COMMUNICATIONS

To the Editor of the JOURNAL:

In his imposing paper "Charlemagne's Archetype of Gregorian Chant" Kenneth Levy has addressed the very big question about the beginning of music writing in the Western tradition (this JOURNAL 40 [1987]: 1–30). I would like to offer your readers a commentary about Levy's conclusion, but even more about the nature of his argument and the principles on which it rests, for these raise issues that are profounder and broader than the conclusion itself.

First to the conclusion: "The 'Gregorian' repertory of Mass propers was fully neumed under Charlemagne [ca. 800 is Levy's approximate date], a century sooner than is generally supposed" (p. 29). As I am among those who have supposed the later date I must briefly say why. It comes down to two sorts of reasons. (The evidence and reasoning are presented in my paper "Reading and Singing: On the Genesis of Occidental Music Writing," in *Early Music History* 4 [1984], a paper that Levy cites, but without taking any account of its contents one way or the other.) First, it looks to me as if the early music-writing technology itself is so far dependent, in semiotic principle and paleographic form, on the Carolingian system of punctuation, that the institution of the latter in centers of Carolingian writing toward the end of the 8th century would mark some sort of *terminus ante quem non* for the invention of the neumes. And it also seems to me that the Mass proper chants, executed by professional cantors and choirs, were probably neumated in a time later than the writing down of the tones given to priests and deacons. To put it simply, if, as I believe, the neumes have been adapted from the system of punctuation that we know from about 780, and if the chants for the cantor and choir were not written down immediately upon the invention of the notational system, it is hard to see how a fully neumated Mass proper would have been in circulation as early as 800. Still, it is clear that Charlemagne and company were pressing very hard to get things of all kinds written down, and it may well be that after the rapid development of the punctuation system and the Carolingian Minuscule hands attention fell to the music-writing system, which, too, could have been accomplished in short order.

The second sort of reason for my supposition of the later date is a whole series of indications that the performance of the Mass propers during the 9th century was based on an oral tradition, and that these indications are consistent with the fact that neumated Mass propers have not survived from that century. But there would be nothing so strange about an oral performance practice that continued after people began to write things down—indeed that is the most likely case. Oral and written transmission are not the two poles of a binary opposition, as Levy seems to believe. From the point of view of what I find historically most interesting in all of this—the evolution of a literate musical culture in the Middle Ages—Levy's earlier date would not be so very unsettling, even if it does make some of the evidence to which I have referred look strange. Perhaps we could agree to split the difference by adopting Michel Huglo's suggestion of a terminal date of mid-ninth century (Levy, p. 8).

Now Levy writes as though the case for the later date had rested mainly on the lack of surviving sources from an earlier time, and he charges its proponents with failing to consider "the probabilities of survival" (pp. 4–7). There are two things to be said about this charge: (1) it isn't so, and (2) it's quite strange that Levy would make it, considering the heavy dependence of his reasoning on the assumption that the pattern of surviving sources is decisive.

One section (VI) of my paper of 1984 is addressed to the question whether we should presume that the earliest notational specimens are later survivals and that substantially earlier specimens have all been lost. I cited a number of "positive, direct reasons for accepting that the earliest specimens of music-writing represent more-or-less the beginning of the practice, and they reinforce the negative evidence of the absence of earlier sources" (p. 195). There is similar reasoning in my paper "Oral, Written, and Literate Process in the Transmission of Medieval Music" (*Speculum* 56 [1981]: 474–75), a paper which Levy also cites without addressing its contents. In 1980 Hucke made clear that the surviving sources define the historical task in a way that does not depend on the "probabilities of survival." "Even if one wishes, despite this evidence [the evidence of a performance practice in the ninth century that did not depend on notated books], to suppose that there were in certain localities chant books with neumes as early as the ninth century, chant books without neumes were written at least until the tenth century. We must be able to explain the beginning of chant transmission in the Frankish Empire without assuming the use of neumes. . . . the propagation of Gregorian chant in the Empire and the distribution of chant books with neumes are not the same phenomenon: they represent two different stages in the spread of the chant." ("Toward a New Historical View of Gregorian Chant," this JOURNAL 33 [1980]: 437–67; Levy cites this paper, too, but again without taking any account of what is in it).

It is in fact Levy's reasoning that depends on the presumption that the surviving sources are representative in the most precise way. In most cases the thought of a single lost deviant source collapses the argument. Two kinds of reasoning are affected:

(1) A terminal date for the transmission of a tradition from one place to another is posited on the basis of the absence of an item from the surviving sources of either the place of origin or the place of destination. This is the argument of Levy's fourth "index," regarding the *missa graeca*, compiled 797–814 according to Levy (pp.8–9; the latest reckoning of Charles Atkinson in a contribution to the forthcoming *Festschrift* for Helmut Hucke gives the date 827–835). The *missa graeca* is nearly everywhere, but not in Benevento. So Levy concludes that the Gregorian tradition was transmitted to Benevento before its compilation. But the oldest surviving notated sources from Benevento are dated to the mid-11th century. Levy's "index" depends

on the assumption that not a single Beneventan source containing the *missa graeca* and written during the intervening two centuries has been lost.

This sort of reasoning is entailed also in the seventh "index," regarding the offertory *Factus est repente* (pp. 11 ff.). The text of this chant appears ("hanging on," he writes, "barely surviving") in only one of the six early sources of Hesbert's *Antiphonale missarum sextuplex*, and is thus presumed to have been "obsolescent" in the North by ca. 800. And as it appears in several Beneventan sources it is presumed to have been transmitted there by 800—since it was no longer in use in the North after that time. This part of the argument depends, again, on the presumption that not a single northern source containing *Factus* and written between ca. 800 and the mid-eleventh century has been lost. (In fact Levy has turned up one such source, the Prüm gradual-troper, written ca. 1000. [He writes "in recent inventories it has been curiously slighted," thereby slighting the most important recent inventory, the *Corpus troporum*, where it is duly reported.] But he introduces this source only after "establishing" a terminus of ca. 800 for the transmission of *Factus* to Benevento, using it only for his further arguments that it was transmitted as part of a fully neumed gradual, but not allowing it to re-enter as counter-example for his reasoning for the terminus of ca. 800. I remain mystified by this strategy. Nor does it allay the mystery that Levy characterizes the Prüm entry of *Factus* as a "musical archaism;" that just closes the circle on the reasoning.)

(2) The one principle that is invoked most often in Levy's paper is the law that detailed agreement among the written sources of a text points to a common textual archetype. I count seven invocations of this law, all quite automatic and unquestioning, and all but the first referring to claimed agreement of neumation as a sign of a neumated archetype. I shall not comment at all on the intangible generality of such a formulation as "the neumatic details of *the Beneventan readings agree with those of northern Europe*" (p. 8, my italics; this is said in reference to the whole Gregorian repertory), and I shall put on the shelf for a moment the question whether this law can have the same force in the domain of musical texts as it can in the domain of language texts, from which it has been unquestioningly taken over. For now I simply want to call attention to the fact that each such claim depends first of all on the assumption that deviant sources have not been lost.

Having made this observation with respect to both types of argument, I want to make clear that I don't blame Levy for operating on the—always provisional—hypothesis that the patterns displayed by surviving sources represent the original situation. The alternative would be paralysis by a sort of demon theory that nothing is really as it appears.

Now I would like to turn to some of the issues that I think are of broader importance, and first of all to take off the shelf the law connecting parallel neumations with the inference of a common archetype. This law has implications for the way that we think about how a musical text is generated, and it is those implications that I think need questioning. For Levy this law is an a priori idea in both of its aspects—that is, in the model of transmission according to which all sources are ultimately the progeny of an archetype, and in the idea that parallel neumation points to the archetype. It can have the force of law only if there are not plausible alternatives. I want to suggest that there *is* a plausible alternative—an alternative that opens onto a different, and I would like to say richer, way of thinking about what a musical text is in relation to what it denotes, and about the *various* ways in which musical texts might have been generated.

I shall approach the question by way of an imagined parallel in the transmission of a language text. Let us imagine a number of sources for an early medieval Latin text, all identical in lexical content, and all written out in a manner that was called *per cola et commata* (phrases comprising sense units are written in separate lines, as a way of guiding the reading-out of the text, exactly in parallel with the purpose of punctuation at the time). And let us imagine that the lineation in all sources is more or less identical. That can be explained by the inference of a common archetype for all sources, but it can also be explained on the hypothesis that the scribes understood the sense of the contents in the same way and translated their understanding into lineations that are more or less identical. And, last-not-least, it can be explained by any number of combinations of these two models. Given the range of possibilities, we would not be entitled automatically to infer an archetypical lineation and take that inference as fact, on which we would then build chronological structures.

The neumation of a melody has this in common with the lineation of a language text: both correspond to immanent properties of the thing denoted that would be projected in a performance of it and of which the scribe or notator *might* take cognizance while writing, and by the same token two scribes or notators might take cognizance of those properties independently of one another. For the language text that property is the sense-grouping of the words. For the melody text that property is the sequence of short figures through which the melody flows, the little turns and directional units in which the melody recognizably articulates (*Floskeln* is the German word, *neumae* has something of that idea in the Latin). Neumatic writing is a graphic transcription of that articulation—indeed in its earliest history it is far better understood in that sense than as a denotation of a pitch sequence.

I say that the scribe or notator *might* take cognizance of such properties in writing, and that is also to say that he might not, that he might copy mechanically. To me it is both intuitively and evidentially clear that writing down, especially in the early times that we are talking about here, can have been a mix of copying and putting down what was in the scribe's head. Levy's law premises a model in which there was *one* transcription from the oral-aural state, and from then on it was transmission by copying through closed written channels. That is an a priori idea, and I do not see that we are bound to assume it for all musical genres and circumstances and times.

That a melody with well-defined contours would be given parallel neumations by different notators can be understood without requiring the inference of a single textual archetype. I can think of no better example than Levy's Figures 4 and 5. If your readers will return to those I think they will see that the neumation projects the constituent units of the melody, and that a widely variant neumation would be highly unlikely, given the way that this notational system was adapted to its function. (Karlheinz Schlager did once give persuasive evidence of the stable reflection of the articulation of an offertory melisma through its neumation and the adaptation of several different prosula texts to it. But there is nothing surprising about that. [See "Die Neumenschrift im Lichte der Melismentextierung," *Archiv für*

Musikwissenschaft 38 (1981): 300–16.]) All this is no more than to sound a caveat about Levy's claim that "there is no good way to explain the exact correspondence in Prüm and Benevento" other than by way of a notated archetype.

In addition to this matter of parallel grouping of notes, Levy singles out the parallel use of the *quilisma* and *oriscus* in both versions of the melody shown in his Figures 4 and 5 as a particularly strong indication for a notated archetype, but I think that it is a particularly strong indication for the possibility of independent neumation. We have ninth-century characterizations (from Aurelian and Hucbald) of the performative denotation of the *quilisma*: a figure sung "with tremulous voice." If that performative aspect was a constituent of the melody as it was known, then *of course* it would have been written with the figure that the notational system provided for denoting that aspect, whether by way of copying or not. (There is discussion of this in my paper of 1984.) As for the *oriscus*, its use in the denotation of a cadential figure like the one in Levy's Figure 5 is rule-bound: for a note preceded by the same pitch and followed by a descent on the next syllable. A notator who knew the melody and knew the rule would write *oriscus* for that melodic situation. It is strange to claim that its use in the same position in parallel transmissions of the same melodic passage can be explained only by the inference of a written archetype. Levy writes as though neumes were mute forms that had no relation to the sense or the qualities of what they denote, and that were put down only in acts of mechanical copying, not in reflective acts. There are two conclusions: (1) Since there is a plausible alternative explanation for parallel neumations to Levy's law of archetypes, that law cannot be automatically invoked, and every one of Levy's "indices" that depends on it is questionable for that reason alone. (2) It follows that when there is talk of the transmission of a melody from one tradition to another during the period from which we have no written sources, we have to allow for the possibility that the transmission was oral and that there was an independent neumation in the host tradition afterwards. That the neumations in the two traditions are parallel does not gainsay that possibility. It is even possible that an initial neumation in Benevento, now lost, approximated the tradition of the Prüm version less closely, and that the surviving written tradition of Benevento is based on a northern exemplar that was introduced in some later time. It is just because of the low probabilities of manuscript survival that we are all whistling in the dark here, and no one is in a position to claim that he has got the tune exactly right.

Levy's attitude toward neumes as inert paleographic items informs the reasoning behind the "first index," which he identifies as a "matter of paleographic common sense" (p. 7). What it is that is so characterized is the conclusion that "a common neumatic archetype lies behind the diverse regional manifestations" of neumatic writing that are already apparent in the earliest surviving notated Gregorian propers ca. 900, and that "a period of development lay between the neumed archetype and its first preserved descendents. Allowing for paleographic change, one should suppose at least an intervening half-century, and perhaps much longer." What is it that is actually meant with the phrase "paleographic change" that would have required at least fifty years? Levy doesn't say, but I think we need to try to

Editor's Note: due to a printer's error, page 571 of the Fall 1988 issue was printed without the necessary hand-drawn examples in the first complete paragraph. This is the correct version of page 571, to be inserted in the Fall 1988 issue.

imagine it, not just let it slip by as an a priori idea. Again, I shall concretize the question with a single example.

There has been some idea in the literature that the prototypical neumatic script may have been the one called "Paleofrankish" (e.g. Jacques Handschin, "Eine alte Neumenschrift," *Acta musicologica* 25 [1953]). Without necessarily endorsing that hypothesis, let us consider a character in that script, the *porrectus*: a. V And let us compare it to its counterpart in another early script, that of St. Gall: b. N

The difference here is representative of the "diverse regional manifestations" of neumatic writing in its earliest known forms, and so it can serve us in the effort to imagine what can be entailed in "paleographic change" or "development." Handschin saw how fundamental this difference is in that he saw it as a difference in the *way* that the respective scripts represent melodic figures, not just in their forms. He characterized the Paleofrankish script as a "*Tonortschrift*" because, as in figure a. above, the sign traces a movement from one position to another in a space that corresponds to the tone space. The *porrectus* of the St. Gall script, by contrast, is based on the opposition of *virga* and *punctum* as symbols of higher and lower notes, respectively. It is in effect a compound of *virga-punctum-virga*. (Handschin did not put it in quite this way. But he did see that any understanding of the historical relationships among different neumatic script types presupposes an understanding of the principles on which they function in representing melody. See my paper "The Early History of Music Writing in the West," this JOURNAL 35 [1982]: 263.)

For our purposes here it doesn't matter which of these forms is the earlier one. If we want to believe that they both derive from a script archetype, we have to be able to imagine a change from one to the other, and that means not just in the sense of a change of forms, but a change from one idea about how neumes represent to the other. Now I cannot escape the impression from the way Levy writes that he has in mind an evolutionary change that amounts to a gradual transformation: figure a. gradually develops an up-stroke at the beginning, or figure b. gradually loses its upstroke (something like Dom Ferretti's representation of the *punctum* from the grave accent: | ı ı _ . ◆ ■ [*Paleographie musicale* 13, 65]). Such a transformation could take fifty years or longer. But what seems just as likely to me, given the fundamental difference in functional principle, is that someone says, "no, let's not represent musical figures that way, let's do it this way," or that two different starts were made, perhaps one triggered by the other, but based on different principles. Either way the time interval could be fifty years, but also five years or five months. Levy's "paleographic common sense" comes down to a belief in paleographic development as a gradual transformation of paleographic forms. That is hardly the only view we can take of paleographic change, and it is certainly not privileged as "common sense." It does not, therefore, constitute very solid ground for establishing chronologies.

There is a similar conception of change in straight lines and in one direction behind the liturgical arguments as well, and in particular it is a supporting thread in the complex web of the "seventh index" (pp. 11 ff.). There Levy proceeds from Michel Huglo's identification of the offertory *Factus est repente* as Gallican, taking that as evidence of its antiquity (Huglo 1972 in Levy's bibliography, p. 226). Putting that together with the solo appearance of *Factus* in but one of the *Sextuplex* sources (as a "survivor," "barely hanging on"), as the second of two offertories provided for Pentecost, he concludes it was "obsolescent" in the North by ca. 800. On that ground he regards the Canosa gradual (mid-11th century) as representing an "earlier" state of the Gregorian gradual than any of the *Sextuplex* sources. And in reporting the presence of *Factus* in the Prüm gradual he regards it as an "archaism." A simpler, more strightforward report would have it that the *Sextuplex* sources and the Canosa and Prüm graduals all represent the liturgical conditions in the time and place in and for which each was written, and that they incorporate elements of greater and lesser age. Somewhere in the background of Levy's particular type of interpretation is a normative idea of liturgical progression, and when particular items are not in step with that progression they are explained with such expressions as "archaic," "earlier states," etc. This is no less dubious than those familiar accounts of music in the 18th century in which one trait or composition or composer points to the future whereas another is retrospective. The pluralist character of the historical situation is ironed out in the interest of a smooth, rectilinear historical narrative.

In the same publication in which Huglo identified *Factus* as Gallican, he also identified the offertory *Elegerunt apostoli* as Gallican, and on the same grounds (p. 226). This offertory, too, is entered in only one of the *Sextuplex* sources (Senlis) where it is assigned to St. Stephen. On Levy's reasoning those two facts would mark it, too, as a survivor, barely hanging on. But we find it still as the offertory for St. Stephan in the *Graduale romanum* (1938). We could regard it as an archaism there. But Huglo gives the simpler explanation that it has replaced the Gregorian offertory *In virtute*, which is everywhere else in the *Sextuplex* sources. That suggests that the exchange between the Gallican and Gregorian traditions—at least as far as the offertories are concerned—could be a two-way affair. The principle about the displacement of one liturgy (e.g., the Gallican) by another (e.g., the Gregorian) is a generalization and approximation of what happened in the long run, a kind of sketch. It does not provide so detailed a picture of the liturgical situation at any particular time that inferences can be made from it about the chronology of individual items. (The *Elegerunt* story is not reported in Levy's paper.)

A similar image of historical change underlies the discussion about "the probabilities of survival," with particular reference to the history of notation: ". . . the neumed antiphoners were rendered obsolete by notational innovations of the tenth and eleventh centuries. The emergence of staff lines and clefs meant that new books were substituted, and the older ones with prediastematic neumes had little further purpose" (p. 6). But now and then one encounters a manuscript that shows that the story of notation cannot be told in a narrative of such a straightforward progressive form. I'll just mention one that I happen to have run across recently: Munich, Bayerische Staatsbibliothek, Clm 17025, a calendar, gradual, proser and sacramentary from Scheftlarn, written in the 13th century. The notation is reported in *The New Grove* as "small quadratic notation on four red lines" (17:629). But there are pages where the notator changes from that notation to non-diastematic neumes, perhaps to save space, and possibly to record more familiar matter such as hymns (folios 5, 44, 76v, 101v). We encounter something similar in both continental and British manuscripts with organum, in which the new organal voices are recorded in a more nearly diastematic notation than the old chants. In such situations different modes of notation, functioning on different principles of representation, were simultaneously available and could be applied to needs that differed according to the type of melody that was to be represented and the different competencies of the singers. That suggests a somewhat more complex picture than that of successive notational types strung out on a thin time-line.

What becomes more and more evident through this review is how Levy's hypothesis rests, not so much on his "witnesses," as it does on these few historiological principles that are the basis for the selection and interpretation of evidence. The paradox is that the more we unravel the intimidatingly complex surface of the argument, the more drastically oversimplified is the representation of the texture of the historical world that comes into view.

How it all works is vividly illustrated by the interpretation of Aurelian, Levy's fifth "index" (pp. 9–10) and the last of the ones on which I shall comment here. Levy offers a series of interpretations of remarks by this enigmatic author, without ever telling us why these should be privileged above other possible interpretations: In referring his "specialist readers" [more likely, his listeners] to particular chants Aurelian "may have expected [them] to have neumed antiphoners for consultation." "Aurelian makes detached melodic comparisons, singling out individual syllables of particular chants... with such directions as 'on the fifteenth syllable of . . .' This too indicates neumation." "The survey of the verse repertory that Aurelian describes . . . is unlikely to have been a scroll through a memory bank but a point to point comparison of neumed chants in a reference antiphoner." "When Aurelian opts to preserve [a] musical anomaly 'because it was used among the ancients' he is likely to have found the former tradition in a noted antiphoner." "The reader is told of a musical passage in two Gregorian gradual verses that is 'not found elsewhere in the prolixity of the whole antiphoner.' One *must* again conclude that Aurelian is not referring to a singer's well stocked memory but to the 'prolixity' of a fully neumed antiphoner [my italics]."

Each of these passages can be understood just as well in the sense that Aurelian did indeed expect his audience to know from memory the verse repertory that he described, especially in the light of two passages of the same treatise that Levy does not cite: "Although anyone may be called by the name of singer, nevertheless he cannot be perfect unless he has implanted by memory in the sheath of his heart the melody of all the verses through all the modes, and all the differences both of the modes and of the verses of the antiphons, introits, and of responses." (The passage, beginning 'Porro autem' and ending 'in teca cordis memoriter insitum habuerit' can be found in Gushee's edition, p. 118; see Levy's bibliography for details.) The second passage harks back to Isidore of Seville: 'The muses, from whom [music] took its name and by whom it was reported to have been discovered, were declared to be the daughters of Jupiter and were said to minister to the memory, for this art, unless it is impressed on the memory, is not retained. ('Dicebatur autem musae . . . non retineatur'; Gushee, p. 61. Both passages are cited in my 1984 paper, p. 161, where the Latin can also be found.) I wouldn't read these two passages against the *possibility* of Levy's hypothesis of a date of ca. 800 for a notated archetype, for surely there was sometime in the early history of all this when notated books existed but practice relied mainly on oral tradition or memory (those are not different); it is the allowance for pluralistic situations of just this sort that is missing from Levy's paper. But those passages do speak directly against Levy's forced use of Aurelian as a witness for the necessity of his hypothesis ("One must again conclude..."). Indeed, only a prior conviction about the conclusion could produce these selections and interpretations of evidence. That goes for the paper as a whole.

But there is something more about this circularity. The movement of music through tight scriptual channels defines a condition of musical transmission at one extreme through the history of the high art-music of the West. That condition sets the frame in which this story has been thought out, as is apparent even in the choice of language: writing of a transmission as a "recension" (p. 15); reporting when a manuscript was compiled by saying it was "copied around 1000" (p. 17); reporting differences in the neumation of an item as "replacement," "resolution," "translation" (p. 20), always without any effort to back up the special connotations of those words. But the story did not unfold entirely in that frame. Its unfolding is itself the beginning of the erection of that frame, but the reasoning is as though the frame were eternal and universal.

David Hughes ("Evidence for the Traditional View of the Transmission of Gregorian Chant," this JOURNAL 40 (1987): 377–404) allows that "there was perhaps a time at which this was the state of affairs," referring to his formulation of what amounts to an oral component in the transmission of chant (p. 377).[1] But he writes that that time belongs to a "probably irrecoverable phase of chant history." I cannot agree with that, and not only because of my belief that a stable written tradition is not necessarily incompatible with a continuing measure of orality in the practice. Even granting that it were, the oral origin is visible through the written surfaces

[1] I am afraid that Hughes has misrepresented my position in his lead paragraph, especially in attributing to me the belief that "each performance of a specific chant was in part an improvisation, and *hence* to at least some extent different from all others [my italics]." I do not wish to burden your readers now with an extensive commentary on this paper as well. But I do beg to remind them, and Professor Hughes, of a conclusion in my first publication on this subject: ". . . by the time of writing the melodies were being transmitted as *individual* melodies, not as concrete instances of melodic types; that a degree of standardization and individuation of the repertory had taken place; that the model for each performance was a particular plainchant, not the principles of a melodic type—something more nearly like the ordinary notion of memorization than oral composition. This would be to suggest, finally, that the tradition of oral composition had declined by the time of writing down . . . ("Homer and Gregory: The Transmission of Epic Poetry and Plainchant," *Musical Quarterly* 50 (1974): 333–72; the passage cited is on pp. 367–68.) This would, of couse, be entirely consistent with the results that Hughes has presented. And I would like to recall also a passage on p. 346 of the same paper: ". . . we require an understanding of oral transmission as a normal practice whose object and effect is to preserve traditions, not play loose with them." With all due respect to Giulio Cattin, his dictum "Where oral tradition prevails, no performer need feel obliged to repeat the same song identically at each performance" cannot be read as authoritative (Hughes, note 34). Hughes has read into my work on this subject an idea that all chant performance during the time spanned by his manuscripts was at least partially improvisatory, and to that extent it was casual and unstable. This misreading on both scores has, I regret to say, constituted a disruptive noise in the channels of communication.

that are its progeny, no matter how many generations of sources and whatever measure of redaction separate them. That is as surely so for the plainchant traditions as it is that the oral-compositional nature of a blues performance would show through generations of pressings of a recording, or of editions of a transcription of it. How the written transmission was related to the performance practice in the communities for which it was written is a separate question that has still to be pursued. But it will surely not yield a uniform answer for all traditions and all communities. The theory of oral composition with respect to plainchant and related traditions was not ever intended to account for variation in transmission as its primary task; variation was but one clue, and not the principal one. The primary task has been to show that the generative systems of the oral tradition—which were carried into the era of composition with the aid of writing—informed the music that was produced, and that they are therefore relevant to the understanding of that music and of the history of its transmission. No matter how uniform the written transmission is, and even if we regard it exclusively as a product of copying faithfully from one source to another, it is transparent to the oral tradition that was its ultimate source.

What is striking is the resistance to a serious confrontation with the reality that at some time in history, no matter how far back one wants to push it, the Western musical heritage goes back to an oral tradition that left its mark and that is never entirely out of the picture as a factor in musical practice. This is something for music historians to deal with, as well as ethnomusicologists.

I should like to join my colleagues Professors Hughes and Levy in dedicating these remarks to Michel Huglo on his 65th birthday.

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To the Editor of the JOURNAL:

IN "CHARLEMAGNE'S ARCHETYPE" I proposed an early date—by ca. 800—for an authoritative full neumation of the Gregorian repertory. That is a century before the recent opinions of Corbin, Hucke, and Treitler have put it; it is earlier than any previous qualified opinion. Specialists have long looked to Charlemagne, and on Charlemagne's word to Pippin before him, as the movers in stabilizing the Gregorian melodic repertory. But no one thus far has undertaken to demonstrate that the repertory so stabilized was cast in neumes. Professor Treitler finds my early date "unsettling." The reason, as he makes clear, is that it calls into question certain theories he has been promoting for some time, and much of his communication is given over to the restatement of those theories. There is the theory of "re-improvisation" (to use the term of van der Werf, *Emergence of Gregorian Chant* [1983]); this supposes that our early noted versions of Gregorian chant do not represent fixed melodic shapes, but instead remained susceptible to improvisatory impulse even into the 11th and 12th centuries. There is the related theory of "frozen improvisation" (which Treitler shares with Hucke: this JOURNAL, 33

[1980]), which supposes that the early Gregorian neumations are "transparent" windows on prior oral-improvisatory practice. And there is the theory of the origin of neumes, which, in a complement to his view of the Carolingian musicians' improvisatory stance, he identifies in "the Carolingian system of punctuation" (see his "Reading and Singing": *Early Music History* 4, [1984]). Treitler's principal reproaches to me are that I ignore the substance and consequence of these related theories. The length of my paper limited discussion to the most pertinent matters concerning date, but since the other theories have now come up, let me briefly say why I do not think they are very good.

The theory of "re-improvisation" addresses the nature of the neumed recension. Treitler speaks of "an oral performance practice that continued after people began to write things down." He interprets variants in Gregorian recensions of the 10th and later centuries as indications that the melos remained open to oral, improvisatory input; he even speaks of "a whole series of indications that the performance of the Mass propers during the 9th century was based on an oral tradition"; these are indications one should want to review. For my part, I see a written archetype that reflected a fixed, concretized melos: a model neumation of the late 8th century that provided a detailed record of a crystallized melodic text that excluded improvisation. The unheighted neumes described certain aspects of the melody in considerable detail; they showed relative lengths and some niceties of delivery, like liquescences, *oriscus*, and *quilisma* (to give them their later neume-names). That neumatic specificity, present even in the archaic Paleofrank notation, is a good indication of the fixity of the melos. What the early neumes dealt with least well was pitch. For pitch, the neumes offered only silhouettes of ups, downs, and repetitions; they lacked the particulars on levels and interval widths. Yet of all the melodic factors, pitch was most safely left to the memory that served as backup to the neumes. Between Treitler's views and mine, the essential difference is the nature of the oral input that supplemented the neumes. Where he sees a continuing license to improvise, I see missing particulars that were supplied from verbatim memory. When neumation began, the Gregorian melos seemed secure in professional memories. But soon the interaction between an incomplete, evolving notational system and an attenuating memory resulted in small changes that appear in 10th through 12th century copies. These are copies removed from my proposed "early" neumed model by at least a century.

With "frozen improvisation," Treitler moves the theory of "re-improvisation" backward to the terrain of prehistory. He observes that much of plainchant originated in improvisatory maneuver, and he adds that the preserved Gregorian neumations are good windows on oral-improvisational practice. The first observation is safe. When Wagner (*Gregorianische Formenlehre*, 1921) and Ferretti (*Estetica gregoriana*, 1934) characterized florid Gregorian chants like the Tracts as realizations of "psalmodic" frameworks, the improvisatory factor was basic to their conception; when Cardine (*Congresso Internazionale di musica sacra*, [Rome, 1950], 187–91) declared there was an unwritten stage behind the first neumations, that factor became explicit. Treitler's statement that "the oral origin is visible through the written surfaces that are its progeny no matter how many generations of sources and whatever measure of redaction separate them" essentially affirms what has long been evident. It is his second observation that raises an issue. He writes, "No matter how uniform the written tradition is, and even if we regard it exclusively as a product of copying faithfully from one source to another, *it is transparent to the oral tradition that was its ultimate source* [italics mine]." Actually, how good a window on orality does the Gregorian recension supply? We have the one late, remarkably uniform recension. Is it a "transparent" record of oral deliveries, or has the melos on its way to this ultimate recension undergone enough in the way of written compositional and editorial tinkering to distort our rearward vision? It will take more than the arguments Treitler discovers in comparative literary theory ("Homer and Gregory," *Musical Quarterly* 1974) to convince me that the solo deliveries of Balkan epic bards illumine the choral ritual of the Gregorian schola cantorum, or that the Gregorian recension we have stands as a "transparent" mirror of improvisational orality, rather than the end-product of a considered editorial process.

The largest part of Treitler's communication is taken up with the early history of neumes. Here there are two major questions. One is ultimate origins. The other, what Froger (*Le graduel romain*, IV, 2 [1962]:92) described as the *zone brumeuse*: that is, supposing there was an "original," noted archetype, what kind of neumation could it have that would produce the different neume-species found in Lorraine, St. Gall, Brittany, etc. ca. 900? No one has answered these questions, and Treitler complains in effect that my proposal of the "early" written model does not do so. In "Charlemagne's Archetype" I remarked that a complementary paper "On the Origin of Neumes" was on the way. It appeared in the same year (*Early Music History* 7 [1987]: 59–90), offering a review of current theories (among them Treitler's of 1982 and 1984) and some fresh formulations. My aim was to place "Charlemagne's Archetype" in its notational-historical context. Concerning the *zone brumeuse*, there are two proposals. First, the model neumation ca. 800 (Froger's "original") had a cursive, "conjunct" ductus (as found in the majority of 10th-11th century copies from all over), not a "disjunct" ductus (as in the neumations of Brittany, Aquitaine, Nonantola, and to some extent Lorraine). Second, contrary to what is generally supposed, the model neumation was not "nuance-rich" (like St. Gall 359, Einsiedeln 121, or Laon 236), but "nuance-poor," as in the majority of surviving early sources. During the 9th and 10th centuries there were clarifications and supplements (*episemata*, "Romanian" letters, *coupures*, etc.) that put onto written record some of the detail that was previously left to memory. This was done differently at different places, and it contributed to the variety of the "nuance-rich" notations. My other concern in that paper was the ultimate origin of neumatic notation. My proposal is that the "original" neumes represented a "graphic" method: they amounted to a plotting of relative pitch positions, along with some details of duration and nuanced delivery. Vestiges of this method survive in the notations called "Paleofrank." Some time later, in "Charlemagne's Archetype" and its descendants, the "graphic" was replaced by a second method that I describe as "gestural" or "cheironomic." It is the same method that has long been suggested as an ultimate "origin" of neumes. I propose it only as a later stage, without connection to origins. However I see the implementation of the "gestural" rationale as a major factor contributing to the differentiation of local neume-species.

In sum, my proposals put chronology, memory, and notational etiology into a single frame where certain of the major problems that have long vexed musical Gregorianists may be resolved. I attribute to the generation approaching 800 an activity that other undertakings of Charlemagne's circle (the reforms of texts, script, education, liturgy, clerical life) might suggest was theirs: the promulgation of a canonical musical repertory in an authoritative, written, neumatic formulation that was designed to facilitate teaching and performance. Earlier stages in that musical development—and perhaps the notational development itself—may, as Charlemagne says, be laid at the doorstep of his father Pippin. I claim no decisive proofs; my interpretations of date and notational method may turn out wrong, and at the least I expect clarifications and adjustments to emerge. But for the moment I believe I have gathered together the main surviving elements of the puzzle, and for the first time have made them fit into a coherent whole. In Professor Treitler's communication I can discover nothing that shakes my view. Quite the contrary: he finds my date for the neumed model "very unsettling," and he proposes that we "split the difference by adopting Michel Huglo's suggestion of a terminal date of mid-ninth century." That of course brings it near the likely date of Aurelian's treatise—the 840s. But I think that is not enough. I think my model of an early neumatic promulgation (by ca. 800) of a musically concrete, editorially processed, stabilized melos stands much closer to the reality of the Gregorian musical situation than the late neumation and "frozen-improvisational," then "re-improvisational," model that Treitler has endeavored to defend.

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To the Editor of the JOURNAL:

LET ME TAKE care of one small matter first. I cited Cattin's description of the *troubadour-trouvère* oral tradition only with reference to that repertoire, and that is made clear in my paper. Central to my thesis is the solidity of the Gregorian tradition (or traditions, if we want to separate the oral from the written in this context): the tradition of secular music is quite different, and I referred to it only by way of contrast.

As to misrepresentation, I am inclined to plead slightly guilty with extenuating circumstances: to sum up the complex work of Professors Treitler and Hucke in a sentence or two is no easy matter. I tried to give both authors full credit for diversity and subtlety; but in Professor Treitler's "Homer and Gregory," surely the whole issue revolves around the use of formula—and hence around the question of oral composition. "[The singer] will have planned before beginning . . . quickly thinking what should come next as he scans the next phrase of text" (pp. 346–47); "I believe that such sports [standard formulas in unusual positions] complement the more consistent transmission of melody types in supporting the view of chant performance before the age of notation as a process of construction or reconstruction . . . " (p. 367). Our difference in this respect lies in our