

THEORY FORUM:

REGARDING “A PROPOS METER AND RHYTHM IN THE ARS ANTIQUA”¹

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Hans Tischler very generously rates my ideas in the paper² that he has reviewed as “basically sound,” notwithstanding the fault that he finds with the details of my argument. But I fear that he has taken in only one aspect of what I tried to show in that paper, the idea that “metric accent is an essential feature of the music of [the School of Notre Dame],” as he puts it. I did indeed make such a claim (and there is nothing particularly original in it), but as only part of the description of a total music-historical situation that entails aspects of style, notation and transmission, and the explanations of musical theorists. My aim was to look at the several interactive facets of that situation all at once and to try to show something of its significance for a very large-scale historical movement in our tradition.

A reply is called for in order to place the historical theme back in the center, not to carry on the dispute over transcriptions. For it is transcriptions that are once again at issue in Tischler’s review. Yes, one should make them as good as one can. But as Tischler himself recognizes, the differences over transcriptions hardly affect the view that one takes of the “central issues.” Yet these disagreements have continually obscured the view toward any issues beyond themselves in this subject.

To measure the historical significance of the establishment of what

we know as “modal rhythm” we must see in it at least these achievements:

- Conceiving and explaining rhythm as the succession of durations.
- Regulating duration and succession as compositional factors and coordinating them with sonority (consonance-dissonance), and formulating precepts regarding such regulation. Conversely, conceiving sonority, which had been from the beginning the foundation of the organum concept and practice, as a function of time.
- Devising and explaining a notation to signify durations and their succession.

These achievements need to be understood in relation to other aspects of the musical situation: performance practice, habits and traditions of transmission, the always-evolving role of notation in performance and transmission, and other aspects of style, especially melodic style and ideas of form. Much of my paper was given over to the discussion of those relationships.

I claim at the outset that the succession of durations came to be apprehended in patterns depending on two overlapping factors: (1) the orientation of groups of durations with respect to accent, and (2) the background division of time in equal intervals (what Franco of Cologne called “perfections”). This corresponds to what we nowadays identify as the relationship of rhythm and meter.

That the temporal dimension of music was brought under control in this way, in the practice and in the explanations of the theory, is in my view to be associated with the increasing assumption of responsibility for that dimension by composers, whereas it had earlier been the exclusive domain of performers. Indeed that shift of responsibility marks an essential step in the separation of the acts of composing and performing, and the separation of the roles of composer and performer. Their roles are differentiated with respect to their different relationships to the finished polyphonic composition, an entity that we can identify for the first time in these circumstances. The possibility of such a separation depended on a notational system that functioned as medium of transmission from composer to performer, and the more explicit the notational system became, the sharper the separation could be. Therein lies one of the larger historical significances of the system of modal rhythm and its notation. That there was some sort of consciousness about these matters at the time is clear from the demands for greater notational explicitness in the theory and from the actuality of increasing explicitness in the practical sources.

The theoretical explanations of the rhythmic system and its notation have their own historical matrix, which we are obliged to take into account when we try to use them as guides to the interpretation of the music. The theorists were at pains to give expression to their own ideas

about what the practice should be (for example, the push for a more explicit notation). But their main task was to *explain*, and in that they were constrained by the traditions and conventions of their theoretical genres. To mention only two of the most important factors, their treatises are informed by a tradition of modeling explanations about music on explanations about language, a tradition that descended from the very beginning of medieval Western music theory in the ninth century; and they are informed by Aristotelian principles of reasoning that had only just been adopted in Paris during the period under study. The first of these factors provides the background for the quantitative rhythmic theory that was presented by John of Garland and his followers, in which the basic principle is the laying out of durations of one and two units in fixed patterns, by analogy with the short and long syllables of language and without any conception of musical accent or metrical grouping. The second factor is responsible for the almost mechanical way in which principles and definitions are applied in the explanations of the system, with results that can sometimes seem to us obscure, counterintuitive, or contrary to the practice as we see it in the sources.

The theorists owed as much allegiance to such constraints in working out their explanations, as they did to some criterion of verisimilitude vis-a-vis the practice, such as we might think would be a primary obligation in the description of musical practices. We are obliged to look for the coherence in what they say, in the light of *their* interests and the conventions of *their* theoretical genres and *their* reasoning style, as well as we are able to understand those. Having done that we can try to read them as guides to the understanding of the practices that are their subjects. But that cannot set the limits in our efforts toward understanding. We are entitled, even obliged, to evaluate and compare their explanations in the light of our independent understanding of the practices, gained through whatever analytical concepts are of heuristic value to us.

Tischler provides an example: John of Garland's rule that in organum "everything that meets with another according to the virtue of consonance is to be long." Tischler says what we all know, that "if followed, this rule would destroy all modal rhythm and would make nonsense of all organal sections" (for example, by distorting obvious pitch-patterings, such as sequences). Tischler's strange explanation is that the rule is meant to apply only to the first mode, for John (and other authors) "always take the first mode as their paradigm." But the rule will not produce good first-mode patterns either. The fact is that consonance and dissonance produced as an organal voice moves against a sustained tone do not alternate consistently enough in accordance with any modal pattern to allow us to use them as reliable indicators of duration. John's rule makes concrete a first principle, that organum should be made on the basis of a prevailing consonant sonority (and if that is to be the case, it stands to reason that it is better in the main to have the

long tones be consonant than the short ones—or to put it more aptly, to sing the consonant tones long and the dissonant ones short).

There is, to be sure, a certain *a priori*, non-empirical character to all this. And the effect is compounded by the fact that John's theoretical matrix did not offer him any way of taking account of pitch-patterning as it interacts in the music with duration and consonance, something that we can and must show in our analyses. But it is Tischler who strays off the beam in his discussion of this matter, and in doing so he indirectly draws us to the recognition that John's explanations are, after all, quite consistent and logical.

Tischler illustrates his argument with a passage of *discant* from the Florence manuscript, whereas the rule in question is given in John's thirteenth chapter, which is all about *organum*. In fact the rule comes just after John has instructed that in "organum as such" the duration patterns are "contingent" on "concordance," and that they are not expected to reflect a "proper mode." For *discant* John has an altogether different rule, which is aimed at producing durational patterns in a "proper mode." Tischler's caveat that "we must take with a grain of salt any theorist's rules, for such rules do not consider exceptions" is simplistic and cavalier. Our obligation is to seek the sense in exactly what such writers said and did not say, and then to evaluate that in the light of both their needs and standards and ours. Their treatises are more than flawed rule-books for our transcriptions.

I have been doing no more here than expressing an attitude about the critical reading of historical sources that is widely held in historical disciplines of all sorts and that descends through a strong tradition of nineteenth and twentieth century historiography from ideas formulated first by Vico and Herder.³ The historian engages his sources on their grounds and on his own, and also on the grounds of those who have engaged them before him. Tischler's attack on my analysis of the organum *Judea et Iherusalem* as "quasi-" and "pseudo-Schenkerian" and "anachronistic" rests on a fundamentally absurd proposition, that in the analysis of historical sources one is allowed only those concepts that could have been available to persons active in the tradition to which those sources belonged. My analysis was meant to show something of the close interdependence of notational configuration and rhythm, melodic contour, voice-leading, and sonority that I believe is characteristic and new for the style in the phase represented by that piece. It hardly competes with conceptions of that time, which were not addressed to anything like our descriptive, historical task.

Still, the struggle to achieve a workable clarification of the relation among consonance, duration, and succession (or temporal placement) is apparent in contradictions and disagreements within and between the treatises, as I claim. Franco's accentual theory was able to deal with this central theoretical problem more successfully than was John's quantitative theory. And it was able to do so principally through Franco's formulation of the precept that the first sonority of a perfection should

be consonant, whatever its duration—that is, that dissonance is normally to be resolved on the downbeat. But of course that principle is one of the basic premises of modern tonal analysis. So here is an instance in which an idea of the past can yet be made out on the horizon of the present, as Hans-Georg Gadamer would put it.⁴ Talk of “anachronism” reflects a flawed idea of historical understanding.

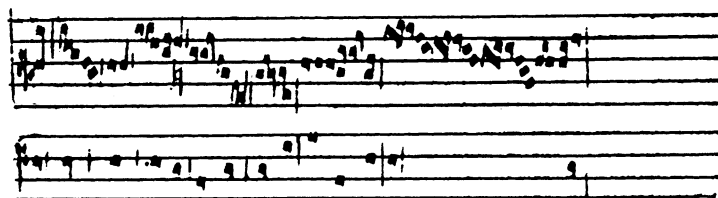
If it is claimed (the implication lies not far below the surface of Tischler’s text) that Franco’s treatise speaks to a later style and is not relevant to the Notre Dame school, then that depends on a smoothing out of complex historical realities that exceeds our prerogatives. Franco wrote earlier than Anonymous IV, who held fast to the basic principles of John of Garland, and he wrote while the transmission of the Notre Dame repertory was still very much alive. It was among my purposes to show that the development in the theory must be understood as a dialectic, not as a linear unfolding.

Sometimes Tischler seems almost to share my attitude about the reading of historical sources. So, for example, he says that John’s silence about accent does not bind us to the belief that the music is not accentual. I agree with him about that, of course, but not about the reason for John’s silence. Tischler’s explanation is that accentuation would have been taken for granted, and did not require special mention. In support of that claim he cites a universal law that transcends history: “It is a psychological necessity to organize our environment in patterns . . . and patterns are recognized by their recurrence, which inevitably carries with it emphasis.” Even if the meaning and status of this law were clear (I wonder especially about the second and third clauses) there would be serious problems about its use as a guide to the reading of medieval theoretical treatises. For one thing, it seems to be of dubious relevance to an evaluation of what John did or did not say, for surely the *perception* of rhythmic patterns is not any part of his subject. This question about accents, once again, is not whether one sang, or heard, *boom-two-three boom-two-three*, but whether the music was organized with respect to regularly recurring points of structural emphasis. For a second, it is not my impression that it would have been thought good form by theorists of John’s schooling to skip over the statement of precepts that were regarded as self-evident—especially if they concerned something as fundamental as the role of accent in rhythmic organization. And for a third, John’s theory is fundamentally opposed to an accentual theory. He cannot have presupposed such a theory. John’s silence on the matter of accent, as on the matter of perfection, is consistent and meaningful. Those concepts had no place in his theory of rhythm. It was, again, one of my main purposes to indicate something of the dialectic that the quantitative and accentual theories played out during the whole period.

Such integration of pitch and durational components as is exemplified

by pieces like *Judea et Iherusalem* belongs to a conception of music as concentrated, organized according to an overall plan, and fixed. It is presumably with such music in mind that practitioners and theorists pushed for the explicit signification of rhythm in notation. In doing so they were promoting a prescriptive role for notation, in contrast to the earlier role of notation in giving the performer a point of departure for working out a performance. One sees earlier a similar push toward a prescriptive pitch notation for plain-chant, in the writings of Hucbald (ninth century), Odo (tenth century), and Guido (eleventh century). There it was presumably not the complexity of individual musical items as much as the immensity of the repertory and the decline of the tradition of oral composition that created the pressure. The parallel may be coincidental in respect to the causes of the two developments. But both contributed to a single higher-level historical phenomenon, the increasing scriptuality of the European musical culture.

It is characteristic that such designed compositions, set down in an explicit notation, are found in a stable written transmission, as I showed in the case of *Judea et Iherusalem*. The stability is in both the pitch component and the ligature configurations, and that is in sharp contrast to the transmission of the organum *Alleluia, Pascha nostrum* (Example 4 in my paper). In both cases changes of notation from one manuscript to another are usually understandable in the sense of a greater clarification of a rhythmic configuration. What is instructive is that the changes are more thoroughgoing in the case of the *Alleluia*. The interpretation is that it is an earlier piece, that the W2 scribe applied the same notational standard across the board, and that the notational configurations in the later *Judea et Iherusalem* were sharper and more settled in the first place. There is no need to be shaken from this understanding by Tischler's *a priori* declaration that "the notation [in W2] is non-mensural and stemmed and unstemmed notes do not carry differential value." This is wrong in both principle and fact. In principle, when trying to decode any sort of unfamiliar sign-system we must of course begin with the presumption that any differential use of signs differentiates *something*, and try to discover what that is. In fact, a short phrase later on in the W2 version of the same piece shows clearly enough how stemmed and unstemmed notes are differentiated:



A 7 Iheru sa Ieru

a straightforward first-mode pattern, in which the first three notes (*longa-brevis-longa*) are written as they are because the pitch-repetition blocks a ternary ligature. The notation of W2 is more explicit than that of the other main sources in this and other respects as well—in the width of notes, and in the features of “propriety” and “perfection,” as those are described by John of Garland. Another underlying problem here, of course, is in the rigidity of Tischler’s categories “mensural” and “non-mensural.” Modal notation is mensural to begin with; it represents music that is measured according to a system of proportional durations—*mensurabili musica*, as John of Garland has it in the title of his treatise. The question is only how these notations represent mensuration, and there are not absolute boundaries.

This change in the role of notation and the stability of transmission gives us another view onto the large-scale historical movement in which this later phase of the Notre Dame style constitutes so prominent a marker.

The greater part of Tischler’s review is given over to problems of transcription, and it is hard to escape the impression that this is where he believes the real issues lie. Yet he writes “one of the most important points regarding the transcription of Notre Dame music is . . . that great flexibility is necessary. . . . Several solutions for a particular passage are often possible and equally ‘correct.’” The worst of it is that he never suggests how some larger question of interpretation might be affected by a difference over a transcription. The quarrel over transcriptions and the insistence on having it one’s own way is the beginning and the end, it seems. No other subject in our field has been so badly hobbled by this attitude. Just how interesting is it, after all, that in Tischler’s Example 3 the diplomatic facsimiles from F and W2 do not correspond to the manuscripts and that the transcriptions do not correspond to the facsimiles in even so straightforward a matter as pitch? It has a certain interest in the light of the posture that Professor Tischler assumes at the beginning of his review—the referee, ready to “set things straight.” But has it any scholarly interest?

NOTES

1. Hans Tischler, *Journal of Music Theory* 26 (1982): 313–329.
2. “Regarding Meter and Rhythm in the *Ars Antiqua*,” *The Musical Quarterly* 65 (1979): 524–558.
3. Compare my essays “History, Criticism, and Beethoven’s 9th Symphony,” *19th Century Music* 3 (1980): 193–210; and “To Worship that Celestial Sound: Motives for Analysis,” *Journal of Musicology* 1 (1982): 153–170.

4. Compare *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik* (Tübingen, 1960), p. 261; also *Philosophical Hermeneutics*, trans. and ed., David E. Linge. (Berkeley, 1976), "Editor's Introduction," pp. xvi-xvii.

