

## Comment on Huron and Veltman: Does a Cognitive Approach to Medieval Mode Make Sense?

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**ABSTRACT:** This commentary examines Huron and Veltman's article from the perspective of historical musicology. The following issues are discussed:

- The authors regard modes as conceptual categories of the medieval listener, which seems unlikely on historical and theoretical grounds.
- Pitch class profiles are not a good way of capturing the melodic nature of the modes.
- The diatonic rather than the chromatic scale should be employed as the reference pitch system for the modes.
- The tentative explanation of the transition from modality to tonality ignores the fundamental differences between modes and keys, and the role of polyphony in this supposed transition.

The article's methodology, to apply quantitative methods to problems of historical musicology, is fundamentally sound, and suggestions are made in this commentary as to how its shortcomings can be amended by reformulating research questions and redesigning methods.

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I have read Huron and Veltman's article with considerable interest. I must say I am much more convinced by its methodology, to apply quantitative methods to problems of historical musicology, than by some of its conclusions. I have my doubts about the concept of mode the authors seem to have, some of the ways of testing this concept, and finally of the historical perspective of their findings. For the sake of argument, I've tried to be very explicit in expressing my views, which I trust will not be interpreted as an act of disrespect to the authors.

My response will be mostly from the point of view of historical musicology. My PhD thesis was on the subject of polyphonic modality, and dealt with the very interesting role this phenomenon played in the Renaissance and early Baroque. In studying this particular phase of music history, many researchers have wrestled with the relationship of modality and tonality. Some place the transition of the one into the other as far back as the early 15<sup>th</sup> century, whereas others claim that tonality only fully emerged around 1700. Whether the one is true or the other, modality had a life span of at least the same length as that of tonality; yet it is surprising to observe to what extent our conception of modality seems to have been shaped by its supposed decline rather than by its success and longevity.

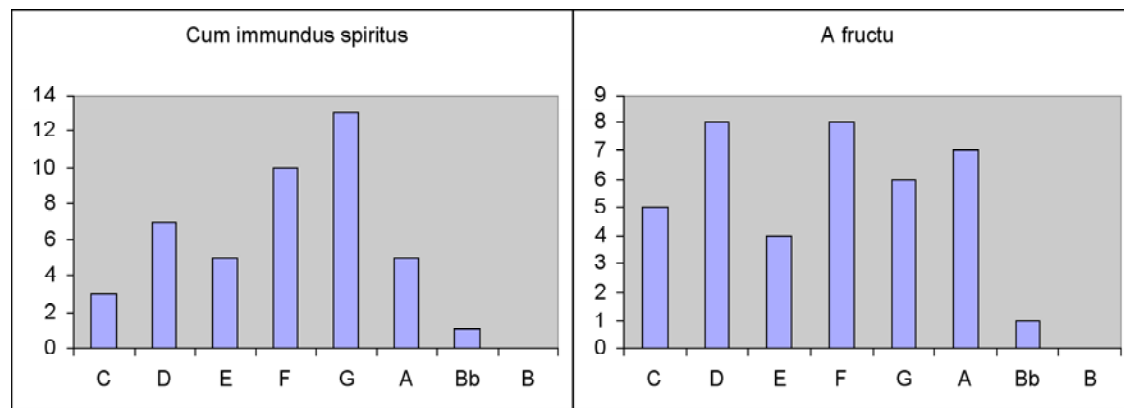
But are the modes indeed historical analogues to the keys of tonal music? Are modes the conceptual categories that medieval and Renaissance listeners intuitively employed to structure their pitch perceptions around when listening to Gregorian chant, and even polyphony? There is good reason to believe they are not—which should not be taken to mean that they were “tonal” listeners.

Modes appeared in Western music around 800, as one of a number of innovations that seem to originate in attempts to standardize and codify liturgical chant. From a musical point of view the problem was that the repertoire was entirely oral and therefore unstable. The ultimate solution was of course music notation, but its development was a slow process. Modality can be seen as the quick-and-dirty solution to the most pressing of the issues: how to connect psalm recitation and antiphons. I will not expound the details of this solution here (see Powers/Wiering 2001) but it entailed the classification of chants by their final, range or *ambitus*, and initial melodic gesture (in this order) and a parallel classification of recitation formulas by reciting tone and initial and closing formulae. Such classifications were recorded in lists of chants called tonaries.

In this manner, the modes provided an effective tool in assembling parts of the liturgy; moreover they soon acquired the status of what Harold S. Powers has described as “the Church's one and only musical dogma” (1982). So they did not become obsolete as musical literacy increased. This is not to say that there was a good fit between the modes and the repertoire. On the contrary, many melodies seemed to display characteristics of more than one mode and had to be reworked. Other

melodies proved to have too few distinctive features, and new features had to be defined in order to be able to deal with these melodies. Even though modal features such as *repercussio*, types of ambitus and even scale itself were introduced to modality as explanations of musical practice, these innovations were taken up as fuel for theoretical abstraction. Scalar aspects of mode in particular were connected to notions stemming from Boethius's account of music as a mathematical discipline. Such considerations tended to push modes towards the realm of abstract ideas, as if they were ideal, archetypal melodies that God had created but we humans can recreate only imperfectly (Wiering 2001, 162-64).

Some of the issues just mentioned can be seen in action in the chants in the article's Figure 1. Ignoring the given modal designations it is pretty obvious that these must be Dorian; whether they are authentic or plagal is harder to determine. The melodies have a very similar ambitus: if we consider the low A in the second melody as an outlier, both have a range from C-Bb, which leaves us pretty much in the dark as concerned theoretical ambitus. Forced to make a decision about the second melody this A comes in handy, for there is a complete octave species A-A, which is a unique feature of mode 2. It is highly questionable if this decision is consistent with a "structural" view of mode, for the pitch class profile of this melody (shown below in Figure 1) resembles the profile of mode 1 more closely than that of mode 2 (article, Figure 3). By the way, this is a qualitative judgment: I haven't calculated the correlations.



**Fig. 1.** Pitch class profiles of the melodies from Huron and Veltman, Figure 1

The profile of the first melody is equally perplexing. It does not correspond at all to the profile of mode 1, nor to one of the others (the closest I came to one of the modal profiles was by considering this melody to be in mode 2 on G). As said, the ambitus is not going to help. The answer seems to lie in the initial melodic gesture, which quickly rises from D to A. Compare this to the second melody, which first jumps from C to F, then back through D to C and only later rises to A. But it is easy to imagine and probably to find chants that display an intermediate melodic gesture and are thus unclassifiable—unless we come up with yet another criterion.

This analysis is flawed, however, as we are looking at parts of pieces only, a problem the authors show their awareness of (p. 39). The other parts are the psalm-tones, which alternate with the antiphons in performance. Even if we were in doubt as to the mode when hearing the first iteration of the antiphon, it would become quite evident at the second hearing, after the psalm-tone had imprinted the reciting tone A or F has been imprinted on us. But if we think this through, is mode then only the result of a melodic entity being associated to another by tradition? My reply to this is essentially that of Harold Powers: even though the eight-mode system was borrowed from elsewhere (Byzantine chant), it was reasonably successful as it absorbed many features of the inherent modality of Western plainchant, which was melodic and open-ended by nature (Powers & Wiering 2001: 1, ii and 2, iii). This inherent modality, which no doubt was reshaped a lot by modal theory but certainly not replaced by it, is the medieval analogue to the tonality of more recent days.

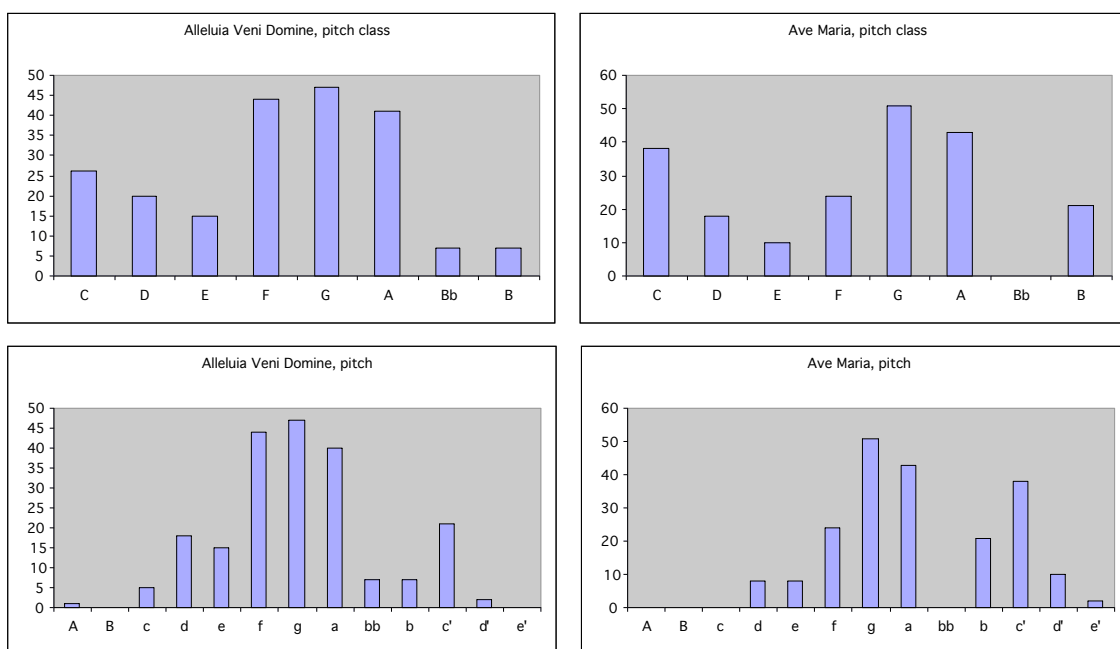
So what can we conclude from this? My criticism of Huron and Veltman so far is thus not about the examples they chose, even though I find these less than happy. To excuse the authors I must admit that it is not that common to find flawless examples of the modes, which in itself indicates how problematic modality is. My discussion serves to illustrate a more general point, namely that there exists a considerable gap between the theory of mode and the practice of plainchant, caused by the original classificatory purpose of the modes and kept open by an idealistic view of mode as a theoretical,

primarily intellectual concept that is only secondarily informed by observation of musical experience. This gap becomes visible as soon as one tries to analyze a plainchant melody or polyphonic composition in terms of its mode.

I will now sum up my opinion in a few crude statements. Modes as we know them through the accounts in theoretical treatises do not correspond to cognitive categories. Using the descriptions of mode from treatises as such categories ignores the purpose these descriptions were made for and is therefore unlikely to provide a good account of contemporary listening habits. And the labelled data from the *Liber usualis* are flawed as they are not based on perceived modality but on classification by means of arbitrary musical features.

But suppose that I'm wrong and the eight modes *were* cognitive categories, there are several issues in the article that merit critical discussion:

1. are pitch classes are the best way of constructing profiles?
2. scalar versus melodic approach to mode;
3. the significance of modal transposition.



**Fig. 2.** Pitch class and pitch profiles of Alleluia Veni Domine (mode 3) and Ave Maria (mode 8) from the mass for the 4<sup>th</sup> Sunday in Advent.

The authors derive the profiles of the modes from pitch class frequencies. One may question whether the notion of pitch class is appropriate to the repertoire, for notes an octave apart assume different role in each mode. For example, the octave of the final is never the actual final of a chant, probably never even a phrase ending, and the octave below a reciting tone has a very different relationship to the final than the reciting note. Therefore, one can imagine that it is easier to differentiate modal profiles if these are based on pitch rather than pitch class. To test this, I picked a small number of chants and determined their pitch profiles. Two examples are shown in Figure 2.

As expected, the pitch class profiles of both chants are rather similar, especially if B and B-flat counts are merged (for which a case could be made): then the most important difference is the frequency of F. The pitch profiles show an interesting additional difference, the distribution of d and d'. In my opinion, such differences should not be ignored if they have a sound theoretical basis and can be put to good practical use.

Concerning pitch-class profiles, the authors make the inevitable but sad observation that it is impossible to carry out experiments on 11<sup>th</sup>-century listeners. Yet there may be a way out. There still exists a significant community of clergy who practice plainchant on a daily basis. Even though their perception of plainchant modality may not be as pure as it was in the 11<sup>th</sup> century, it seems worthwhile to consider what could be learned from perceived modal hierarchies. Moreover part of this community is actively engaged in chant research and therefore possibly willing to contribute to a novel approach of their field. This raises the question of how to set up a suitable probe tone experiment. It is of course inconceivable to use chord progressions for priming, and also incomplete scales may not be very

helpful, for example if we wish to be able to distinguish between modes 1 and 8, which employ the same octave species. Some form of melodic priming must therefore be developed, probably based on the archetypal melodic formulae of the modes.

This brings me to another point that is already implicit in much that I have said so far, namely that the focus on pitch class only ignores the melodic nature of plainchant modality. Pitch class frequencies are secondary to melodic aspects. Let me illustrate this reflecting on an important observation by the authors, namely that in modes 3, 5, and 8 the pitch class C is predominant. The implication may be—the authors put this forward very tentatively—that C became perceived as the principal characteristic of these modes, which paved the way to their convergence into Ionian and subsequently C-major. Making a rough generalization, most plainchant is rooted in recitation, which is the translation into the pitch domain of natural intonation patterns. As a consequence, the ‘arch’ is the dominant melodic shape. Structurally, the final of such an arch carries the most weight; it is also often identical to the beginning. The most frequent note is however probably going to be one that is used in the middle, for reciting the bulk of the text. Such recitation patterns rather than pure frequencies have shaped the structural functions of pitches. Indeed, this is the cognitive notion I would derive from Johannes Cotto’s theory, namely that the prime constituent of a mode is the interval between a lower final and a higher non-final note, each of which having a distinct quality deriving from its position within the gamut. Making such notions quantifiable would be a major research challenge and in my view a better way of approaching modes as cognitive entities.

The third issue concerns modes and transpositions. In the explanation of Table 5, 5 out of 6 identifications are considered correct. However, to me it seems that only one identification is correct, namely chant 7a. In the five other cases the mode itself is correct, but the final is not, resulting in classifications such as B Hypophrygian and D Hypomixolydian. But then also cases like chant 1a (D Hypomixolydian) might be considered to be (partially) correct. Evidently, some explanation is missing here. Also, the test chants ought to be identified, so that we can check whether the classifications make musical sense.

Furthermore, I do not understand why all chromatic transpositions should be compared. Of course we can imagine F# Dorian, but for a medieval listener this expression would make no sense as (s)he would lack the concepts of both absolute pitch and the chromatic scale.

The role of the chromatic scale in tonality is to provide the general framework for pitch organization, and each chromatic pitch can theoretically be explained in each key. This framework is not universal but specific to our musical culture. The corresponding framework for medieval music is the diatonic gamut from G2 to E5, with B-flat3 and B-flat4. A culturally informed formulation of the classification task would be to predict the final and the ambitus *within the gamut* on the basis of a pitch profile. Assuming that the melody would give enough information to establish the gamut—which most melodies do—the final can be only on one of the 8 different pitch classes (of course my argument for playing it by pitch rather than pitch class would extend the number of options). For each final two modes remain, so 16 options would be more realistic. Actually, I would seriously consider going one step further and start from the ‘quality’ of each tone, which is its intervallic context, which since the 13<sup>th</sup> century has been generally expressed by means of its solmization syllable(s). Hexachord mutation would raise some interesting problem, or might perhaps even contribute to the solution.

Finally, a word about the discussion. The tentative explanation of the transition from modality to tonality is carefully worded—so careful that it is hard to disagree. Yet the view that tonality is the outcome of the predominance of A and C in modes where these are not finals, is problematic for a number of reasons. First, as I have demonstrated, this stems from a scalar view of modes which ignores melodic aspects that can provide a plausible explanation of the phenomenon. Second, mode and key have a fundamentally different purpose: modes are intellectual abstractions for classification; keys provide a perception-based explanation of the concrete structure of a composition. There was no evolution from one into the other. It may often make sense to describe a composition’s structure in terms of tonality but yet to classify it in a mode. There is music from the 17<sup>th</sup> and 18<sup>th</sup> century that is unquestionably tonal but that was classified in a mode by its composer, usually because of its liturgical place or the compositional problem it addresses. Third, this explanation ignores historical evidence, including the fundamental role of polyphony in the development of tonality. Tonality consists not so much in the use of different scales but in a very special way of connecting chords, and it is likely that the properties of those chords were instrumental in determining the scales.

In short, I believe the suggestion made in the discussion is rather far-fetched. In defence of the authors one might say they are just following usual scholarly practice, for historical musicology indeed is ridden with similar claims based on flimsy evidence. But I’d like the role of empirical musicology in relation to historical musicology to be to critically investigate such claims by confronting them with quantitative evidence and submitting them to sound reasoning, so that one can, step by step, resolve burning problems that so far have been open to guesswork only.

Modality offers many opportunities to do so, and I'm glad the authors have made the attempt. From my point of view, the next logical step would be one back, to question the modal categories themselves. After all, we do not even know for sure that people perceived eight different modes. Even for the early Renaissance, after centuries of priming with the eight-mode system, there is some evidence of the practical use of three or four modes (Judd 1992; Wiering 2001, 44,132). A large-scale quantitative analysis of the surviving plainchant, taking geographical distribution and historical change into account, may help us to come closer to the solutions than we can by employing traditional analytical tools. For this we must inspect a number of features, plausible from source evidence but also informed by what we know about the properties and limitations of human perception and cognition. It is in this respect that the article makes its most important contribution, as a manifesto for the application of such methods to music-historical questions.

## NOTES

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## REFERENCES

- Huron, D. & Veltman, J. (2006). A cognitive approach to medieval mode: Evidence for an historical antecedent to the major/minor system. *Empirical Musicology Review* Vol. 1, No. 1, pp. 35-55.
- Judd, C.C. (1992). Modal types and *ut, re, mi* tonalities: Tonal coherence in sacred polyphony from around 1500. *Journal of the American Musicological Society*, Vol. 45, pp. 428-67.
- Powers, H.S. (1982). Modal representation in polyphonic offertories. *Early Music*, Vol. 2, pp. 43-86.
- Powers, H.S. & Wiering, F. (2001). Mode: I. Medieval modal theory. In: Stanley Sadie (Ed.), *The New Grove Dictionary of Music and Musicians*, 2<sup>nd</sup> ed. New York: Grove.
- Wiering, F. (2001). *The Language of the Modes: Studies in the History of Polyphonic Modality*. New York, London: Routledge.