to consider

how likely it is that musical insight would judge that this interval introduces a modulation and, if so, what sort. Now, the obvious interval to consider for this purpose is the downward leap of a fifth, ϑY , so crucial to Hagel's Hyperian construal of O. As it happens, we do not find this interval in what remains of the first eight lines of Athenaeus' piece, i.e., before O is heard for the first time. It should be noted that there are lacunae, one of which occurs in line 5 *after* ϑ . So it cannot be excluded that ϑY would already have been heard before line 13. But, even if it were not heard before line 13, which is what we should assume to be on the safe side, we can still enlist Aristoxenus' idea to see how musical insight might judge things and thereby test Hagel's argument. For the tonal material of line 6 and that of line 13 are strikingly similar. In line 6 we get:

Y, M, Y, M, I, ϑ , I, ϑ , Γ , \bar{U}

(f, a, f, a, b \sharp , c, b \sharp , c, d, e).

In line 13, we get:

Λ , Γ , M, \bar{U} , ϑ , I, ϑ , Γ , ϑ , Y, O, M, Λ , M

(bb, d, a, e, c, b \sharp , c, d, c, f, ab, a, bb, a).

To be sure, our musical insight enables us to discern differences in line 13. There is the spice of O, in addition to the Hyperphrygian Λ . But those differences stand out against the things that our musical insight discerns as the same, notably the whole sequence of ϑ , I, ϑ , Γ (c, b \sharp , c, d). If we recognize that sequence in line 6 as Phrygian, surely we will recognize it as unambiguously Phrygian in line 13 as well, especially since that sequence is set off in both lines by two notes that may also be recognized as Phrygian, namely, M (a) and \bar{U} (e). Then, notice that the downward leap of a fifth, ϑY , so important to Hagel's argument, is heard for the first time in the piece (so far as we know) immediately after the sequence of ϑ , I, ϑ , Γ . Thereupon, we get the first melodic figure alleged to be Hyperian: YOM. What would be the judgement of musical insight about this figure and its variants? To start with, what would be its judgement of Y?

Too much is the same in lines 13 and 16 for it to be likely that Y, a note that has by now so solidly established itself in our musical insight as belonging to the Phrygian-Hyperphrygian sphere of influence, could be understood as the Hyperian T even be it in the company of O, and even if O appear to be the lowest note

of the Hyperian chromatic pyknon: O, M, Λ (ab, a, bb). Musical insight, in precisely the way it is characterized by Aristoxenus, is on the lookout for coherence. That does not mean that it cannot spot differences. It can and does, as when it distinguishes Λ (bb) and I (b̄), and as when it notes the oddity of O. But it does so in such a way that it understands what we hear as coherent, i.e., it recognizes what is different against the backdrop of what it takes to be the same again. It will not matter to musical insight that we can re-describe what it discerns as the same again as having some functional identity other than the one that matters to it. Even if the alternative description is possible, it will not be plausible, at least not to musical insight. It is the presumed judgement of musical insight that matters here.

At this point, one may well wonder how musical insight would characterize O in lines 12 through 16, if it characterizes the other notes, including Y, as Phrygian with a dash of Hyperphrygian just for fun. The answer is very simple: it might very well characterize O as ‘exharmonic’, just as Pöhlmann and West do. But that need not be a big disappointment because there is more that we can say about what makes O exharmonic.

If we assume that the genus of our tetrachords is chromatic rather than enharmonic, then it is indeed the case that O is some kind of a semitone lower than M. This is interesting because no matter which key we understand M as belonging to in context—whether it be Phrygian or Hyperphrygian—it is a fixed note. In Phrygian, it is *mesē diezeugmenōn*; in Hyperphrygian, it is *mesē synēmmenōn*. For the purposes of analyzing lines 12 through 16, it is more likely to think of M as Hyperphrygian *mesē synēmmenōn*, i.e., the lower fixed note of the tetrachord: M, Λ, K, Γ. Then, precisely because Hagel is right to point out that O so frequently appears in the sequence, O, M, Λ—whether ascending or descending—we can say that what makes O so distinctively exharmonic is that it has the effect of surrounding the fixed note M with semitones on both its lower side and its upper side. That is interesting because the ancient Greek theoretical treatises on music allow a semitone on the upper side of the lower fixed note of a tetrachord (both in the chromatic and the diatonic) but apparently disallow one on the lower side of such a note. In other musical cultures, having some kind of a semitone on both sides of such a note is not only *not* a big deal—it is considered musically interesting. This is certainly true in Arab music and musical cultures related to it. O is

therefore exharmonic to the extent that it breaks this rule. And that is what makes it so much fun too, as I am inclined to think, after improvising on the notes Y, O, M, Λ, ϑ, Γ (f, a♭, a, b♭, c, d) on the ‘oud in the Arab style, i.e., by treating M and Γ as defining the boundaries of a Kurdi tetrachord (a, b♭, c, d) and M, together with Y and O, as belonging to a lower, defective, conjunct Ḥijāzī tetrachord (<eḡ missing>, f, a♭, a). I always wondered whether actual ancient Greek musical practice rigidly adhered to the rule at issue here. Maybe the thing to say about Athenaeus is that he decided not to.

Now, I am not going to say that I know what went on in Athenaeus’ head. My point is that the argument which Hagel musters to get inside his head is inconclusive because it depends entirely on the observation that certain melodic figures in lines 12 through 16 can be described as Iastian-Hyperastian. They can indeed. But since they can also be described as Phrygian-Hyperphrygian, and perhaps more plausibly so, why should we believe that Athenaeus himself intended, planned, or conceived of them as Hagel says? You might just as well believe that the f♯ that I introduced into the little melody imagined earlier in this review [page 128] to illustrate change of key belongs to B-major rather than G-major and that I had thus effected a rapid modulation to a very remote key by briefly dropping my melody a semitone before restoring it to C-major—the key that it started out in. But that would be a crazy way to characterize things. Even if Hagel could show us that no description of the relevant melodic figures in lines 12 through 16 of the ‘Delphic Paeon’ is possible except for the one he offers us, that still would not get us inside of Athenaeus’ head. For it might still be the case that they are the residual side effects of the things that he, in fact, deliberately set out to accomplish in this piece (whatever they might be). One important difference between me and Athenaeus is that I am here to tell you that, in the little melody I imagined earlier to illustrate change of key, I deliberately introduced the single f♯ as a modulation from C-major in G-major. What Athenaeus deliberately intended in his melody and what is just an accidental effect of what he deliberately intended cannot be determined because he can no longer tell us himself. This matters a lot. If we cannot say that he deliberately intended O as a modulation to Hyperastian, and if it is simpler, more elegant and more plausible to characterize O as ‘exharmonic’, then the only ‘modulation’ that occurs in his ‘Delphic Paeon’ is the switch from the disjunct (Phrygian)

tetrachord of the GPS to the conjunct (Hyperphrygian) tetrachord of the LPS. We already knew that that sort of thing happens in the ancient Greek musical fragments. Alas, in spite of everything that I said earlier, it is not even clear that that sort of thing would have counted as a genuine modulation by all Greek music theorists. The forked road that I laid out earlier in my review is sometimes referred to as the σύστημα ἀμετάβολον. That literally means ‘the unmodulating system’.

I am now ready to take the reader into [Hagel 2010](#) through the back entrance. If she or he asks why we are not going through the front entrance, the answer is very simple. There is no front entrance. No single, unified program pulls the book together. *Ancient Greek Music* is really a scrapbook of hitherto unsolved puzzles and riddles. The solutions offered by Hagel are not necessarily related to one another because the puzzles and riddles are not always related to one another. But there are some hunches, conjectures, and conclusions that run through the book like recurring leitmotifs. These leitmotifs were already audible in [Hagel 2000](#). They include the idea, now familiar to us, that modulation to quite remote keys took place in ancient Greek music. That idea gets some more discussion in chapter 8 where Hagel reviews once again, but now in English, the existing musical fragments.

One important difference between [Hagel 2000](#) and [Hagel 2010](#) is that chapter 8 in the latter book follows the Pöhlmann/West collection of musical fragments [[2001](#)], which had come out in the meantime. This gives Hagel some new material to work over, notably [Pöhlmann and West 2001](#), nos 5 and 6 from a collection of cartonnage scraps in the Ashmolean Museum that are believed to go back to the third or second century BC. West had published these fragments in [1999](#). But since they are never mentioned in [Hagel 2000](#), I have to suppose that they came out as Hagel’s first book was going to press. Very little of this material survives. We are lucky to have 13 notes in succession in line 6, column two of fragment 15 from no. 6. For the rest, all we have is an isolated note here and there or a sequence of three notes, occasionally four. But Hagel tries to argue in the new book that what we have is enough to give us another example of modulation to remote keys. The analysis he offers owes much to his earlier account of Athenaeus’ ‘Delphic Paean’, which unfortunately

receives little explicit treatment here. ‘Without Athenaeus’ Paeon’, he says in the new book,

any attempt to interpret the mutilated melodies¹⁶ would be at a loss. With this piece as a guide, however, we learn from them that Hellenistic free modulation could go beyond what was sung at a traditional ceremony in Delphi. [269].

Maybe so. But the arguments that are supposed to get us to this conclusion are just as speculative as those in Hagel 2000. I will discuss them only to the extent that this will take us to the next assumption—or set of assumptions—shared by both books.

What survives of Pöhlmann and West 2001, no. 5 is, without doubt, an interesting piece (or set of notes) to stew over after Athenaeus’ ‘Delphic Paeon’ because the note signs can all be understood to be Hyperphrygian (Y, T, Π, M, Λ, Γ) with one seemingly exharmonic exception. This time the exception is N. It might be thought that N is the upper moveable note of the Hyperdorian tetrachord in ascending order: Π, O, N, H. H would be *mesē diezeugmenōn*, unless we had some special reason to take Π as *mesē synēmmenōn*. Whichever end of this tetrachord gets to be *mesē*, N is either chromatic or enharmonic by the conventions of the notational system. That means that it will either be a tone above Π (and thus identical in pitch—or roughly so—to Hyperphrygian M) or a semitone above Π and, thus, some kind of semitone lower than Hyperphrygian M. On the other hand, it could conceivably belong to other tetrachords, in which case it will likely be some kind of semitone higher than Hyperphrygian M. A quick look at the Alypian tables shows that it could just as well be the higher moveable note of either a chromatic or an enharmonic tetrachord in the following keys: Hypolydian, Hypoaeolian, Hyperastian, and Iastian. What Hagel hopes to show is that all the notes in no. 5 that are ambiguous between chromatic and enharmonic turn out to be chromatic and, in particular, that N turns out to be a semitone lower than M and, therefore, a semitone higher than Π, which would make it the same in relative pitch as O in Athenaeus’ ‘Delphic Paeon’. That would yield a string of *five* successive semitones:

¹⁶ *scil.* of the Ashmolean papyri.

- YT the semitone between the two chromatic moveable notes of the Hyperphrygian tetrachord whose higher fixed note is M,
 TII the semitone between the higher chromatic moveable note of this tetrachord and the one unequivocally diatonic note of this tetrachord,
 IIN
 NM and
 MA.

The value of that many semitones in succession—in the scalar system presupposed by the melody, if not in the melody itself—is that it would allow for rapid modulation to keys very remote from each other on the circle of fifths. But Hagel holds off from this conclusion until he gets to [Pöhlmann and West 2001](#), no. 6. So bear with me.

No. 6 is from the same cartonnage as no. 5 and possibly even from the same roll. Pöhlmann and West believe that the fragments from both Numbers may come from a single ‘music manuscript’ but they say ‘we should expect such a manuscript to have contained a number of different items...’. They say, moreover, that this is confirmed ‘by the presence in two places of a *paragraphos* accompanied by a *coronis*’ [2001, 38]. We must wonder, then, about the relation between the music in the two sets of fragments. On the face of it, it seems possible that what has survived could come from two different musical pieces or perhaps from more than two. But if we compare the note signs of no. 6 with those of no. 5, we will be struck by how many of them are the same. They are mostly Hyperphrygian and, indeed, the usual suspects: Y, T, II, M, Λ, Γ. K is missing but our seemingly exharmonic N is present, though less frequent. There are two Phrygian notes not found in what survives of no. 5. They are I and ϑ. Then, finally, there is one more seemingly exharmonic note that belongs neither to Phrygian nor to Hyperphrygian. It is Δ.

The overlap of note signs leads Hagel to say this:

The resemblances between Number 5 and Number 6 are so striking that they [*scil.* [Pöhlmann and West 2001](#), nos 5 and 6] can hardly be treated independently. [260–261]

‘Independently’ can, of course, be interpreted in many different ways. It is probably true that any observations we make about the note

signs we find in the one set of fragments should be informed by what we find in the other set of fragments. But Hagel takes ‘independently’ in a much more literal way. He goes on to speak of the melody or melodies of nos 5 and 6 as figuring in the same ‘tonal space’. This means, I think, that he is supposing that the same set of notes figured throughout the whole of the single ‘music manuscript’ of which we only have the fragments of nos 5 and 6. At any rate, he does nothing to caution us from taking him that way. So, however many individual melodies we may, in fact, be dealing with, Hagel apparently takes them all to have had all of the following notes (in order from lowest to highest, following Hagel):

Y, T, Π, N, M, Λ, K/I, ϑ, Δ, Γ.

I am uncomfortable with this conclusion. It seems hasty to me. But I will hold off.

Hagel also claims that the interval between every two successive notes in the sequence from Y to Γ that I just laid out, starting with YT, is exactly one equal-tempered semitone. So by the time he is finished with no. 6, he has no less than nine successive semitones. That is four more than he found in no. 5 all by itself. It is five more than he found running from 0 to ϑ in Athenaeus’ ‘Delphic Paeon’ back in 2000, as he himself points out on page 263 of the new book. The take-away lesson is supposed to be that the Ashmolean melodies must have had lots of very far ranging modulations. This is because four or more successive semitones gives us greater freedom to drop (or raise) a melody or melodic figure with a given arrangement of intervals by a semitone in the way exemplified by Athenaeus’ ‘Delphic Paeon’ according to Hagel 2000.

That modulation to a remote key requires fairly long trains of successive semitones is a point that can perhaps be made more concrete in light of the following example. Frère Jacques is a melody that spans the same *ambitus* as the material we have seen in the Ashmolean fragments: a major sixth, if we disregard the ‘ding, dang, dong’ bit at the end. Suppose that I sing it in C-major, a key with no sharps or flats. Then, suppose that I decide to raise the whole tune by just one semitone and sing it all over again in C \sharp major, a key with seven sharps—very remote from the natural key on the circle of fifths. In order to sing ‘Frère Jacques’ the second time, I will need to ensure that I have available to me all the sharpened notes I need.

No big deal if I am singing, but a bigger deal if I am playing some instrument that has, say, a limited number of open strings. For the purposes of singing or playing the tune the first time round, I will only need the notes of the C-major scale from, say, middle c to a above middle c. Among these notes, there is only one semitone: the one between e \sharp and f. If I were at the piano, I could play the whole tune on the white keys. But to sing or play the tune in C \sharp major, I will need to divide all the whole tones between middle c and a above middle c, i.e., the one between c and d, the one between d and e \sharp , the one between f and g, and the one between g and a. Plus I will need an a \sharp . If I am sitting at the piano, that means I will need the five black keys laid out on the keyboard between middle c and b above middle c. As a result, my rendition of ‘Frère Jacques’ first in C major and then in C \sharp major will require a train of ten successive semitones. Moreover, these will have to be equal-tempered semitones to ensure that the C \sharp major version does not sound weirdly out of tune. Hagel claims that the train of nine successive semitones which he believes that he has found in the Ashmolean fragments clearly indicates that the melodies from these papyri must have modulated to keys very remote from one another: as exemplified by my repeat performance of ‘Frère Jacques’.

Hagel puts it this way:

The variety of notes itself is sufficient proof that what we have here is music of a very sophisticated style. Yet we are surprised by its narrow compass of a major sixth; and even of this sixth, the higher notes appear only rarely, so that the major part of the melody is restricted to a mere fourth [*scil.* that between Y and Λ ?¹⁷ —AL]. Sophisticated melodies within so narrow a range are naturally impossible within a single scale. We must therefore expect that the music of the Ashmolean papyri is heavily modulating: which means that in the course of the melody the available notes must frequently rearrange themselves to new scalar patterns. [263]

¹⁷ Actually, it is not clear which fourth Hagel means here. I take it to be Y Λ , given what follows in Hagel 2010. But, of course, even if all that is true of the fragments from no. 6, it is not exactly clear what the ‘musically effective tetrachord’ of no. 5 is supposed to be.

Almost every clause of this passage is open to dispute. For starters, the first sentence is simply false. So too the claim that ‘sophisticated’ melodies are impossible within the compass of a fourth unless there is a lot of modulation going on, i.e., change of keys. In Arab music, and related musical cultures, a lot of very ‘sophisticated’ melodies take place within the compass of a fourth without any modulation at all. A friend of mine, a professional musician based in Montreal who grew up in Tunisia, likes to joke—usually in reference to the ‘*oud*-player Farīd al-’Aṭraṣh, but often as a universal principle—that all it takes for a really great melody is three notes (not exceeding a fourth).¹⁸ However jocular the remark, it is at the same time not intended as hyperbole. This matters. The point concerns how we approach scraps of real music from the distant past. If we expect to find in it music familiar to us from our own culture, then that is precisely what we will find. But finding what we expect to find is not responsible scholarship. However much imagination it exhibits, it will never be anything more than the result of a fancy Rorschach test. Scholars who study the material in Pöhlmann and West 2001 should do so as good ethnomusicologists might, i.e., with the expectation that they could discover almost anything in it.

Second of all, why must we expect that ‘the music of the Ashmolean papyri is heavily modulating’? If the answer is all those successive equal-tempered semitones, why be so sure that that is what we have? Here the answer starts with the claim that the Hyperphrygian and Phrygian moveable notes Y, T, Λ, K/I, ϑ are chromatic. Π is unequivocally diatonic. So, then, we purportedly have the following sequence: Y, T, Π, M, Λ, K/I, ϑ, Γ such that the only intervals that are not semitones are the whole tone ΠM and the whole tone ϑΓ. Then it is just a matter of inserting N half way between Π and M and then Δ half way between ϑ and Γ. But why should we accept these claims? Why, for instance, should we take for granted that the relevant moveable notes of the relevant tetrachords are chromatic rather than enharmonic? This question is not a quibble since the material at hand seems to go back to a time when the enharmonic genus of the tetrachord was still a live option. But then second of all, why must we think that N and Δ equally divide the whole tones ΠM and ϑΓ, respectively? Even if they do divide these whole tones, why

¹⁸ Mohamed Masmoudi, founding member of Sokoun Trio.

suppose that that will get us all the semitones Hagel needs? Let us take the questions about N and Δ first, but let us focus on N.

If we go strictly by the conventions of the notational system, N will either be some kind of semitone higher than Hyperphrygian M,¹⁹ some kind of semitone lower than Hyperphrygian M,²⁰ or N and Hyperphrygian M will be the same pitch (give or take).²¹ Whether we take the tetrachord of which N is a part to be chromatic or enharmonic in genus, these are the only options. (N is never an unequivocally diatonic note. For if we adhere strictly to the conventions of the notational system, it is always the higher moveable note of a tetrachord, and it is never the one such note that can *only* be construed as diatonic.) There will be questions no matter which of the three options we pick.

If we suppose that N and Hyperphrygian M are exactly the same in pitch, we will then have to wonder why they are notated with different signs. Here there are two possibilities. One is that they may be the same in pitch but different in function. But then, since N is the only note in the surviving scraps from whichever key it may perhaps belong to, this difference in function remains a mystery. Perhaps, on the other hand, N and M are not quite the same in pitch and the reason N appears at all is to signal to the singer to adjust a bit upwards or downwards from Hyperphrygian M. Hagel rejects this possibility:

Within this line of interpretation, there is room merely for a microtonal difference of tuning shade between M and N. But no ancient source recognizes a ‘modulation of shade’. [260]

The talk about shade (*χρόα*) here concerns slight differences among tetrachords of the *same* genus. For example, Aristoxenus recognizes a ‘soft’ diatonic whose intervals arranged from lowest to highest are a half tone, three quarters of a tone and one tone and a quarter, as well as a ‘tense’ diatonic whose intervals are a half tone, tone, tone. The difference between the two is a difference of ‘shade’, which is effected by flattening the central interval of the tense diatonic by

¹⁹ That would be in Hyperian chromatic and Hypolydian chromatic.

²⁰ That would be in Hyperdorian enharmonic and Hypoaeolian enharmonic.

²¹ That would be in Hyperian enharmonic, Hypolydian enharmonic, Hyperdorian chromatic, and Hypoaeolian chromatic.

an enharmonic δέσις. The discussion of ‘modulation’ in the surviving theoretical treatises certainly allows for change of genus, e.g., shifting from a diatonic tetrachord to an enharmonic tetrachord. As we saw earlier, this is one of the four kinds of ‘modulation’ that Cleonides explicitly mentions in the *Introduction to Harmonics* [von Jan 1895, 13.20.1–2]. Hagel is certainly right that there is no mention in Cleonides, or anybody else, of shifting from one shade of a given genus of the tetrachord to another. But that all by itself does not settle the question at hand. One of the questions at issue when we examine the surviving scraps of notated music is precisely what the relationship between theory and practice may have been. Perhaps some musicians liked changes of shade but such changes were not explicitly discussed in the treatises just because they were not taken to be significant enough changes to warrant any discussion. Perhaps such changes were discussed in treatises or parts of treatises that have been lost. Perhaps the conjectured microtonal difference between N and M in the Ashmolean papyri indicates something other than a change of ‘shade’ in the relevant sense. Here we could conjecture ‘til the cows come home’.

If we now suppose that N is some kind of semitone higher than Hyperphrygian M, we will face the same sorts of questions all over again. The easiest way to bring them into focus is to restrict our attention for now to the fragments of no. 5. For we find in these fragments notes that can be construed as the Hyperphrygian tetrachord: ΜΑΚΓ. This tetrachord is either chromatic or enharmonic. If it is enharmonic, then it seems that, on the hypothesis now under consideration, K and N are the same in pitch. But if they are the same in pitch, why are they notated with different signs? Perhaps they vary in function; but once again that difference remains a mystery because we still do not know which key N comes from. On the other hand, perhaps they vary slightly in pitch, in which case we are back to speculating about differences of shade. This time the question will be how enharmonic ΜΑΚΓ varies from enharmonic ΜΑΝΓ. The same questions will arise if we take ΜΑΚΓ as chromatic, except that they will concern the relationship between N and Α. If we now suppose that N is some kind of semitone lower than M, we will face another question, namely, whether, in fact, N belongs to Hyperdorian and, if so, why it is not notated as O, as in Athenaeus’ ‘Delphic Paean’.

In the face of all these questions, I think it is instructive to consider a different question, this time raised by Hagel himself: ‘But should we presuppose a strong interest in notational logic on the side of the composer at all?’ [260]. The answer is that perhaps we should not. If not, then perhaps Hagel is right to suggest that N was used for ‘the next note below M’. Why below rather than above? Hagel must be thinking that even if the composer’s interest in the ‘notational logic’ were not strong, it would not have so badly weakened as to fade away altogether. For all of the Ashmolean fragments use the vocal notation rather than the very different instrumental notation. The vocal notation is peculiar in that, for the central vocal register, it uses the letters of the Greek alphabet in alphabetic order. But the further in the alphabet we go, the lower we are in pitch. So if N is not to be thought of strictly according to the conventions of the notational system, namely, as a note that belongs to some specific key other than Hyperphrygian, but is to be thought of loosely according to these conventions, namely, as the next note after M, then the ‘notational logic’ suggests that N would be lower in pitch than M. But then by how much? Hagel says it would be lower by a semitone.

But why a semitone rather than something else? Hagel seems to assume that, if it were something else, it would have to be an enharmonic $\delta\acute{\epsilon}\sigma\iota\zeta$. He also seems to assume that it would be an enharmonic $\delta\acute{\epsilon}\sigma\iota\zeta$, if and only if the tetrachords of [Pöhlmann and West 2001](#), nos 5–6 were enharmonic. The flip-side of this assumption seems to be that if the interval between N and M were a semitone, then the tetrachords of nos 5–6 would have to be chromatic. None of these assumptions can be justified for the simple reason that N is the note that does not belong, just like O in Athenaeus’ ‘Delphic Paean’. But they are very important assumptions for Hagel. For if he can find some reason for rejecting the enharmonic, he gets semitones all the way down. N will divide the whole tone ΠΜ. For that matter, Δ will divide the whole tone ϑΓ, assuming—perhaps not unreasonably, but who knows?—that Δ and N are a fourth apart. Then, to sweeten the deal, all the surviving Phrygian and Hyperphrygian moveable notes—Y, T, Λ, K, I and ϑ—will be chromatic (K and I will be identical in pitch). The result will be nine successive semitones from Y to Γ and, hence, freedom to modulate far and wide. The trouble is that it all just seems much too convenient. Why take for granted that, if NM is an enharmonic $\delta\acute{\epsilon}\sigma\iota\zeta$, all of the tetrachords of nos 5 and 6 are

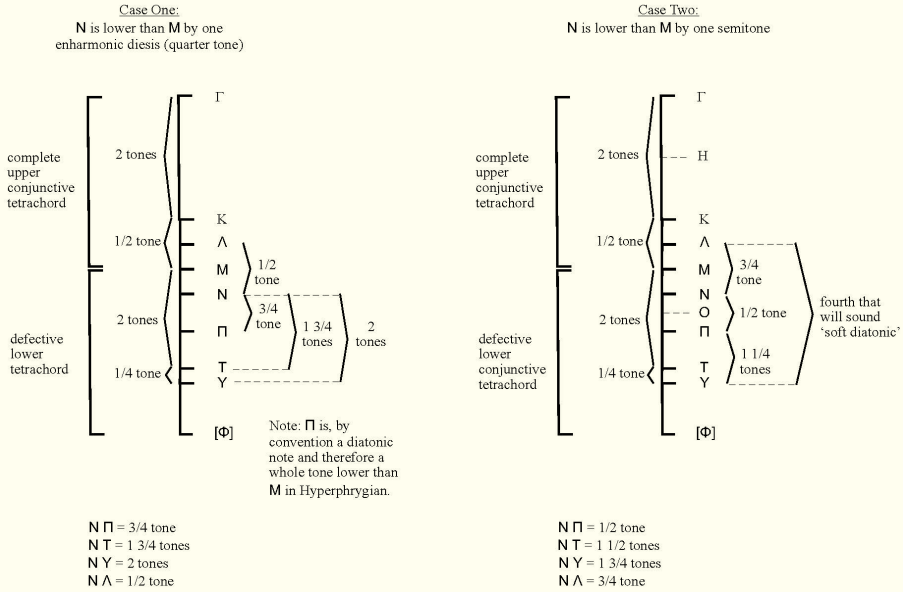


Figure 5. Two ways of construing the tonal material of Pöhlmann and West 2001, no. 5 on the ‘enharmonic reading’

too? Why could the tetrachords of nos 5–6 not still be enharmonic even if NM were not an enharmonic $\delta\acute{\iota}\epsilon\sigma\iota\zeta$ but rather a semitone after all?

To reject the enharmonic construal of the relevant tetrachords, Hagen helps himself to a claim made by Pöhlmann and West in their commentary on no. 5.

Pöhlmann and West also seem to take for granted that if the melody or melodies of no. 5 are enharmonic, then N is lower than M by an enharmonic $\delta\acute{\iota}\epsilon\sigma\iota\zeta$. But then they consider which intervals N forms with which of the surviving notes from these fragments [see Figure 5, case 1]. To start, N forms an interval with Y. Indeed, NY turns up five times in the first three lines of no. 5, fr. 1. We also find one occurrence of NII in line 4 of fr. 1, one occurrence of NT in line 9 of fr. 3 and one occurrence of NΛ in line 11 of fr. 3. With the relevant enharmonic assumptions in play, NY will be some kind of ditone, NII will be roughly three quarters of a tone, NT will be roughly one tone and three-quarters, and NΛ will be some kind of semitone. Pöhlmann

and West disqualify the ‘enharmonic reading’ of no. 5 on the grounds that these intervals are ‘clearly...less plausible’ than the intervals we would get on the ‘chromatic reading’, i.e., on the assumption not only that Y, T, Λ, and K are chromatic but that N is taken a semitone lower than Hyperphrygian M. On the chromatic reading, NY will be one tone and a half, NII will be some kind of a semitone, NT will be a whole tone, and NΛ will also be a whole tone. Hagel embraces the Pöhlmann/West claims without comment [260 and n12]. This seems to me much too hasty.

I take it that the intervals formed by N with Y, II, T and Λ in no. 5 are on the ‘enharmonic reading’ implausible not because of the difficulties that they might present to singers or instrumentalists. For the two that might seem oddest to those of us minimally tutored in ‘the music of the Western World etc...’ are NII at three quarters of a tone and NT at one tone and three quarters. But depending on how the quarter tones add up in practice, these intervals may well turn out to be intervals heard in Arab music and related musical cultures all the time. The Arab equivalent to NT is the interval distinctive of the ‘Rast’ tetrachord, i.e., the slightly wonky third formed by the Rast final, which might be c in the octave of your choice and its third degree, which is then an e neither flat nor natural but somewhere in between. The Arab equivalent to NII is the interval between the two inner notes of the Rast tetrachord: call them d in the octave of your choice and wonky e. Both of these intervals are easy to sing and vastly easier to sing than the weird and horrible seventh diminished by an enharmonic $\delta\acute{\iota}\epsilon\sigma\iota\varsigma$ that one finds three times in what survives of the Orestes fragment [Pöhlmann and West 2001, no. 3]! So the implausibility of NII and NT will turn on something else, namely, the alleged fact that they are not attested in the surviving theoretical accounts of the enharmonic genus of the tetrachord.

But lots could be said here. One simple conjecture would be that, though the tetrachords (explicit and implied) of no. 5 might well be enharmonic, perhaps N is indeed some kind of semitone lower than M and higher than II [see Figure 5, case 2]. Indeed, perhaps the ‘notational logic’ is strictly adhered to such that N is a Hyperdorian note that belongs to the enharmonic tetrachord ΠΟΝΗ, with H taken as *mesē diezeugmenōn* and thus a whole tone lower than Γ. Perhaps O and H are, in fact, part of the melody or melodies of no. 5 but have not survived in what is left of the manuscript. If that

were the case, we would have something not much more complicated than a modulation from disjunctive tetrachords to the neighboring conjunctive, *synēmmenōn* tetrachord. The only complication would be that, as we saw earlier in the discussion of Athenaeus' 'Delphic Paeon', Hyperdorian is two doors down from Hyperphrygian in the flatward direction. Hence, the connection would have to be made either through the enharmonic *πυκνόν* shared by Hyperphrygian and Dorian MAK or by one or both of the notes that Hyperphrygian, Dorian, and Hyperdorian all have in common, namely, Π and Γ. This would still give us a modulation in Hagel's sense, but it would take place between fairly close neighbors and not—as Hagel would have it—between remote keys in such a way as to raise or drop a melodic figure by a semitone. But what makes this conjecture interesting to me is that it keeps everything in conformity with the official playbook but it preserves the allegedly 'implausible' intervals that Hagel and Pöhlmann/West were prepared to rule out of court. It is just that those intervals will now be found between different pairs of notes than the ones predicted earlier. We now find the interval of three quarters of a tone not between N and Π but rather between N and Λ; and we now find the interval of one tone and three quarters not between N and T but rather between N and Y.²² The upshot of all this is once again that we find a rush to judgement in Hagel—and not just in Hagel, but Pöhlmann/West too.

But there is something else that makes this conjecture worth considering—again in the interest of slowing the rush to judgement. This will also give us a point of contact, down the road, with no. 6.

Suppose again that the Hyperphrygian tetrachords of no. 5 are enharmonic, and that N is a semitone lower than M and a semitone higher than Π [see [Figure 5, case 2](#)]. But let us follow Hagel this time in supposing, moreover, that N does not belong to any specific key as such, but that it is just the 'next note below M'. Then, it is possible to understand what survives of no. 5 as showing no modulation at all, i.e., no change of key. If there is 'modulation' here, we might just as well understand it as change of genus, i.e., arrangement of the tetrachord.

²² The semitone is now found between N and Π rather than between N and Λ, and the interval between N and T is one tone and a half. There is no ditone except between M and T.

This can be seen if we take the fourth bounded by Y and A and then notice that, with the insertion of II and N, we get the following successive intervals from lowest to highest: YII is a tone and a quarter; IIN is a semitone; NA is three quarters of a tone. The interest of these intervals is that they are definitive of Aristoxenus' 'soft' diatonic. Now, according to the official playbook, the normal form of the tetrachord, whatever its genus, is that its smallest interval should be the lowest and its largest interval should be highest. The central interval may either equal the lowest, or it may equal the highest, or it may be larger than the lowest (but more usually smaller than the highest). That rule implies that, in a well formed soft diatonic tetrachord, the order of intervals from lowest to highest would be semitone, three quarters of a tone, a tone and a quarter. But all that follows from this rule as such is that YIINA is not itself a well formed soft diatonic tetrachord. It is, nevertheless, a sequence of successive intervals spanning a fourth that one might plausibly encounter in a soft diatonic melody. For suppose that a soft diatonic melody is based on a system of two conjunctive soft diatonic tetrachords, and that it moves about for a time within the fourth bounded by the higher moveable note of the lower tetrachord and the higher moveable note of the higher conjunct tetrachord. Then, the sequence of intervals from lowest to highest will be one tone and a quarter, a semitone, three quarters of a tone, i.e., the same sequence from lowest to highest that we get in YIINA. The musically trained listener who heard that melody as soft diatonic would presumably hear any part of the melody or melodies of no. 5 that moved in and through YIINA as soft diatonic.²³ But

²³ Here is another way to put it. The tetrachord YIINA does not fall within the bounds of Hyperphrygian fixed notes, contrary to what one might have expected. But one may well wonder why a melody of a certain *τόνος* could not establish itself between moveable notes. Pöhlmann and West 2001, no. 32, which is from one of the anonymous texts on music collected by Bellermann [Najock 1975, 33 (§104)], gives us an interesting parallel: a melody seemingly notated in Lydian that establishes itself within an octave between Lydian moveable notes rather than Lydian fixed notes. This does seem odd because we expect the fixed notes of a *τόνος* to have some kind of special melodic significance and that the melody should somehow insist on these notes. But, again, our expectations may not always be a good guide. It is instructive to note that Winnington-Ingram [1936] had such expectations and expresses his exasperation at the end of his treatment of the surviving musical fragments. He was looking, in particular, for evidence

now we remember that the melody or melodies of no. 5 also featured notes definitive of enharmonic tetrachords (enharmonic by hypothesis): MAKΓ and YTM. The latter system, as it stands, is defective but it is at least suggestive of the full enharmonic tetrachord: ΦYTM. To the extent that the melody or melodies of no. 5 moved in and through these systems, the musically trained listener would presumably have heard them as enharmonic. The upshot is that nothing seems to stand in the way of understanding the melody or melodies of no. 5 as exhibiting modulation, not at all in the sense of change of key but rather as a change of genus of the tetrachord. I find nothing in Hagel's analysis that would exclude this possibility.

I should note that the Pöhlmann/West commentary on no. 6 takes the surviving notes from these fragments—M, Λ, ϑ, Γ—to form a soft diatonic tetrachord. The odd note here is ϑ because it is Phrygian while the other surviving notes—Y, T, Π—are Hyperphrygian with the exception of N and Δ. If the tetrachord bounded by M and Γ were a normal (tense) diatonic, we would expect the second moveable note to be indicated by the sign H. Thus, we would have MAHΓ.²⁴ Pöhlmann and West take the appearance of ϑ in the place of H to signal the flattening of the diatonic note. Hagel naturally resists this idea because it would compromise his claim that the melodies in nos

that the fixed note *mesē* would be found to be modally significant in the fragments. But he could not find such evidence. He says:

Thus, though the fragments give some support to the scheme of tonics and modal analysis that has been based on the Aristoxenian doctrine of the species of the consonances, this support is very incomplete.... [1936, 46]

Hagel too discusses modality in the new book. See, in particular, pages 219–229. His conclusions seem tentative.

²⁴ It may be a surprise that, even in the Hyperphrygian diatonic tetrachord, the first moveable note is indicated by Λ because we have already found that Λ could indicate the first moveable note in this tetrachord in both the enharmonic and chromatic. But, by the conventions of the notational system, the sign for the first moveable note of any given tetrachord is the same for all three genera.

5 and 6 were laid out on a grid of nine successive semitones.²⁵ He may well be right to say that Pöhlmann/West are wrong on the soft diatonic in no. 6. But I find his arguments inconclusive. The only thing that matters here for my purposes is this. Suppose for the sake of argument that Pöhlmann/West are right about the soft diatonic tetrachord in no. 6. Suppose too that the function of \mathbf{N} and $\mathbf{\Delta}$ in these fragments is to facilitate change to and from this genus to some other genus. If so, the interest of both nos 5 and 6 may well have been that they illustrated change of genus rather than change of key.

Hagel reviews many more fragments of ancient Greek music in chapter 8 of his new book. We have now seen enough to appreciate his perspective on this material. This will allow me to give the reader a better sense of some of the rest of the new book.

We have already seen that Hagel's claim to find evidence in ancient Greek music of modulation to and from keys remote from each other on the circle of fifths depends always on the claim to have found long sequences of successive semitones (indeed, equal-tempered semitones or roughly so). If his claims be accepted, and if modulation to and from remote keys was common practice, we should expect a preference for the (tonic) chromatic genus of the tetrachord where the two semitones of the $\pi\upsilon\kappa\nu\acute{o}\nu$ are roughly equal. The idea is that if one can align tonic chromatic tetrachords in neighboring keys in the right way, it should be possible to get a train of successive semitones long enough to take shortcuts to the more remote keys. It is clear that a preference for the tonic chromatic is assumed in Hagel's analyses of Athenaeus' 'Delphic Paeon' and the Ashmolean fragments. But the assumption was, in fact, stated explicitly in Hagel 2000 [72: cf. 87–88]:

Voraussetzung für die Konstitution einer Reihe von drei ‚Halbtönen‘ ist natürlich die chromatische Stimmung mit einem Pyknon aus (wenigstens ungefähren) ‚echten‘ Halbtönen...Nur mit der Grundlage einer solchen Stimmung kann auch eine Modulation um einen Halbtönenschritt geschehen.

²⁵ As he says elsewhere in the book, speaking of the 'soft' diatonic:

such a division is in principle mutually exclusive with *synēmmenōn* modulation, which requires a stable whole tone at the top of the tetrachord. [141]

A precondition for the setting up of a sequence of three ‘semitone steps’ is, in the nature of the case, the chromatic tuning with a pyknon of (at least roughly) ‘true’ semitones...A modulation by a semitone can occur only on the basis of such a tuning.

But this assumption raises a challenge for Hagel because the surviving theoretical treatises do not seem to privilege the tonic chromatic, and because they happily envisage alternatives to it: different shades of the chromatic, the enharmonic and its different shades, as well as the diatonic and its variants.

Actually, it is more challenging than that for Hagel because his account would lead us to expect that the tonic chromatic would at least find special favour with Aristoxenus—and this for a couple of reasons. First, Aristoxenus seems to have thought carefully and systematically about modulation in the sense of change of key, perhaps, as Hagel says, in response to musical innovations of the fifth century BC. It is unfortunate that his account is lost; but its broad outline is preserved in Cleonides. Second, Aristoxenus rejects numerical ratios of whole numbers as the way to define intervals and tunings. Third, he is naturally understood as committed to the idea that the octave is the sum of six whole tones each of which can be divided into equal semitones. This opens the door to equal temperament, otherwise closed to theorists who insist on numerical ratios.²⁶ But, for all that, Aristoxenus expresses an unequivocal preference for the then out of fashion enharmonic which has a ditone as its top interval presumably equivalent—though Aristoxenus would never put it this way—to an interval with a ratio of 81:64 produced by two Pythagorean whole tones (each 9:8).

This is a challenge for Hagel. He meets it in chapter 5 with a two-pronged strategy, if I really understand what is going on here. First, he takes Aristoxenus’ preference for the out-of-fashion enharmonic as a ‘hobby-horse’ [155]. Second, he reviews all of the surviving theoretical discussions that define intervals and tunings by numerical ratios and divides them into the sheep and the goats. The goats are those that may be shown to be totally ‘crackpot’ from the point of

²⁶ Aristoxenus’ countryman, Archytas, had shown, after all, that there are no natural numbers that could express as a ratio the interval that exactly divides the whole tone.

view of musical practice, like, for example, those who are directly and perhaps exclusively motivated by cosmological issues (Nicomachus and ‘Timaeus Locri’) or those who seem interested in the mathematics of numerical ratios for their own sake (possibly that is Hagel’s judgement of Archytas, at the end of the day, though he does credit Archytas with a significant interest in the *aulos*). The sheep are those who can be shown to be responding to musical practice. In truth, it turns out that there are no sheep ‘pure laine’, as we might say in Québec, but rather only goats with certain sheep-like qualities that can be most clearly detected when they are forced to contort their mathematical commitments or quite possibly fudge their results (this is especially so in the case of Ptolemy). For the purposes of judging the ratio of goat-to-sheeplike qualities, the ‘musical practice’ of relevance here is, in the first instance, different from the one that I have been discussing. It will be the practice of performance on string instruments: the lyre and the cithara (no great surprise here because string-lengths can be readily compared in terms of numerical ratios). This is not to say that modulation did not take place on these instruments. It did. But first of all, Hagel says that the impulse for remote modulation came from another instrument, the *aulos*; and, second of all, the string instruments posed their own special problems, namely, how to ensure or maximize the richness of tone of those strings whose pitch could not, for whatever reason, be achieved by tuning through perfect fourths and fifths.

Didymus and Ptolemy are of the greatest interest here. Ptolemy all the more so, not only because his book on *Harmonics* survives but also because, as Hagel tries to argue, Ptolemy’s program of squaring the judgement of ear and mathematical reason fails ultimately when it comes time to test the different divisions of the tetrachord familiar to the trained, musical ear against the findings of the eight-string canon. Here Hagel argues that we find either duplicitous fudging or—one would prefer to think—self-deception. This would consign Ptolemy to fully-fledged, irredeemable goathood were it not for two things. First, Hagel argues that it indicates an awareness of genuine problems in providing for maximally resonant intervals smaller than the fourth (pure thirds). Second, Hagel argues in chapter 4 that Ptolemy’s report of contemporary lyre and cithara tunings may be regarded by and large as trustworthy (and, therefore, very valuable). I am not yet sure what I think about the details of Hagel’s account of Ptolemy

on the divisions of the tetrachord. But I think it is quite likely that Hagel is right to say that the outcome of Ptolemy's experimental tests will leave us shaking our heads; and I believe that figuring out the details here matter to our understanding of Ptolemy's legacy—and not only for music theory. Jamil Ragep [2009] has argued that the distinctive concern for observational precision in astronomy in the Islamic world was motivated at least in part by the awareness of, and irritation due to, the excessive neatness of Ptolemy's numbers. What a serious study of Ptolemy as both astronomer and music theorist may well show is just how hard it is to carry out the program of squaring mathematical reason and judgement of the higher senses.

Because almost all of the surviving ancient Greek treatments of music theory privilege numerical ratios, one could easily get a disproportionate sense of the significance of the string instruments for theory and practice alike. Part of the significance of Hagel 2010 is its effort to compare the significance of the string instruments (in chapters 4–5) and its oft overlooked competitor, the *aulos*. Chapter 9 collects and reviews the surviving literary and archeological evidence for this instrument and the way it may have developed over time. The special contribution of the *aulos* to the story is precisely what would have made people like Ptolemy turn away from it as a theoretical aid in preference for stringed- instruments, namely, its lack of precision in intonation. To be sure, it had finger-holes; once they were bored, they remained where they were until the instrument disintegrated. If you fully stopped those holes in the relevant ways, you would get a determinate pitch, all other things being equal. But in actual performance on a woodwind, all things are not equal. You can more or less cover a hole with your finger or incrementally cover it and thereby vary the pitch. Of course, *embouchure* can produce very fine changes of pitch. That is why the clarinetist, at the beginning of Gershwin's 'Rhapsody in Blue', can produce such a dramatic *glissando*, the like of which is impossible on the piano—much less on the ancient Greek lyre or cithara. So equal temperament—or thereabouts—and the possibility of modulation to remote keys found its home in performance on the *aulos*. The full actualization of this possibility is supposed by Hagel to have come about in the fifth century BC with a 'new music' documented only in literary testimony. All one can tell for sure is that this 'new music' was received at the time with bemusement and amusement, as new music almost

always is. But Hagel believes that the literary evidence gives some (circumstantial) evidence that the new effects of this music were due to modulation. It is important, for his purposes, that the ‘new music’ was associated with the names of *auletes*. The significance of his analyses of the surviving music that I discussed is that, if he is right to say that they exhibit modulation to remote keys, then they can be understood to give us a sample of the ‘new music’ even though they were composed much later in time. Finally, I should add that Hagel takes the system of ancient Greek musical notation to have evolved to answer the needs of this modulating *aulos* music—the system does indeed follow the circle of fifths: Hagel provides a nice diagram showing this on page 13. He offers an account of its puzzles in chapter 1 and tries to reconstruct its evolution from the internal evidence of the system itself (as, say, documented in the Alypian tables). I have to say that I find this reconstruction totally farfetched. But be that as it may, one way of understanding what is going on in [Hagel 2010](#), as a book rather than as a collection of essays, is that it tries to show how the coexistence of aulos and string instruments, uneasy though it may have been, was possible.

On balance, the thing to say about [Hagel 2010](#) is that it is indeed a very stimulating book but at the same time very frustrating. The biggest source of frustration is its high tolerance for speculation that at times morphs into wishful thinking. But there are other significant frustrations. It was not well edited. The English is sometimes so clunky that it was easier to mentally back-translate it into German. Even then, I often had the impression that the author was talking to himself rather than to his reader. The book is also very badly organized. I could never tell where the discussion was headed. Often I could not tell how or whether earlier material was significant for later chapters. I initially took the attempt to reconstruct the development of the notational system in chapter 1 to be foundational for the rest: that was suggested by the presentation of the riddle associated with this system—Dorian is Hypolydian!—as being deep and fundamental. Fundamental though it may be, its solution did nothing to advance later discussion in the book. As a result, I could not understand the order or even the choice of topics until I read [Hagel 2000](#). Even then, [Hagel 2000](#) does not help motivate everything in [Hagel 2010](#). The author never provides a map of his project in the new book or a single, self-contained, coherent statement of his overall motivations.

I have tried to do that in this review; but I am still not confident that I really have the big picture, much less the details. That may reflect on me but it also reflects in large measure on the author. The burden of communication falls on his shoulders at the end of the day. The last thing to say is that there is no index of subjects. In a book this messy and sprawling? *Ô stupeur!*²⁷

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²⁷ I am grateful to my colleague Peter Schubert for helping me to understand more clearly the issues raised in this review and for kindly giving me feedback on this review. I also thank Mohamed Masmoudi and Matthew Provost.

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