

[Contents](#)[References](#)[MTO Home](#)[SMT Home](#)[MTO Talk](#)

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Diatonic *ficta* revisited: Josquin's *Ave Maria* in Context

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ABSTRACT: Roger Wibberley in [MTO 2.5](#) has criticized a version I published of the sequence from Josquin's *Ave Maria*, on grounds that it flouts Glarean's modal classification. Cristle Collins Judd has already challenged Wibberley's construction of mode, and I further deny Glarean's relevance on chronological grounds. The first part of my article restates and revises some of the premises (ignored by Wibberley) which provided the context for my discussion of the Josquin piece; the second part extends my original discussion of that passage, and offers some comments and questions in response to [Wibberley's paper](#).

[References](#)

A mind is like a parachute. It only works if it is open.

[1] I am grateful to Roger Wibberley and other correspondents following his article in [MTO{1}](#) for airing some important questions and providing me with an incentive for this reply. I do plan eventually to produce a more fully revised and corrected expansion of the thesis I set out in "Diatonic *ficta*" (henceforth DF), [{2}](#) incorporating replies to Karol Berger and Peter Urquhart, but this may serve as a partial, interim statement. [{3}](#)

[2] Cristle Collins Judd began her posting of July 23 to mto-talk ("Wibberley, MTO 2.5," [mto-talk 23 July 1996](#)) by addressing not so much Wibberley's solution but his modal premises. In order to restore premises rather than symptoms to the centre of the discussion, I shall first set the Josquin aside. Since some readers may not be familiar with DF and the context in which I used that example, and since none will be able to infer it correctly from Wibberley's article, it might be helpful if I now restate and amplify some parts (only) of the thesis particularly germane to this discussion, and

take this opportunity to adjust some areas where my presentation may have proved incomplete or too elliptical.

[3] Since I invoke counterpoint so strongly, I had better explain the specific sense in which I use the term. Counterpoint, as defined in DF from Tinctoris and earlier theorists, is concerned not with lines or vague general attributes but with *two-voice* progressions--what *we* might call two-part or *dyadic harmony*. [4] At least up to the late 15th century, the handling of more than two parts was treated by theorists as an extension of those dyadic principles. It is in respecting and reconciling melodic principles and the rules of counterpoint that *ficta* is necessitated; I have tried to show that *ficta* needs to be viewed in the context of counterpoint as a whole, and not informed just by precepts specifically labelled as *ficta*. The list discussion has referred to the need to set priorities in cases of conflicting principles (Judd, 23 July: "the challenge to Bent's solution of the passage in question comes not from modal theory, but in relation to how one interprets horizontal and vertical priorities in determining *ficta*." Just so.). I did indeed attempt [5] to draw from theorists a set of primary guidelines for applying contrapuntal precepts (melodic and "harmonic") precisely with a view to setting priorities when those precepts come into conflict, though there is still a long way to go in spelling out qualifications, exceptions and licences to those primary considerations, some of which have been addressed by Berger and Urquhart.

[4] A fundamental difference between renaissance notation and ours is that, then, "not to notate accidentals is not to misnotate the music." Notated accidentals were truly accidental. No more or less importance attaches to their prescriptive power or indeed to their absence than would, say, to that of sporadic fingerings in some early keyboard sources. We may be glad of help, however occasional or eccentric, but notation should not be viewed as incomplete or inaccurate when lacking such accidental indications. When we transcribe old music into a notation in which accidentals have become essential, we tend to read the notation thus transcribed as a stronger default than it ever could have been, one from which "deviations" have to be justified and to which accidentals have to be added, or notes inflected. It is we, *not they*, who "add accidentals," depart from the notation, and make inflections. They had no term to distinguish our F flat from F: if a note was F according to the clef, it was still "F," even F fa ut, *even if* it had become our "F flat" by local contrapuntal operations. This is what I shall mean by the term "contrapuntal descent" (see [8]), as distinct from a descent caused by tuning.

[5] It is the modern transcription that has traditionally been treated as our default, as when we refer to "the notation as it stands," or at "face value," despite changing standards in editorial practice. After considerable editorial experience, it is now my conviction that so to treat it is a greater disfigurement and source of misprision than to start from the other end, as I now advocate. It is obvious that *their* starting point for these determinations, *their* access to the music, was not from a modern transcription but rather through singing from their manuscripts and prints. Early notation provided a weak intervallic default organization by clef and signature, but because it was incompletely prescriptive of pitch (hence "weak default"), the performer expected to arrive at actual sounds by some means besides prescriptive notation. Modes and hexachords (see [18] below), while very important for other purposes, run on separate tracks from each other and are at best marginally relevant to the realization of counterpoint and the determination of *ficta*. The most important key to successful realization of weakly prescriptive notation is to complement it as they must have done, armed with an approximation of the elementary training shared by composers and singers, and which composers presumed in their singers when they committed their compositions to notation, namely, for these purposes, practical training in counterpoint. Taken in partnership, notation and counterpoint create a more strongly prescriptive basis for realization. Like them, we should develop the (for us very different) musical skills that are dictated by singing from the original, acquiring an awareness of the constraints and freedoms inherent in the notation, as well as a sense of the violence done by putting weak-default (early) notation (without the complement of a strengthening counterpoint training) into a (modern) form that demands to be read by the standards of modern notation as a "strong" default. DF grew out of a recognition that the answers to many of these questions follow naturally from the experience of reading and singing from original notation instead of from conceptually different modern translations. If this is a counsel of perfection, we need at least to learn (by doing it) to *simulate* that experience so that in using modern scores we can make all allowance for their inherent distortions, as one glimpses the original language through

the shortcomings of a translation. It is those earlier habits that (echoing Crocker 1962) we need to recover, by reading their books (musical and theoretical) rather than ours, by observing what they don't say as well as what they do.

[6] Armed with the rudiments of mensural and contrapuntal skills (correct realization of perfect simultaneities and cadential approaches in discant-tenor pairs, and perfection of melodic 4ths and 5ths unless prevented), one *reads* one's own part in a state of readiness to re-interpret, of readiness to change one's *expectation* of how to read the under-prescriptive notation (not to *change the notation!*) in prompt reaction to what one *hears*. The "default" of the line you *see*, together with the melodic articulations you expect to apply (perfecting linear fourths, making cadential semitones) is controlled and sometimes over-ruled by the counterpoint you *hear*. The "default" that is "changed" is not the *notation as transcribed*, but the *expectation* of how the original notation is to be realized. [6] Once the new (i.e. old) habits of listening and adjusting aurally have been internalized, most solutions follow naturally, and almost never require the lengthy discussions that arise when singing from transcribed score. I fear that I am now as skeptical of the authority of assertions about what is and is not possible in early polyphony, from those who have not acquired fluency in reading in this way, as I would be reluctant to accept literary correction from someone who read a language only in translation. The weak default of under-prescriptive notation becomes a strong default when coupled with contrapuntal training, but it is a different strong default from modern, mostly white-note, notation "as it stands," and the inherent status accorded accidentals by current editorial conventions.

[7] We are still free to treat results so obtained as a default that can or must be departed from, but this default is as different as it could be from that of a modern transcription. We will approach their thinking and musicianship more closely by trying to do it their way (the Kon-Tiki principle of testing whether the expedition is possible using the original equipment), even if the results turn out to be very different from what we have grown used to by doing it from the opposite, unquestionably anachronistic direction, and even if we then decided (on grounds *yet to be determined*, since "modal fidelity," *pace* Wibberley, will no longer do) that the new results need further adjustments of a different kind.

[8] Our musical culture has raised the definition of frequency and pitch-class to a high status, for analysis, editing and performance. My reading of a range of early theorists leads me to posit a slightly fuzzier status both for what we would call pitch-class and for frequency, a status that places pitch closer to the more flexible view of durations and tempo that we still have. This reading rests partly on conspicuous circumlocutions and the late arrival of precise language, notation and measurement, partly on a pervasive Pythagorean mentality expressed in the tuning system, partly on my understanding of counterpoint and the internal evidence of some paradigmatic pieces, *not* the Josquin. We routinely make *rhythmic and durational* analyses on the basis of notated values even though we know that performance fluctuations, some necessary, some elective, expected but elusive to precise definition, are ignored by the analyst. We are not necessarily shocked if an analysis disregards the fact that a piece, any piece, may end slower than it began. A terminal ritardando needn't affect certain kinds of analysis; nor need the ritardando of pitch caused by a logical downward sequential spiral (Obrecht) shock us. I do not assume, as Wibberley seems to impute [2], that for the "Obrecht piece to begin on F and end on Fb was of little if any consequence for the singers." By suggesting that if they *knew* they were spiralling for reasons either of tuning or counterpoint (if I understand him correctly) they would have found a way not to do so, Wibberley subscribes to a rigid frequency stability which, however well established it became in the keyboard-reference era, did not, I believe, govern earlier music (see especially [13] below). Without cumbersome advance planning, I maintain that it is virtually impossible to sing the Obrecht *Libenter gloriabor* Kyrie (and about 30 other pieces) from original notation in any other artful way than to let the sequence, indeed, wind smoothly down in its *contrapuntal* operation (irrespective of the tuning used, even if that were equal temperament). This happened in one of our singing sessions when someone innocent of its notoriety brought a facsimile along. We read it, it descended, as everyone was (and always would have been) well aware as it was happening. The sequence of descending fifths and rising fourths F B E A D G C F is notated only with a few encouraging B and E flats, but its smooth counterpoint locks it into -- in our terms, F Bb Eb Ab Db Gb Cb Fb (see DF, 34-40). Another of us, who had previously been sceptical of "my" solution on paper, exclaimed with surprise that it

sounded fine. That is precisely the point. Try it!

[9] Nor need such a spiral impinge on, *pace* Wibberley, a modal analysis. The work by Powers and Judd on Aaron's modal assignments [\[7\]](#) makes it clear that Aaron in the *Trattato* was indeed making those assignments "on paper," in such a way as to permit two startlingly different-sounding pieces (such as *Mon mari m'a diffame* by De Orto and *E la la la* by Ninot le Petit, nos. 12 and 27 in Canti B) to receive the same classification. (How, for example, does Wibberley deal with such witness or advocacy of mode?) This is closer in time to Josquin than Glareanus and should restrain Wibberley's construction of the relevance of (his perception of Glareanus's view of) mode to the sound (= contrapuntal realization) of a piece. The relatively higher status we now accord to sounding pitch definition is reflected in the facts that we (not they) have made accidentals essential, and that we fix frequency and pitch class much more sharply than we do metronomic values. Pitch, in short, has a higher status for us than rhythm. Skilled singers *of course* would be aware of changes, both at the micro-level of tuning, shifting commas of intonation, and at the macro-level of occasional contrapuntal spiralling sequences such as I believe to be indisputable in the Willaert and unavoidable in the Obrecht examples.

[10] Berger (in *Musica ficta*) was unwilling to accept the evolution of his "renaissance" view of a keyboard-like repertory of available pitches from my free-standing, vocally-conceived, Pythagorean, pre-keyboard "medieval" view of pitch, tuning and vocal counterpoint. Indeed, he (like Wibberley) is reluctant to accept any possibility of fluctuation, by tuning or counterpoint, and there we differ. I believe that Berger's view is broadly valid for a later period and with different qualifications from those to which he applies it, and that it can at some point be reconciled with mine, though not as a background to music before 1500, where I judge it to be anachronistic. I wrote: "Musica recta is not an arsenal of fixed pitches but denotes a set of relationships to a notional norm of pitch stability that is more like a flotilla at anchor than a Procrustean bed or a pre-tuned keyboard. The 'operation of *musica ficta*', that is, the substitution at any point, for contrapuntal reasons, of a tone for a semitone (or vice versa), could mean that the absolute frequency of the As, Bs, Cs that follow may not be the same as they were before, although the local interval relationships of small segments will remain intact. The taking of a *conjuncta* (substitution of a tone for a semitone or vice versa) anywhere in the system may change the actual pitches following that point, without changing the relationships except at that point. The value of a semibreve may be changed by proportional operation or mensural change; the contextual relationships of that semibreve will continue to be observed after the point of change even if the absolute durations represented by the same symbol in the same context are different from before. Both for mensuration and for pitch, the values are achieved through local context and without reference to long-term absolutes." (DF p. 10, and *passim*.) Especially since the Powers-Judd illumination of renaissance views of mode, it has yet to be shown that there is any basis other than modern prejudice for claiming such absolutes with respect to sounding pitch, as distinct from notated status, for a musicianship that was not yet, before the 16th century, bound by keyboard-like reference. It is the notion of frequency volatility of both these kinds that has already educated the loudest howls of protest (e.g. from Berger, p. 45). This is a genuine point of disagreement, much more fundamental than the Josquin example. That singers were aware of these shifts does not prove (again, *pace* Wibberley and Berger) that they would have found them undesirable or striven to avoid them. Before about 1500, and often afterwards, there is nothing to constrain a piece to a fixed frequency or, in certain special circumstances, to a fixed constancy irrespective of tuning, for a letter-name-plus-hexachord syllable point on the gamut; these fixities are what I see as coming in with the rise of the keyboard as instrument of practical and theoretical reference. They would have been as ready, I believe (as we are not), to redefine a frequency or, as in the Willaert, to adopt a changed but logically-approached pitch for E, as to accept (as we can without special pleading) a new value for a semibreve beat after a proportional shift, whether specified or otherwise necessitated.

[11] Diatonic (and hence also chromatic) status was defined *melodically* in the 16th century and earlier. [\[8\]](#) The presence of sharps and flats does not necessarily render music chromatic; diatonic status then, as later, is not confined to "white notes." F-F# is a chromatic, F#-G a diatonic semitone, *irrespective of the size or tuning of the interval*. F#-G-Ab presents two adjacent *diatonic* semitones. This or any melodic progression that proceeds by diatonic intervals (e.g. the tenor of the Willaert duo) is diatonic. Most *ficta* is diatonic, hence "Diatonic *ficta*." I did not intend the

apparently pseudo-diapason as a label for exotic procedures; {9} rather, I used some unusual pieces to illustrate how far diatonicism can go, in order to demonstrate how some modern scholarship has misused the term in relation to early music, and to bring out the different underlying assumptions and the different prescriptive power of old and new notation respectively. I am pleased to note in the recent discussion that most correspondents avoid indiscriminately calling sharps and flats chromatic, and thus tacitly acquiesce in the view that, except for specifically chromatic intervals such as F-F#, *ficta* was largely diatonic.

[12] I have read with interest the mto-talk postings of Nicolaus Meeus on tuning and intonation ("Wibberley, MTO 2.5," [mto-talk 19 August 1996](#), [mto-talk 26 August 1996](#)). He rightly surmises that I believe some kind of just intonation (with pure 5ths and 8ves) applied to a *cappella* vocal counterpoint "with pure intonation, Pythagorean in principle [Meeus and Lindley also use this qualification], but probably with justly tempered thirds in practice" (DF p. 18). However, Meeus's view of tuning is so clearly anchored to a sophisticated keyboard-equivalent that, in setting a standard of reference for a piece or a passage, it comes at the discussion from the opposite direction from mine. As for adjustments *on* dissonances, the frequent pitch redefinitions that result from a Pythagorean approach (as I understand it; see next para.) result in no bumps, no audible local dislocations.

[13] It is highly significant that there was no standard *starting point* for tuning the Pythagorean monochord. In DF I presented Pythagorean tuning as the antithesis of keyboard reference: "even if two monochords were tuned with true Pythagorean ratios, their resulting frequencies could be slightly different if those ratios were applied from a unison by a different route through the spiral of fifths." (See DF, especially 3-7.) The monochord was unsuitable as an accompanying instrument; apart from very elementary pedagogic use, it was a representation of Pythagorean ratios rather than a proto-keyboard for an individual performance; this of course allows for the kind of disciplined frequency movement I believe to be endemic to their thinking, hence the Pythagorean spiral, not circle, of fifths. The monochord *represented* the proportions that yielded those sounds, but in practice (by pure 5/4 thirds) may have been on a slightly different track from them (separate tracks, yet again). I believe that the view of a constantly redefining Pythagorean application overcomes the rejection (by Meeus, [mto-talk 26 August](#), and implicitly by many others) of Pythagorean tuning throughout the very period where it is prevalent, and the point of theoretical reference for proportions of all kinds. Indeed, the better in tune a performance sounds in terms of its local progressions (with pure thirds and fifths), the more likely it is to move down, as several professional performers confirm. It is a short step from here to believe, as I do, that Pythagorean intonation was *constantly redefinable* around new central notes in the course of a piece. The arrival on each new true fifth sonority would then be the new point of departure for purposes of tuning calculations, thus achieving local perfection and a smooth, gradual descent by comma increments. This rules out a notional keyboard standard for performance; any performance *with* a keyboard necessitated compromises, a fact that exercised several 16th-century theorists. The final sequence of a piece like *Absalom*, in which (I believe) pure (Pythagorean) fifths would, ideally at least, have been mediated by pure 3rds (5/4) which in turn anchor the next pure fifths of the sequence, is almost bound to end at a fractionally lower frequency unless it is (artificially, and irrelevantly for this discussion) disciplined by adherence to a pre-tuned keyboard standard or repertory of available pitches (Berger's view).

[14] If you believe, with Lowinsky, that frequency must have been constant, a musical absolute (as if to be accompanied on equal-tempered or fretted instruments), then obviously pieces such as the Willaert duo and Greiter's *Fortuna* can be regarded, as he did, as precocious manifestos for equal temperament. {10} He was prepared to accept *contrapuntal* descent under certain conditions, but not frequency descent. By *contrapuntal* descent, I mean pieces like the Obrecht and Willaert examples (see DF) and the Greiter where *even with constant frequency* the "F" at the end is, by purely *contrapuntal* spiralling, one or more semitones lower than the "F" at the beginning. If you believe, with me, that (1) such logical *contrapuntal* and melodic descent through the spiral of fifths in these pieces is inherently Pythagorean in concept and (2) that Pythagorean *tuning* of 5ths in practice, with pure 3rds (5/4), was predisposed to result in comma slippage, pieces which thus descend *contrapuntally* can be construed as (in these cases, late) manifestos of Pythagorean conception and execution, as posited above. Rejection of *any* degree of frequency volatility

obviously makes it harder to overcome resistance to contrapuntal descent, despite the existence of pieces (Willaert, Greiter) where all are agreed that it *must* happen. Otherwise, the tuning consideration need not be an impediment to considering other premises of this argument (notably contrapuntal correction) independently of a specific tuning system. [\[11\]](#)

[15] Now for a confessional review of some miscalculations in DF, and a further unpacking of some ellipses. First, it was misleading to present my examples in modern score. I should never have expected readers to accept, even hypothetically, a paradigm shift that emphasizes the radical difference between old and new notation while at the same time transcribing the examples in such a way as to imply that they are equivalent, and exposing them to all the shock of unfamiliarity, and to conceptually foreign analyses of their tonality and tuning. As I put it, "neglect of some primary musical facts has led us to tolerate the aural dissonance of intolerable intervals before we accept the merely graphic dissonance of an intolerable-looking modern score." (DF p. 48.) My examples in DF should, rather, be seen as "phonetic" approximations, into a different language, of what might be sung from the original, or might be at least the first default so derived. But it is hard to know what else to do. It is unrealistic to assume that readers will have the training or time to get together to sing examples from parts, the counsel of perfection spelled out above, thus simulating the *process* by which contrapuntal training was applied to notated music, but modern scores might at least be read with that awareness.

[16] Since DF I've had the benefit of reading and discussing recent developments in work on mode, especially by Powers and Judd, and I would now reformulate some of what I said about mode in the light of that, though the basic disconnection, on which Judd and I agree, is not much affected. For the moment, I will confine myself to quoting her clear statements to the mto-talk discussion: (23 July) "there is simply no need for mode or ficta to impinge one upon the other because they occupy different conceptual and theoretical realms," and, after quoting Wibberley's "Only by this means [retaining some diminished fifths in performance] would it have been possible to remain faithful to the mode on account of the actual notes Josquin composed in the particular combination chosen by him," Judd continues: Wibberley's "conclusion is based on a modern understanding of mode as tonal system. Nowhere is such a view articulated by Glarean. Such a view fundamentally misrepresents the very nature of *musica ficta* in seeking to fix pitches in a way that Renaissance musicians clearly did not. Although Bent's solution is not one that I would adopt for this passage, there is nothing in the Glarean passage quoted by Wibberley in para. 9 to argue against it. Wibberley is imputing to Glarean an "internal" view of the modes, but nothing in Bent's solution changes the final or range (i.e. the external criteria by which the mode is recognized), hence Bent's view that her solution does not disturb "modal coherence."

[17] While I do not think that I am "mixing up" [\[12\]](#) performance and understanding, I do see that this is something that needs to be addressed, and one way in which I hope to advance the formulations of DF is to make that distinction a bit clearer. I hope I have done so by introducing the "default" element into paras [4-7] above. Having advocated even more separation of tracks than I did in DF, I see that here I have not gone far enough, and that certain rare conundrum passages like the Josquin that are capable of a perfect contrapuntal realization might in practice not receive it. Just as there might be a reading of a piece that was perfectly consonant with the traditional (Wibberley's) modal-scalar ideal, but a reading that one would not choose in practice, so there may be passages like the Josquin sequence where the perfection of the counterpoint would have been tempered in practice. This is where my discussion above of a "default" concept may be helpful. While I in no way withdraw from "my" version of the Josquin as an exercise in contrapuntal perfection, I am perfectly prepared to accept the possibility of a more cautious compromise (see below), if only to direct attention to the premises by admitting that they need not stand or fall by an "extreme" example. I chose examples that would make vivid the radically different conceptual underpinnings of old and new notation, rather than to illustrate the much subtler consequences that this understanding brings in practice for most "normal" pieces; but if we can now agree to recognise the Josquin as a special case, the way may be open for broader acceptance of what I have outlined above.

[18] In an attempt to set out the main tributaries of a proposed radical shift in *understanding* the basis of notation (not necessarily or always entailing a shift in the sounding *results*), I inevitably overstated or understated some aspects, largely by insufficiently freeing myself of some

modern prejudices. In attempting to formulate the complementary nature of notation and counterpoint, I may have overstated the weakness of the default, leading others to impute a less disciplined relativism than I actually intend. I hope that the default element helps here. I seem to have *understated* my position on the role of hexachords and solmization, and hence misled Daniel Zager and some others about how mastery of solmization relates to *ficta*.^[13] I never meant to claim (as Zager implies but does not state) that solmization can resolve counterpoint/*ficta* problems. I do not share his dependence on solmization to determine counterpoint. Rather the converse: contrapuntal decisions, once made, can be expressed in terms of solmization, the nearest they had to a precise language in which to conceptualise and name sounds. But since they stretched the system to cope with all eventualities, so that anything could be solmized by extensions and disjunctions, the criterion of easy solmization is not a valid arbiter of which sounds are or are not possible. To argue a particular solution *from* solmization is to let the tail wag the dog. I wrote: "Hexachords provide a functional context for semitone locations which have been predetermined by musical considerations, but they do not in themselves determine what the sounds will be. The hexachordal *voces* are the means by which those sounds become practically accessible in vocal polyphony, just as, by analogy, fingering is the means by which small groups of notes are physically negotiated on instruments." Hexachordal thinking permeates their terminology. It guides us away from the notion of "inflections" of individual notes and into that of small scalar segments (sometimes projected as tangents from the *scala of musica recta*) that accommodate and articulate semitones, the need for which is *pre-determined* on contrapuntal principles. It cannot in itself solve individual *ficta* problems just as, conversely, no *ficta* solution can be rejected on grounds that it can't be solmized. Nor, as Judd agrees, can modal theory solve *ficta* questions, whether Aaron's or Glarean's. What I have proposed is the beginnings of a system drawing simple rules and priorities from counterpoint theory, principles whose development, exceptions and qualifications will have to venture beyond the point where theory helps us and be derived in turn from actual composed music; but they can be projected homeopathically in the direction indicated by that theory (rather than antibiotically from our alien perspective), and fleshed out from a practice stripped of some modern varnish (such as the notion of modal fidelity as an arbiter of tonal stability, and of modern notation "as is"). Some of these precepts are strong and binding. Some are weaker and open to alternatives and competing priorities. All of them can be accepted without the obligation to choke down my Josquin example whole as a prescription for *practice*. That is negotiable.

[19] Having thus slightly rearranged the furniture to permit (I hope) constructive discussion, now to the Josquin. I should add to my list of miscalculations that I ought to have saved that example until a later time, in order not to distract attention from the premises, or at least I should have continued the argument, which I will now try to do. It *is* an exceptional puzzle, and has been so recognized by several writers, notably by Dahlhaus, with the rather different conclusion that "der Tonsatz abstrakt konzipiert ist und dass sich Josquin ueber die Unentschiedenheit, wie er zu realisieren sei, hinwegsetzte, da sie ihm gleichgueltig war." ("the composition is conceived in the abstract, and that Josquin disregarded the inconclusiveness as to how [the composition] was to be realized, because he was indifferent to [the inconclusiveness.])" Dahlhaus thus posits *the composer's indifference to the actual resulting sounds*, and argues that counterpoint thus abstractly conceived may have lacked either prior aural imagination of such sounds or, indeed, any musically acceptable realization.^[14] To this view Berger and I join in taking exception, if for different reasons.^[15] Josquin's sequence *is* a conundrum, of a fairly rare type. In the disputed measures, he gives us not just one text-book sequential "cliche" chain of fifths and sixths, but *two* superimposed contrapuntal pairs, discant and tenor, tenor and bass^[16] *both* of which, (not just the upper pair, *pace* Wibberley [13]), have claims to perfection *and* place constraints on the other. Dahlhaus gave up on the passage. Lowinsky favoured B natural in bar 48 because it fitted his sense of the piece as being *tonal* as well as (*nearly* pure) Ionian.^[17] Urquhart accepts Bb at bar 48, but avoids my version by accepting the simultaneous false relation of a B *natural* against it in the treble.^[18] Wibberley also accepts the Bb, but his compromise has a melodic augmented fourth *and* a simultaneous diminished fifth at 48-50.^[19] The conundrum is that, *pace* Dahlhaus, I have shown that Josquin wrote a passage that *is* capable of contrapuntally perfect realization, whether we like it or not, and this intervallic perfection has been acknowledged by Wibberley (n.7) and by others in the list discussion. Let us call it "a" solution but not necessarily "the" solution. In the context of DF, I was frankly more interested in its Janus-like status as a theoretical conundrum than in making a binding performance prescription. I would now prefer to call that version a contrapuntally-defined

default, a *starting-point* for negotiation and compromise. I was exploring the implications of counterpoint, not primarily fixing up a piece for performance--perhaps I did not make this clear enough (in DF n.49), but it is clearer to me now.

[20] The provocation, therefore, is not mine but Josquin's. It remains interesting and inescapable that he set up this sequential passage of two superimposed contrapuntal pairs in such a way that an intervallically flawless reading, [{20}](#) if not a perfect solution, is possible. Josquin's conceit deserves better than that we retreat from it on anachronistic grounds based on notions of white-note supremacy or modal chastity. Wibberley has attempted to make the case for doing so on modal grounds, but for reasons given by Judd, and because of the irrelevance to Josquin's personal arsenal of the later testimony of Glarean, let alone Zarlino, it will be clear that I do not think he has succeeded. We have to choose in practice whether to depart from that particular kind of perfection--indeed, whether Josquin was provoking us to do just that, rather than whether to depart from the notational translation of a modern score. That is the nub of the "default" aspect of my hypothesis. Bach chose occasionally to break "rules" about parallels or leading notes; we must be prepared for Josquin to do likewise, and to imply that we need to realise certain passages imperfectly. But we had better have a reason more firmly grounded in what we can discern of the musical practice of Josquin's contemporaries than simply disliking something that differs from what *we* have--perhaps mistakenly--grown used to. Addled by years of hearing and seeing under-inflected performances (of the notation "as it stands") with too many flat leading-notes and proto-tonal diminished fifths, our mistraigned ears are not reliable arbiters. That "we haven't tried it because we don't like it" (to quote the old Guinness advertisement) is a self-fulfilling prophecy.

[21] I am pleased to see that there has come to be acceptance of the bass Bb in bar 48 by Wibberley and Urquhart, despite other disagreements. [{21}](#) This note was ubiquitously rendered as B natural in the Josquin *Werken*, in Miller's edition of Glareanus, by Lowinsky, and in most recorded performances. Good, for that Bb seems to me the one non-negotiable point, and reflects new acceptance of a strong priority that we can perhaps all agree on. The B natural was earlier seen as a kind of first-inversion dominant seventh anticipating the leading-note that takes us back to the bright radiance of C major or, if you prefer, Glarean's likewise anachronistic hypoionian. The Bb in bar 48 *both* avoids a linear tritone *and* a simultaneous diminished 5th in a standard sequential progression, and thus claims priority on two counts. But that Bb is the thin end of a wedge; that there *can* be an ideal solution but no perfect solution makes it very difficult to define how far is too far, now that we have removed anachronistic tonal harmony, simplistic modal restraints, and, I hope, the misapplication of modern notation as defaults.

[22] Except for the bar 48 Bb, alternative compromise solutions can be entertained in performance, including the different ones of Wibberley and Urquhart (and see DF n.49). Since there is no good solution, the actual performance choice is much less interesting, because to some extent arbitrary. I have summarized the most important rules and priorities, (see above, n.5) but there are many caveats, and a much longer discussion is necessary, especially of the circumstances where diminished fifths may be permitted, and where there is some common ground between my views and those of Berger and Urquhart. I could, for example, more readily tolerate an--albeit unnecessary, and denied by a notated B flat--diminished 5th in the different context of bar 43 (with B* natural below F contracting to a third on C and E between the lower parts) before Wibberley's example begins:

[discantus]	D	C	B
[tenor]	F	E	D
[bassus]	D B*	C	G

The fifth at bar 48 on the other hand does not contract but forms part of an ongoing sequence and must, as we seem to agree, be perfect. But how do the following limbs of the sequence differ in the constraints that are placed on consonance and contrapuntal perfection? Wibberley cites Aaron in support of his claim [15] that "None of this means, however, that diminished fifths were to be completely banned from composed music; it simply means that perfect consonances did not admit them, and that where perfect consonances were *to be attained* [my emphasis] such intervals had to be eliminated." If *this* chain of fifths is not a prime, and literally text-book, candidate for "where perfect consonances were to be attained," I don't know what is. Having accepted the Bb in 48 by the

rules of consonance [4], his version presents both a linear tritone and a simultaneous diminished 5th in a standard sequential progression, and thus merely pushes further on the crisis that was avoided at bar 48. He invokes a lower status for the relationship between the upper parts, which might be acceptable when the main contrapuntal relationship was between the lower two. Indeed, diminished fifths sometimes occur either, as Wibberley puts it, between upper parts that are supported from below, or, as I would more often prefer to put it, when the primary contrapuntal cadence, the 6th to 8ve between the lower parts, has an added part above, e.g.:

[discantus]	F	E
[2nd discantus]	B	C
[tenor]	D	C

But he fails to recognise that the Josquin passage does not meet those criteria, because its unique feature is that *two* primary and non-cadential contrapuntal progressions are superimposed, and that the upper part therefore cannot be treated as subsidiary.

[23] In light of Judd's postings to mto-talk it is almost superfluous for me at this point to deny the relevance of Glarean's twelve-mode system (or for that matter Zarlino's counterpoint theory{22}) to discussion of constraints and freedoms that might have applied in Josquin's mind and his expectations of performance. Glareanus says nothing relevant to counterpoint and ficta. His 12-mode system is no more germane to how *Josquin* might have classified the *Ave Maria* than would be a roman numeral analysis to his harmony. Wibberley would be mistaken to assume that my silence on Glareanus was for any other reason (see DF 45). Judd published a modally-based analysis of the motet, not mentioned by Wibberley; {23} in principle, her and my statements can co-exist without disagreements affecting our different approaches: "there is simply no need for mode or ficta to impinge one upon the other because they occupy different conceptual and theoretical realms"("Wibberley, MTO 2.5," [mto-talk 23 July 1996](#)). {24} Judd further commented in the same posting: "Wibberley's straightforward mapping from composer to theorist (and vice versa) highlights an even more problematic issue. I find his view of theorists as "witnesses" difficult to sustain. Aron, Glarean, and Zarlino are, after all, advocates of their own agendas as well as witnesses." Then Wibberley ("Wibberley, MTO 2.5," [mto-talk 19 August 1996](#)): "The presumption underlying my article was that the Josquin motet WAS [Ionian tonality], especially since Glareanus said so." and ("Wibberley, MTO 2.5," [mto-talk 5 August 1996](#)): "What is clear to me is that Glareanus is telling us something quite definitely about the way the COMPOSER has composed the music, rather than about the way others might have performed it." Does Wibberley not distinguish between subsequent comment and classification on the one hand, and what could have been in the composer's mind, on the other? It is indeed a big leap to go from a subsequent theorist (especially Glarean) with his own axe to grind to make the assumption that because this was in his mind it must have been in Josquin's more than 50 years earlier.

[24] Finally, some further comments and questions for Wibberley. Why is it acceptable for *Absalom* to "modulate" (a modern term and concept) and, as Wibberley would have it, to "remove the harmony from its base," but not (by his standards) for the Josquin or indeed the Obrecht? By what standards does he judge such "removal" not only permissible but "very successful" while other comparable pieces are not similarly favoured? {25} Indeed, we cannot be sure exactly what Glareanus means by his "without removing the harmony from its base," {26} invoked by Wibberley against excessive fictive adjustments. Glarean's language and context *may* suggest some connection with Aaron's *distonata via* and Tinctoris's *distonatio*, neither of which easily lends itself to the construction Wibberley would wish to place on it. This is a difficult area, yet to be explained; Wibberley jumps too readily to the conclusion that it must mean removal from defaults of modern, not of renaissance, imposition.

[25] That Wibberley accepts *some* "ficta additions" (I would prefer to call them contrapuntal adjustments) is clear in his posting of 8 August ("Wibberley, MTO 2.5 [mto-talk 8 August 1996](#)). Are all of these consonant with his view of mode in pieces classified by Glareanus (or indeed Aaron), or with the way that these theorists *would have* classified them? Up to what point does he accept "inflections," and which ones, on what criteria and authority, and why no further? How does he reconcile the constraint he draws from Glareanus with explicit and incontrovertible text-book examples of ficta from the early 16th century, such as offered by Ornithoparcus and Listenius? {27} Are works classified by Glareanus to be given different treatment in order that they can conform

with Wibberley's sense of what Glarean means, irrespective of any musical characteristics that may suggest otherwise? He also accepts ([13]) that my example is consonant. He sometimes invokes the rules of consonance, though he does not make it clear where he departs from the notational-contrapuntal premises of DF--where indeed? It is on (albeit anachronistic) modal grounds that he determines that the rules of consonance may here be broken. Wibberley adopts the Bb in the bass at bar 48 "by the accepted rules of consonance," but it was not widely accepted before I spelled out those rules (see above, n.5). Wibberley and others think that my Josquin example takes the application of the rules too far for practical purposes, and I might even agree with them, but they (and I) have yet to define precisely at what point and why the "accepted rules of consonance" become unacceptable. Does Wibberley have a view on this?

[26] Wibberley [21] and n.11 is unclear about the status of the fourth. When the fourth appears in composition treated *not* as a dissonance, it is because it is not part of the primary contrapuntal pair. In this case there will be a

fifth or a third below it:
[discant] F# G
[contratenor] C# D
[tenor] A G

Another and more medieval way of explaining this would be that *each* of the upper parts formed a contrapuntal pair, cadencing on a 5th and 8ve respectively, with the lowest part, when that part is functionally the tenor at that moment. When the fourth occurs between the primary dyadic pair--that is what Tinctoris means by "in counterpoint"-- it must be treated as a dissonance, i.e. prepared and resolved. Wibberley's citation of Tinctoris's "Hence it is rejected [as a consonance] by counterpoint" (n.11) means just that. He misinterprets Tinctoris's statement as meaning generally in the musical texture, but counterpoint clearly must be understood specifically here, or it doesn't make sense, and Wibberley has to labour to do so. He confuses the issue by bringing in acoustics ([15] and n.15). Acoustic perfection is on a separate track (again!) from contrapuntal perfection. Later instructions for the behaviour of a third or fourth voice are also ancillary to the primary dyadic counterpoint.

[27] A substantial portion of Wibberley's article [paras. 14-19] is devoted to the examples in Aaron's *Aggiunta* to his *Toscanello*. I have discussed these examples and rules, [{28}](#) and invite interested readers to compare my explanations with Wibberley's for some of the features he observes. He fails to point out [14] that Aaron's discussion of partial signatures relates them to mode; see ACN p. 321. See ACN p. 324 for a discussion of Aaron's bias to the lowest voice. In para 18 Wibberley raises Orto's *Ave Maria*, used as an example in Aaron's *Aggiunta*, and says that the only reason for the diminished 5th in the preceding bar is that there is a G in the bass beneath it. Not so. The Bb above E contracts to a third, but this E is making a discant-tenor 6-8 cadence with the bass. This is one case for possible exemption for diminished fifths (see [22] above). Indeed, a fifth contracting to a third might sometimes be regarded as "exempt" when it is not part of the primary discant-tenor contrapuntal relationship, and it does not always have to have bass support. Aaron's weighing of priorities in the Agnus of Josquin's Missa *L'homme arme super voces musicales* is discussed in DF 26- 28 and ACN p. 312; he allows a mediated melodic augmented fourth (but not in a sequence) in order to avoid a simultaneous diminished fifth, *and* to concord with the cantus firmus. The melodic augmented fourth is here mediated, and is tolerated in the interests of perfecting a simultaneous fifth; Wibberley's solution achieves neither.

[28] No one these days can deny the importance of language, and the way the terms we use permeate our thought-processes and prejudices. Thus it is surely also important, for our purposes, to flag dangerous short-circuits or short cuts that may symptomise inappropriate matching of concepts and terms, so that, when we have to use modern terms, we can at least be aware that the *absence* of an early term may be eloquent [{29}](#) I pointed out at the beginning of DF that medieval theory had no single word for pitch or for rhythm, but rather *congeries* of differently-shaded words, a powerful symptom of the separate tracks on which, for example, mode, counterpoint, solmization and tuning operate. These tracks are interdependent, but not in the way we imagine when we prioritize not only pitch, but a frequency-biased notion of pitch.

[29] In conclusion:

There is still a widespread and under-supported belief that renaissance composers must have stuck largely to "white-note diatonicism" except where we are forced to believe otherwise. This has been supported from modern misprisions of mode (such as Wibberley's), now being unpicked, that were in turn introduced to counter what we now see as the excessively harmonic-tonal approaches to early music by previous generations of scholars. The unpicking of all related assumptions still has a long way to go. Recent repudiation of artificial shackles of "modal purity" (or whatever we call it) invites us to start afresh with open minds about the sound of early vocal polyphony. (The question of tuning is separable, but obviously important, since it is loaded with many of the same modern assumptions. The contrapuntal arguments are not affected by precisely what tuning system they are realized in, but can be made on their own track.) The urgent question remains: if Judd's view that "modal fidelity" poses no constraints on *ficta* prevails over Wibberley's view that it does, i.e. if it is true that "paper" modal assignments may be disconnected from realized sounds; and if my premises outlined above find even partial acceptance, are we not further overlaying modern prejudices on early music by assuming that in order to be "coherent" it must conform to *our* standards of long-range tonality (and frequency)? Some of the same questions arising from our imposition of value-laden terms have been raised by Richard Taruskin and others for "authenticity," a term of approbation which admits no alternative; I believe we must do the same for "stability" and "coherence."

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[Contents](#)

[References](#)

[MTO Home](#)

[SMT Home](#)

[MTO Talk](#)

 [Return to beginning of article](#)

References

1. Roger Wibberley, "Josquin's *Ave Maria*: Musica Ficta versus Mode," [Music Theory Online 2.5 \(1996\)](#).

[Return to text](#)

2. Margaret Bent, "Diatonic *ficta*," *Early Music History* 4 (1984), 1-48.

[Return to text](#)

3. Karol Berger, *Musica ficta: Theories of Accidental Inflections in Vocal Polyphony from Marchetto da Padova to Gioseffo Zarlino* (Cambridge: Cambridge University Press, 1987), especially 43-8; Peter Urquhart, "Canon, Partial Signatures, and 'Musica Ficta' in Works by Josquin Desprez and his Contemporaries," Ph.D. dissertation, Harvard, 1988; Urquhart, "Cross-Relations by Franco-Flemish Composers after Josquin," *Tijdschrift van de Vereniging voor Nederlandse Muziekgeschiedenis* 43 (1993), 3-41. Their paraphrases have been invaluable in showing where my formulations need to be strengthened.

[Return to text](#)

4. Richard Crocker, "Discant, Counterpoint and Harmony," *Journal of the American Musicological Society* 15 (1962), 1- 21.

[Return to text](#)

5. In DF 23-29; also in my "Accidentals, counterpoint and notation in Aaron's *Aggiunta* to the *Toscanello in Musica*," *The Journal of Musicology* 12 (1994), 306-344 (Festschrift issue for James Haar: Aspects of Musical Language and Culture in the Renaissance), henceforth ACN. See especially 324-5.

[Return to text](#)

6. See also my "Resfacta and Cantare super librum," *Journal of the American Musicological Society* 36 (1983), 371-91, and "Editing early music: the dilemma of translation," *Early Music* 22 (August 1994), 373-394.

[Return to text](#)

7. Harold Powers, "Is Mode Real? Pietro Aron, the octenary system, and polyphony," *Basler Jahrbuch für historische Musikpraxis* XVI (1992), 9-52; Cristle Collins Judd, "Reading Aron reading Petrucci," *Early Music History* 14 (1995), 121-152.

[Return to text](#)

8. James Haar, "False Relations and Chromaticism in Sixteenth-Century Music," *Journal of the American Musicological Society* 30 (1977), 391-418.

[Return to text](#)

9. As Wibberley, [2] and [10].

[Return to text](#)

10. E.E. Lowinsky, "Matthaeus Greiter's *Fortuna*: an Experiment in Chromaticism and in Musical Iconography," *Musical Quarterly* 42 (1956), 500-519, 43 (1957), 68-85.

[Return to text](#)

11. Of course, transcriptions of these or any pieces *can* be sung in equal temperament, but I disagree with Lowinsky that they *must* be; indeed their spiralling and fifth-based conception makes it most unlikely that this would have happened in a locally well-tuned vocal performance.

[Return to text](#)

12. As Berger alleges, *Musica ficta*, 46. He also rebukes me for not keeping in mind letter-plus-solmization designations, a charge directly contradicted by DF 7-12.

[Return to text](#)

13. Daniel Zager, "From the Singer's Point of View: A Case Study in Hexachordal Solmization as a Guide to Musica Recta and Musica Ficta in Fifteenth-Century Vocal Music," *Current Musicology* 43 (1987), 7-21, referred to by Wibberley. Urquhart (p. 368) invokes awkwardness of solmization against my version of the Josquin.

[Return to text](#)

14. Carl Dahlhaus, "Tonsystem und Kontrapunkt um 1500," *Jahrbuch des Staatlichen Instituts für Musikforschung preussischer Kulturbesitz* 1969, ed. D. Droysen (Berlin, 1970), 7-17, especially 15-16.

[Return to text](#)

15. Berger, *Musica Ficta*, 166-70.

[Return to text](#)

16. I give "text-book" sources for this sequence from Hothby and Aaron in DF 29-30.

[Return to text](#)

17. E.E. Lowinsky, *Tonality and Atonality in Sixteenth-Century Music* (Berkeley and Los Angeles, 1961), 20.

[Return to text](#)

18. Urquhart discusses the *Ave Maria* example on pp. 368-69 of his dissertation. His solution, couched in an often seriously misleading report of my premises, is given in his article cited in n.3, especially pp. 25-8, and also in a paper to the American Musicological Society, November 1995.
[Return to text](#)

19. Wibberley states [13]: "Since it is impossible to render the Josquin passage in any way other than that proposed by Bent without failing to eliminate all diminished fifths otherwise occurring between notes of the upper voices (such an elimination being her prime motive)." I invite him to re-read the way I set up the Josquin example. If any fifths, by Wibberley's criteria, are to be regarded as "where perfect consonances were to be attained," they are surely these. See [22] below on diminished fifths, and [27] on Aaron.
[Return to text](#)

20. Before someone raises it on the mto-talk discussion list, I explain in DF (29-34) why the altus part, for internal and diagnosable reasons, takes a low priority in this passage, and why the sequence, as "pure" counterpoint, can be contemplated separately. But the altus may yet be the best way into arguments as to how one might depart from a contrapuntally pure default.
[Return to text](#)

21. See DF n.48, where I comment on the source status of the bass B flat. Wibberley however places it in parentheses in "my" version, while stating: "By the accepted rules of consonance, the bass b must be flattened to b-flat in order to provide a perfect consonance with the tenor" (Wibberley [4]).
[Return to text](#)

22. Wibberley [29] "Josquin would seem, in the example under consideration, to have arrived at Zarlino's "impasse," but Bent has not followed Zarlino's advice in finding a suitable way around it." Why should I have taken the advice of a theorist 100 years later whose theoretical world, including his use of terms like diatonic, is entirely different from Josquin's?
[Return to text](#)

23. "Some Problems of Pre-Baroque Analysis: an Examination of Josquin's *Ave Maria... Virgo Serena*," *Music Analysis* 4.3 (1985), 201-239.
[Return to text](#)

24. Why, incidentally, does Wibberley so dislike Zager's term "modal purity" and how does it differ from his own "modal fidelity" [13, 26]? He complains "If, by "modal purity," [Zager] has in mind a succession of notes and harmonies that arise only from the pure diatonic notes of a particular scale," but in his own n.8 Wibberley refers to "the use of notes outside the diatonic notes of that mode."
[Return to text](#)

25. Wibberley, [mto-talk 5 August](#). His reference to this as a "powerful rhetorical device" suggests that he might be following Lowinsky in demanding extra-musical reasons for what they both call "modulations," a position that can lead to great inconsistencies of treatment between musically similar constructions. Consider the arcane lengths to which Lowinsky went to defend his Secret Chromatic sheep against the musically similar goats who did not qualify by virtue of their texts. See also DF n.47. (NB the *Absalom* "modulation" is not *dependent* on notated accidentals and would have to occur, even without them, as in the Willaert and Obrecht pieces.)
[Return to text](#)

26. I plead innocent to the mind-boggling charges packed into sentences such as Wibberley's [10]: "The whole point of Margaret Bent's solution is that the harmony is, via the "necessary" application of diatonic ficta, "removed from its base." And I don't know what Wibberley means by claiming [7] that I see "modal coherence as a close relative of pitch stability," citing DF 45-47, where I wrote: "Modal theory does deal with some kind of long-term tonal coherence, but not necessarily such as can be equated with pitch stability--another distinction that has lost its force for us." There are

numerous examples in Wibberley of discourteously careless reporting, not only of my alleged views but also Bonnie Blackburn's, astoundingly misrepresented.

[Return to text](#)

27. These and others are cited in E. E. Lowinsky, "Secret Chromatic Art Re-examined," *Perspectives in Musicology* ed. B. Brook et al. (New York, 1972), 91-135.

[Return to text](#)

28. DF 19, ACN (see n.5); for Aaron see also Judd's article in n.7.

[Return to text](#)

29. In view of his reproaches to me for anachronism, I'm surprised to see Wibberley use terms such as root (n.16), tonicization (n.7) and modulation [10].

[Return to text](#)



[Return to Beginning](#)

[Contents](#)

[References](#)

[MTO Home](#)

[SMT Home](#)

[MTO Talk](#)

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