manipulation of data, either conscious or unconscious—while looking at exactly the same objects, and this manner of seeing inevitably influences the results he or she ultimately reaches. That we two should have come to such different conclusions from the same data may perhaps be partly explainable by some process such as this. If so, it is no more than a signal for all of us to continue to be careful: there is an element of the subjective in any interpretation whatsoever, and while it cannot be entirely removed, it can and should be minimized. If I cannot yet agree with the theories of another scholar, that proves nothing except that I have not seen his data the way he does: perhaps some day I shall, perhaps not.

An issue is generally regarded as settled when all or nearly all those who specialize in the question agree on one interpretation (a situation that certainly does not obtain for chant origins or transmission), but is promptly reopened whenever any scholar reviews accepted theories and finds them wanting. That chant scholarship—by far the oldest branch of historical musicology—is still in such ferment today testifies merely to the extreme difficulties of the problems it poses, and to the vigor of its scholars.

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To the Editor of the JOURNAL:

THE CORRESPONDENCE FOLLOWING Professor Richard Taruskin's impressive and influential article "Antoine Busnoys and the *L'Homme armé* Tradition" (this JOURNAL 39 [1986]: 255–93) has so far failed to comment on the fundamental issues of mensural practice that underlie the analysis of the durational design in Busnoys' *Missa L'Homme armé*. The object of the present letter is to open the debate on these issues by discussing three areas of Prof. Taruskin's analysis where a reconsideration of the evidence is perhaps in order: (1) the duration of the "Et incarnatus;" (2) the interpretation of the signature ○2; and (3) the mensuration signs in the "Confiteor."

(1) The first issue concerns final longs. As Professor Taruskin states (pp. 257 and 271–72), the final long is a *nota ultra mensuram*, of indefinite duration, and is therefore to be excluded from the total duration of a composition or section. This is a clear-cut principle, yet it does not always guarantee clear-cut decisions. There are numerous examples in the fifteenth-century Mass repertory where the final longs of the different voices start consecutively rather than simultaneously.

The "Et incarnatus" of Busnoys' *Missa L'Homme armé* provides an example (see Example 1). After thirty tempora (in terms of tempus under ○) the tenor is the first to state its final long; it is followed, one breve before the end of the thirty-first tempus, by the top voice and the contratenor [altus], and, one breve *after* the thirty-first tempus, by the contratenor [bassus]. This is clearly an ambiguous passage. Therefore, simply to say that the counting stops at the thirty-first tempus, as Professor Taruskin does (p. 270), is to draw the line rather too easily, at a point which does not coincide with the entry of a

final long in any voice. A more convincing place to stop the counting seems to me the thirtieth tempus: this is where the tenor stops. Moreover, the imitative continuation in the other voices can easily be seen as a cadential flourish in the final "d minor" harmony. We find support for this alternative interpretation in Obrecht's *Missa L'Homme armé* and in the anonymous *Missa De Sancto Johanne Baptista*,[1] both of which take over the complete layout of Busnoys' cycle. I raise the point because the difference between "30" and "31" in the "Et incarnatus" has a crucial bearing on the validity of Professor Taruskin's hypothesis.[2]

(2) Is there a difference between the relative duration of a section "on paper" and the duration in practice, and is this difference relevant to the discussion here? Theoretically, such a difference was not supposed to exist. Yet the reality was often different. One example is the difference between 𝔠 when it occurs in all voices and 𝔠 when it is combined with other mensurations. As Professor Taruskin convincingly argues (p. 261, n. 15): "(. . .) when all voices share it and when it occurs at the beginning of a tenor cursus (. . .), the prolation signature reverts to its original meaning, in which the minim is equivalent to that of the minor-prolation signatures."[3] In Ockeghem's *Missa L'Homme armé*, for instance, this means that the relative duration of a 𝔠 -minim in the "Et in terra" (𝔠 in tenor; ○ in other voices) is in practice twice as long as in the "Et resurrexit" (𝔠 in all voices), although on paper they are the same. How are we now to count the duration of the "Et resurrexit?" On pp. 275–76 of his article, Professor Taruskin chooses to count the "Et resurrexit" as it appears "on paper." I respect this interpretation, but since so much of Professor Taruskin's hypothesis depends on it, it would have been only fair to have added that it does leave room for discussion.[4]

Busnoys' use of the sign ○2 in the *L'Homme armé* Mass presents another case. Theoretically, ("on paper"), the sign is equivalent to ₵ (in perfect minor modus) and it therefore calls for a tempo increase. There are indications, however, that in this Mass Busnoys meant the sign in practice to

[1] See R. C. Wegman, "Another 'Imitation' of Busnoys's *Missa L'Homme armé*—And Some Observations on *Imitatio* in Renaissance Music," paper read at the Sixteenth Annual Conference on Medieval and Renaissance Music, Edinburgh, August 12–15, 1988 (to be published in the *Journal of the Royal Musical Association*). The Mass for St. John the Baptist is in the Sistine Chapel manuscript 160, fols. 49r-63r. In the "Et incarnatus" of this Mass, all voices state the final long after thirty tempora (in terms of tempus under ○). In the "Et incarnatus" of Obrecht's Mass *L'Homme armé*, three voices state the final long after thirty tempora, and the fourth a breve before the thirty-first tempus.

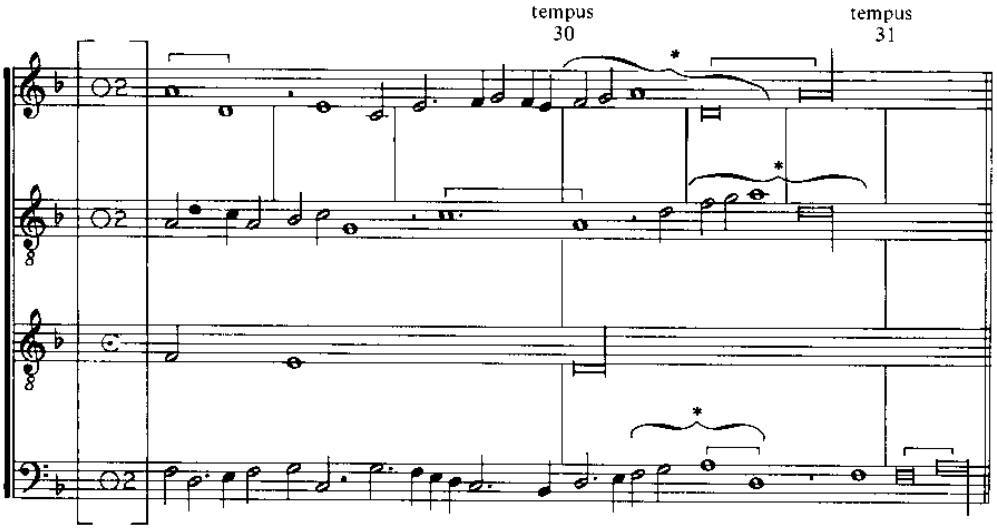
[2] It is only fair to add here that the number 31 in the "Et incarnatus" of Ockeghem's *Missa L'Homme armé* (Taruskin, p. 273) occurs only in the VatS 35 version of the Mass, and that an explanation for the difference from the version in Chigi C.VIII.234 has been offered by Gustave Reese (*Music in the Renaissance* [New York, 1954], p. 125, n. 154).

[3] There is also theoretical confirmation for this; see J. A. Bank, *Tactus, Tempo and Notation in Mensural Music from the 13th to the 17th Century* (Amsterdam, 1972), pp. 167–71.

[4] Particularly since Prof. Taruskin adopts the opposite principle elsewhere in his essay. On paper ○◇◇◇ = ₵ 𝅜𝅜𝅜, but in his analysis of the Naples *L'Homme armé* Mass III, Prof. Taruskin adopts the following policy: "when all voices carry the sign ₵ (. . .) then ○ᵖ = ₵ 𝄽 (i.e. *proportio sesquitertia*: = ○◇◇◇ = ₵ ◇◇◇◇)" (p. 279, n. 37).

Example 1

Busnoys, *Missa L'llomme armé*, final bars of "Et incarnatus" (after L. Feininger, ed., *Antonius Busnois, Missa super L'homme armé*, Monumenta Polyphoniae Liturgicae Sanctae Ecclesiae Romanae, ser. 1, tom. 1, fasc. 2 [Rome, 1948]).



*Curved brackets added by Professor Taruskin (see p. 444).

be equivalent to C (in perfect minor modus). Most importantly, he uses the same note-values in O as in O2[5] (whereas he makes a shift to larger note-values in all his other sacred compositions where O2 is employed—except *Anthoni usque limina*). To perform the semibreves in O2 twice as fast as those in O (as Professor Taruskin assumes to have been Busnoys' intention; cf. pp. 255 and 269) would therefore produce the same effect as to perform the sections in O at double speed. We have the unequivocal testimony of at least one fifteenth-century theorist that the effect of such a performance (Professor Taruskin speaks of "breakneck speed" and "dizzy prestissimos;" ibid.) is not to be desired, since "a difficulty of pronunciation and even a destruction of the whole melody would be heard because of the excessive speed."[6] Bruno Turner's well-known recording of the Mass (Archiv, 1978)

[5] This can be demonstrated on the basis of counts such as the ones presented in C. Hamm, *A Chronology of the Works of Guillaume Dufay Based on a Study of Mensural Practice* [Princeton, N.J., 1964], but a more accurate method is the calculation of average note-values (see my article "Concerning Tempo in the English Polyphonic Mass, c. 1420–70," forthcoming in *Acta Musicologica*). The average note-value of the upper two voices of Busnoys' Mass is 0.685 semibreve in the sections in O2, and 0.632 semibreve in the sections in O. In practical terms, as I will show, the difference between these figures is negligible.

[6] J. Tinctoris, *Proportions in Music*, trans. A. Seay (Colorado Springs, 1979); see also J. Tinctoris, *Theoretical Works*, vol. 2a (*Proportionale musices*), Corpus scriptorum de musica, no. 22 ([Rome], 1978), p. 50. It should not be inferred from Tinctoris's sentence that such performances actually existed; he merely mentions this as the drawback of a purely hypothetical solution to remedy the theoretical inconsistency of "prolatio maior" augmentation.

sensibly keeps the same tempo for ○ and ○2 (c. 60–66 semibreves per minute).

But assuming that in Busnoys' Mass ○2 and 𝐂 are equivalent in practice, what happens when ○2 is aligned vertically with another mensuration, e.g. 𝐂 (as in several sections of the Mass)? Obviously in such cases the theoretical tempo relationship between the mensurations must be retained (namely 𝐂 𝅗𝅥 = ○2𝅝). To write 𝐂 instead of ○2 would destroy that relationship. But when *all* voices are in ○2 the situation is different. The three-voice "Christe" and "Benedictus" of Busnoys' Mass both have ○2 in all voices (at least in most sources for the cycle). Clearly there is no reason why ○2 could not be replaced here by 𝐂 (assuming for the moment that ○2 was the original sign). And this is precisely what happens in two sources for the Mass, Chigi C.VIII.234 and Barcelona 454.

What I would suggest is that all sections in ○2 were performed with the semibreve at the same speed as in ○ and 𝐂, and that whenever the sign was combined with 𝐂, the theoretical 4:1 relationship between ○2 and 𝐂 was retained on paper, leading in practice to a further "augmentation" of the notes in 𝐂 (which, again, was the solution adopted by Bruno Turner).[7]

Where does this leave the counting of durations? Professor Taruskin consistently counts all sections in ○2 as they appear on paper, i.e. as equivalent to 𝅗𝅥, not 𝐂. But the theoretical distinction between the alternative manuscript readings ○2 and 𝐂 for the "Christe" and "Benedictus", admittedly of no significance in performance, then becomes critical for this discussion. Should we take ○2 or 𝐂 to be the original sign? My own tendency is toward ○2, partly because it is easier to imagine a scribe replacing the very rare sign ○2 by the commonly-used 𝐂 than the other way round. This would mean, however, that the number of tempora in the "Christe" and "Benedictus" would be 8 and 12, respectively, rather than the 16 and 24 given by Professor Taruskin (in terms of tempus under 𝐂; see p. 270). Consequently, as he predicts, the "impressive scholastic edifice" of durational proportions in his Table 2 would crumble (ibid.). It is to resolve

Prof. Taruskin agrees that on empirical grounds, theoretical tempo relationships cannot always be maintained in practice, for instance in the case of ○ and 𝅗𝅥: "if one adopts a duple proportion (. . .) one must either perform the section in ○ too slowly or the section in 𝅗𝅥 too quickly for comfort or apparent sense. (. . .) I myself had long since become convinced that ○𝅝 = 𝅗𝅥𝅗𝅥 purely on the basis of my practical experience as director of Capella Nova (. . .)" (p. 280, n. 37).

Readers who are interested to hear what 'dizzy prestissimos' in ○2 actually sound like are advised to listen to the recent recording of Josquin's *Praeter rerum seriem* by The King's Singers (EMI CDC 7491572). There is no better way of describing this rendering of the motet than Tinctoris's "difficultas pronuntiationis immo totius melodiae destructio" (I am indebted to Dr. David Fallows for having drawn my attention to this recording).

[7] Space does not permit me here to fully work out all the arguments. In my forthcoming dissertation on the Masses of Jacob Obrecht I will argue at much greater length that two separate traditions of ○2 existed in the late fifteenth century (that is, not counting the early fifteenth-century interpretation of ○2—perpetuated only by Tinctoris—in which ○2 is identical with ⌽). Both traditions can be found in the sacred works of Busnoys. My point here is that Busnoys's *Missa L'Homme armé* forms part of a late fifteenth-century tradition of compositions in which ○2 was, in practice, equivalent to 𝐂 in perfect minor modus.

precisely this problem that Professor Taruskin assumes ○ to be Busnoys' original mensuration sign in the "Christe" and "Benedictus".[8]

This leads to the fundamental question: did Busnoys really design the *L'Homme armé* Mass as it is represented in Professor Taruskin's Table 2? Much depends on this question. For if the answer is *no*, then

(a) there would be no reason to dismiss all sources for the Mass except Chigi (and possibly Barcelona) as inferior copies (pp. 268–71),
(b) there would be no reason to believe that 𝐂 in the Christe and Benedictus is the authentic signature rather than ○2 (ibid.),
(c) there could be no reference to the Order of the Golden Fleece in the length of the "Et incarnatus" (pp. 271–73),
(d) the case for Busnoys as the author of the Naples *L'Homme armé* Masses would remain as speculative as it was before (pp. 275–83)
(e) the relative length of the "Tu solus"—in relation to the entire durational structure of the Mass—could not be used as evidence for the assumption that 𝐂 3 is equivalent to 𝐂 (not calling for augmentation) (p. 286).[9]

(3) In the "Confiteor" of Busnoys' Mass, the signs 𝐂 and 𝐃 are vertically juxtaposed. According to Professor Taruskin (pp. 285–86) the stroke through

[8] Prof. Taruskin finds confirmation for his assumption in Obrecht's *Missa L'Homme armé*, which is modelled on Busnoys' Mass: "Special interest, of course, attaches to two of Obrecht's tenorless sections—the Christe and the Benedictus— (. . .) Sure enough, Obrecht's mensuration sign at these points is 𝐂 , affording further and (one trusts) conclusive evidence that Chigi is the source of preference where Busnoys's Mass is concerned" (n. 35, pp. 274–75). However, in no source for Obrecht's Mass is the signature 𝐂 actually used in the two sections. The most trustworthy source, VienNB 11883, employs the signature ○2 in the Benedictus, and 𝐂 in the Christe (see C. J. Maas, gen.ed., *New Obrecht Edition*, vol. 6, ed. Th. Noblitt [Utrecht, 1986], pp. xvi, 2 and 27). The single source for the anonymous *Missa de Sancto Johanne Baptista* (see note 1) gives 𝐂 for both sections.

[9] Although Prof. Taruskin's Example 8 (p. 289) clearly confirms this assumption (for his Example 7, see note 12 below), there is evidence that the sign 𝐂 3 existed in at least one other interpretation. In Petrus de Domarto's *Missa Spiritus almus*, for instance, three minims in 𝐂 3 (forming one perfect semibreve) are equivalent to one breve in 𝐂 2, and hence to two minims in 𝐂 (not calling for augmentation). The interpretation proposed by Prof. Taruskin, confirmed by the vertical juxtaposition of ○ and 𝐂 3 in the Naples III Mass (Example 8 in his article), is that three minims in 𝐂 3 were equivalent to three minims in 𝐂 , not two. Like Prof. Taruskin I would not wish Busnoys to have adopted Domarto's interpretation in the "Tu solus" of his Mass (all voices in 𝐂 3), for then the impressive durational layout of the cycle would seem to lose its Pythagorean significance. But since the sign is not vertically aligned here with another mensuration, who can say which interpretation he did adopt?

Admittedly, the Naples *L'Homme armé* Mass is much closer to Busnoys (both geographically and chronologically) than Domarto's *Missa Spiritus almus*. On the other hand, Domarto's interpretation is well established in five musical sources and Tinctoris (*Theoretical Works*, vol. 2a, p. 56), while the crucial sign ○ in the passage in the Naples Mass occurs only in a tenor resolution which was entered by a later hand. Since the manuscript source for this Mass was moved to the other end of Europe shortly after it had been copied, it could be dangerous to build hypotheses on the assumption that the man who wrote the resolution knew precisely what the composer had meant by 𝐂 3. The sign was, after all, far from unequivocal. Several composers used 𝐂 3 in a completely different sense from Domarto, Busnoys, "Borton" and the composer of the Naples Masses: the division of the semibreve in 𝐂 3 is duple rather than triple in e.g. Jean Cousin's *Missa Nigra sum* (or *Nigrarum*; cf. Tinctoris, *Theoretical Works*, vol. 2a, p. 56), and the anonymous Masses *Rex dabit mercedem* (Agnus Dei; VerBC 755, fols. 62v-63r) and *Vinnus vina* (Agnus Dei; VatS 51, fols. 80v-81r).

𝄡 cancels the augmentation normally prescribed by the major-prolation sign. The sign 𝄜, he continues, must then be deduced empirically. Given that so many details of the surviving fifteenth-century sources seem to demand empirical solutions from the singers, Professor Taruskin's reasoning is understandable. On the other hand, one would naturally prefer to have a solution that was both logical and well-fitted to the music of the "Confiteor"—provided that any such solution were available.[10]

It is here that the *Missa L'Homme armé* by Guillaume Dufay comes to our help. In the famous "Genitum non factum" passage in this Mass,[11] the very same signatures occur in vertical juxtaposition as are found in Busnoys' "Confiteor", save that the strokes are omitted. Moreover, the mensural relations between the two signatures are analogous to those in the "Confiteor", a perfect semibreve in 𝄡 being equivalent to an imperfect breve in 𝄐. Since in the same passage the mensuration sign ○ is combined vertically with the two signatures, we have here a clue as to how the controversial signatures are to be construed. Table 1 proposes to show how Dufay derived 𝄐, and 𝄡

TABLE 1

		stage 1 Dufay, *M. L'Homme armé*, "Genitum"		stage 2 Busnoys, *M. L'Homme armé*, "Confiteor"
○		𝄐 (*sesquitertia*)		𝄜
◇ ◇ ◇	=	◇ ◇ ◇ ◇	=	𝅗𝅥 𝅗𝅥 𝅗𝅥 𝅗𝅥
◇◇ ◇◇ ◇◇		◇◇◇ ◇◇◇		
◇ ◇ ◇	=	◇ ◇	=	◇ ◇ ◇ ◇
○		𝄡		𝄵

(not calling for augmentation), from ○ (stage 1); and how Busnoys went one step further in his "Confiteor" simply by putting strokes through both signs (stage 2).[12] From this it would follow that four breves in 𝄜, or four perfect

[10] It should be noted that although signatures like ○2 and 𝄡 3 were ambiguous, they were not illogical. The ambiguities arose because the prescribed proportional changes (dupla, sesquialtera) were carried out on different levels. In 𝄡 3, for instance, either three minims could be made equivalent to two minims in 𝄡 (Domarto; see n. 9), or three semibreves to two semibreves 𝄡 in (Cousin and the anonymous composers of the Masses *Rex dabit mercedem* and *Vinnus vina*). Both solutions were logical, and yet they were completely different in their effect. On the other hand, my interpretation of the sign ○2 in Busnoys' *Missa L'Homme armé* (and other late fifteenth-century compositions) is mainly based on empirical evidence (although there actually exist compositions in which ○2 and 𝄡 are functionally equivalent while being vertically juxtaposed; see TrentC 88, fols. 138v-139r and 150v-151r).

[11] See the facsimile in score in Bank, op. cit., p. 154.

[12] This is not necessarily to imply that Dufay's Mass predates the one by Busnoys, but merely that Dufay's "Genitum non factum" represents the intermediate logical step between perfect tempus and the signatures of Busnoys' "Confiteor".

The solution proposed here seems to remove the basis for Prof. Taruskin's interpretation of Pulloys' *Victimae paschali*, which vertically combines 𝄡 3 and 𝄵 (p. 289, Example 7). If the

semibreves in ¢, are equivalent to three semibreves in ○. Thus the duration of the "Confiteor" would equal on paper 9 tempora in ○, and not 18, as Professor Taruskin has it.

The consequence of this is that Professor Taruskin's Table 2 (as well as 6) seems in need of revision. The durational proportions in the Credo might well be 54:30:9 (rather than 54:31:18). These proportions have no apparent significance in terms of Pythagorean interval relationships. The Credo would then not form part of the "elaborate array of Pythagorean durational 'harmonies'" discovered by Professor Taruskin. And the answer to the question raised at the end of paragraph (2) would be a firm *no*.

Others must decide what Busnoys may or may not have intended to express in the durational proportions of his Mass *L'Homme armé*: there is certainly much still to be discovered. The present letter is concerned only with the fundamental questions of mensural practice which present-day musicological scholarship has to solve before we can even hope to know what fifteenth-century composers intended. Further study and discussion in this field is urgently needed.

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To the Editor of the JOURNAL:

ONE DOES NOT DISPROVE A HYPOTHESIS merely by showing it is not a fact. It is not a fact by definition. Nor does one disprove it by showing that it ain't necessarily so. One must show that it cannot be so; and this Mr. Wegman, on his own casual admission, has not done. Failing such a demonstration, one can attack a hypothesis by proposing a better organization of the existing data. The alternatives Mr. Wegman propounds do not accomplish this —rather the opposite. In place of a construction with many interacting and mutually confirming parts, he offers an atomistic assortment of details without pattern. His attempt to dismantle my hypotheses is founded on methodological premises which I cannot help regarding as impediments to historical knowledge or understanding. After meeting his specific objections I shall want briefly to return to these larger matters.

1) Mr. Wegman rather too hastily dismisses that "cadential flourish in the final 'd-minor' harmony" at the end of the "Et incarnatus." Anachronistic conceptualizations can often blind one to what may really be going on, which is perhaps the best reason to avoid them. Whatever its harmonic character, and whatever its purpose, the activity in the remaining voices that continues

signature ¢ in the "Confiteor" is to be interpreted as in Table 1, it can no longer be used to prove the assumption that the minims in C 3 in *Victimae paschali* are equivalent to the minims in ○ or C (not calling for augmentation; p. 289). Nor can C 3 be used to prove the assumption that the minims in ¢ are equivalent to those in ○ or C (not calling for augmentation) since that sign was ambiguous (see n. 9).

for more than a tempus after the tenor has come to rest clearly has the effect of prolonging the durational count. That the count must end when the tenor reaches its final long is nothing more than an untested assumption. This very passage, in fact, was the test that convinced me that Mr. Wegman's tenor rule was no rule, and confronted me with the "31 problem" in the first place—and I want to remind my readers that, until a solution was found via Prof. Prizer's independent investigations, this was indeed a problem! (Obrecht's Mass, which takes over from Busnoys only the tenor, has no light at all to shed on the passage in question; and neither does the anonymous *Missa de Sancto Johanne Baptista*, for the same reason.)

Assuming, then, that Busnoys wanted to extend the count past thirty —for, as a wise man once put it, "I must assume that a composer intended what he presented, since I could never establish that he had not"[1]—the question becomes how far. If all the accompanying parts came to rest together where the superius and the altus do (that is, before the end of the thirty-first tempus), or if all three accompanying voices ended with the last note of the little four-note motive they sing in imitation (see the brackets I have added to Mr. Wegman's Example 1), then one would have to agree that "this is clearly an ambiguous passage," uncountable in integers, and probably to be measured, as Mr. Wegman suggests, only up to the end of the tenor part (cf. the ending of Busnoys's "Qui tollis," in many ways a parallel construction to the "Et incarnatus").

It is the bassus that extends past the completion of the thirty-first tempus. I believe it can be shown that, although it does not come to rest there, the bassus nonetheless signals the cessation of the tempus count precisely at that point. When the thirty-first tempus is completed, the bassus is singing the note F, a third above the final in the tenor. Since that is not an acceptable interval for the end of a piece in Busnoys's style, the bassus proceeds after a breve's duration to the fifth, an A that is notated as a long in ligature with the F. Now this harmonically-clarifying progression—from the third to the fifth over the final, often notated as a breve-long (or long-maxima) ligature unless it is decorated with a passing tone (as often as not at the whim of the scribe), and usually in the altus—is a veritable cliché for decorating final cadences in music of this period (especially four-part cantus firmus settings), and almost always coincides with the final long in the tenor. Below I list the occurrences of the device in the *L'Homme armé* Masses published by Feininger in Monumenta Polyphoniae Liturgicae Sanctae Ecclesiae Romanae, series 1, tomus 1 (the many instances that are embellished with more than a single passing tone are omitted; those with asterisks are shown in my Example 1).

Dufay (fasc. 1):	Patrem
	*Agnus I
Caron (fasc. 3):	*Christe
Regis (fasc. 5):	*Kyrie I & III (in a ninefold scheme)
	Christe I & III
	Et in terra

[1] Milton Babbitt, "Colloquy and Review," *Perspectives of New Music*, vol. 2, no. 1 (Fall-Winter 1963), p. 131.

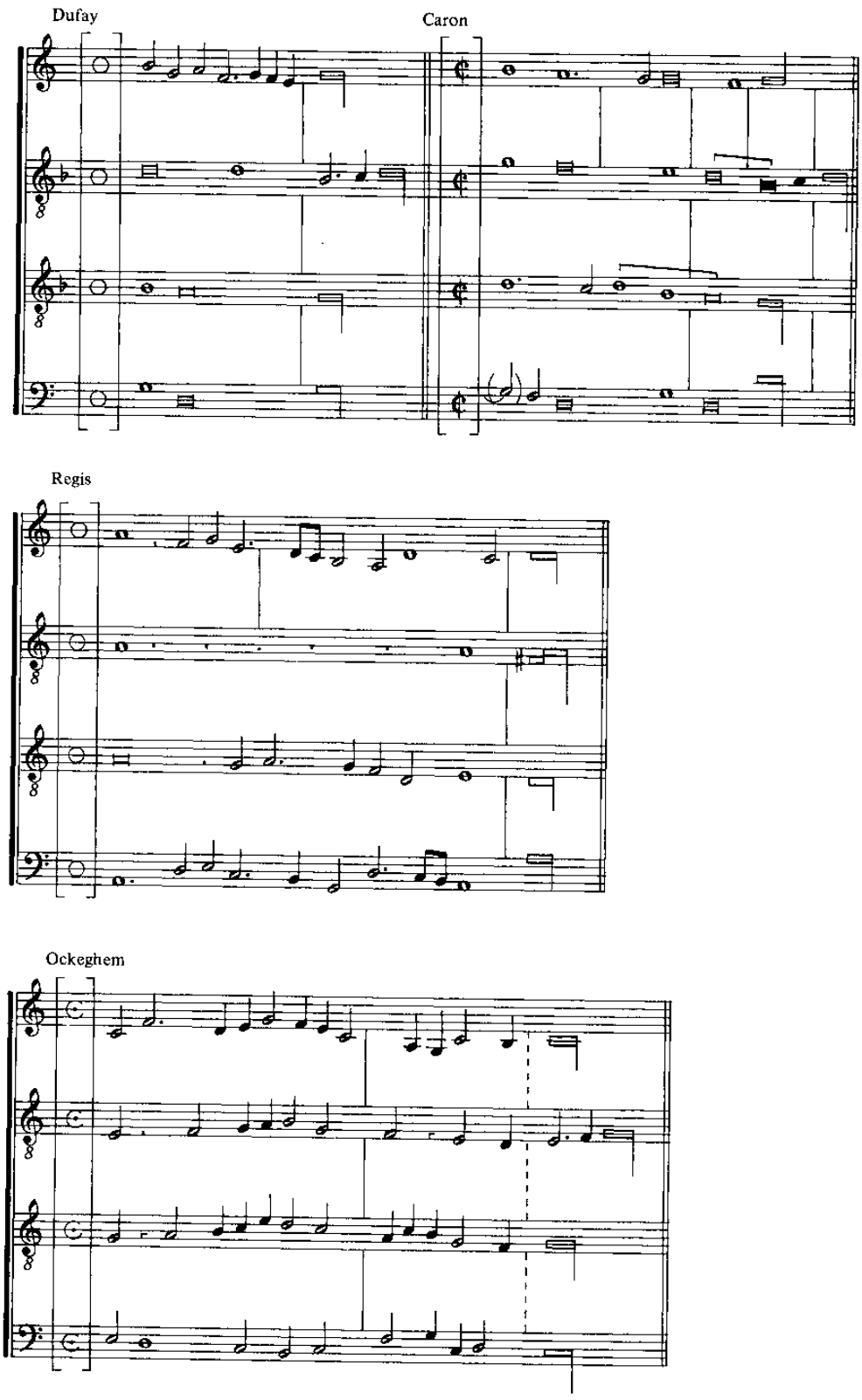
	Qui sedes
	Et incarnatus est
	Et iterum
	Sanctus
	Osanna (II)
	Agnus I & III
Ockeghem (fasc. 6):	*Et resurrexit
Tinctoris (fasc. 9):	Kyrie I
	Christe
	*Kyrie II
	Et resurrexit
	Sanctus
Vaqueras (fasc. 10):	Kyrie I
	*Kyrie II
	Qui propter
	Et in spiritum

In view of the frequency with which, even in such a small sample, the third-fifth progression accompanies the tenor's *ultra mensuram* final, meanwhile marking off the last countable tempus, one can easily hear it as such a marker even in the absence of the tenor. The behavior of the *bassus altizans* was the reason why I orginally counted the length of the "Et incarnatus" as precisely 31 tempora, not a breve more and not a breve less.

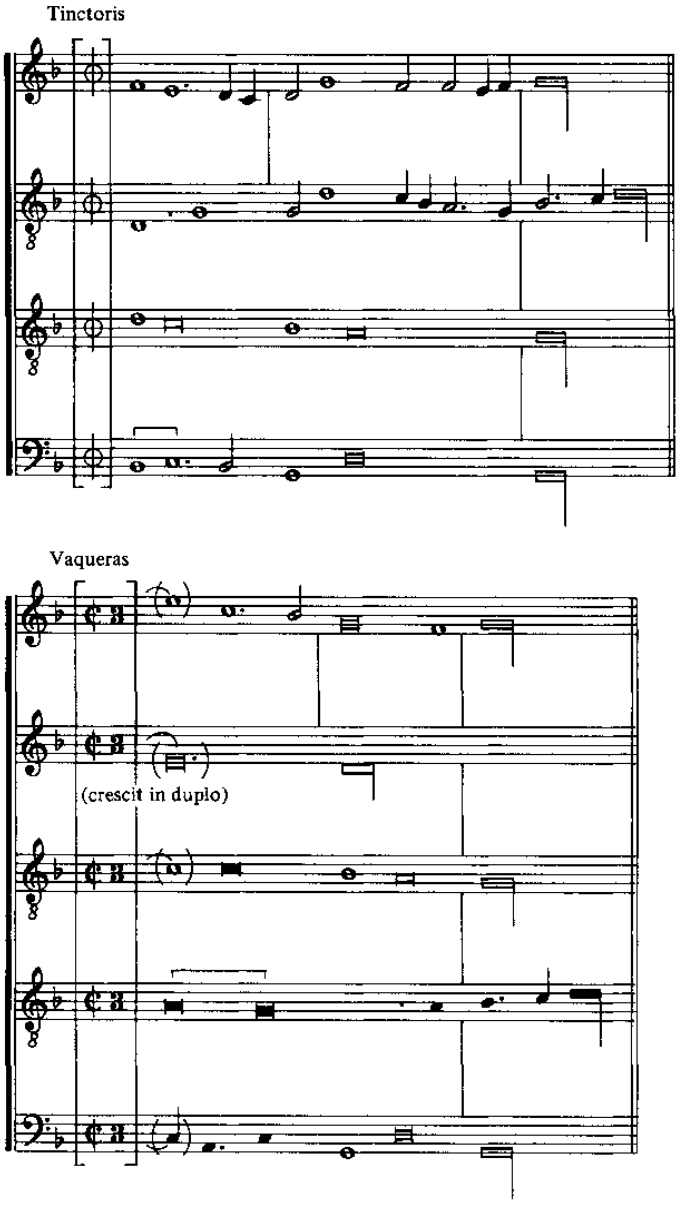
I cannot see in any case that Mr. Wegman is justified in ignoring more than a tempus of composed musical time simply because the tenor has stopped moving. How far ought one to go in privileging the tenor? There are many instances in the literature where the final tenor long is held as a virtual pedal while the accompanying voices play out lengthy passages of intricate counterpoint. Two notable cases in point can be found, as a matter of fact, in Obrecht's *Missa L'Homme armé* (Example 2): at the end of Kyrie II (two breves in ○) and at the end of the Osanna (two longs in ○2). Would Mr. Wegman exclude them from his tempus count? Would he cite Busnoys's mass as authority for doing so, as he purports to adduce Obrecht (anachronistically and therefore circularly, I'd say) as an authority for his interpretation of Busnoys? For an extreme example of final tenor long as pedal, one might consider the end of Isaac's big motet on the antiphon *Virgo Prudentissima* in honor of the Emperor Maximilian. Too lengthy to cite here (five longs in ○2), it may be found in DTÖ vol. 32 (ed. Johannes Wolf) or in the appendix to Louise Cuyler's *The Emperor Maximilian I and Music* (London: Oxford University Press, 1973). In the words of Glareanus (translated by Burney), and as this passage spectacularly illustrates, Isaac was "particularly fond of making one part sustain a note, while the rest were moving about, like the waves of the sea, against a rock, during a storm."[2] Early stages of Isaac's storm had been brewing in the work of Busnoys, Obrecht, and many

[2] Charles Burney, *A General History of Music*, ed. Frank Mercer (New York: Dover Publications, 1957), vol. 1, p. 759.

Example 1



Example 1, continued

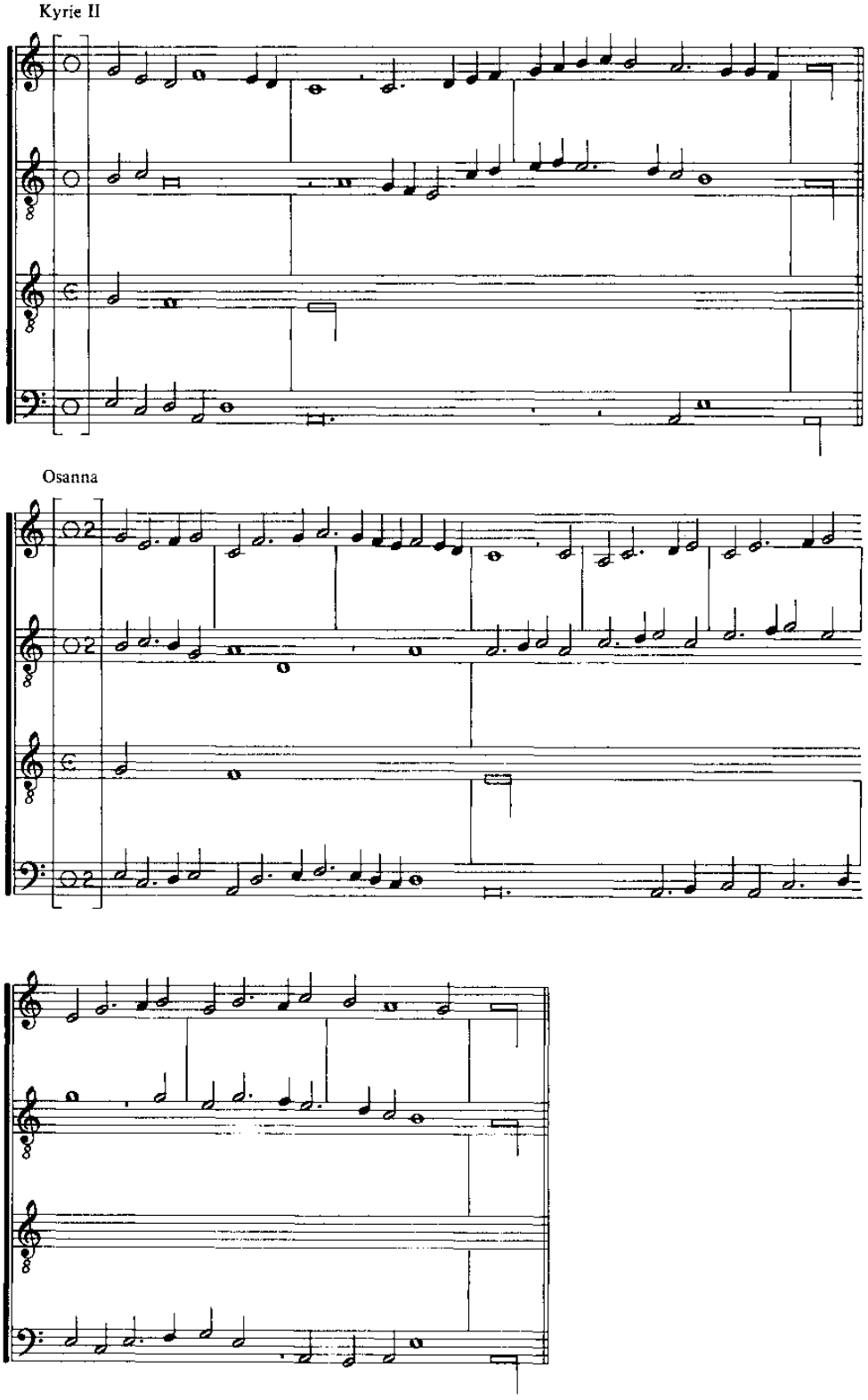


others. It is a rigid notion indeed of musical "structure," and one quite without historical support, that would admit to its purview consideration of rocks only, never mind the waves.

2) Doesn't it go without saying that all discussion of relative durations in fifteenth-century mensural practice involve relationships "on paper," whether or not they happened to be observed—here or there, now or then—"in practice?" The mensural system, after all, was a system of notation. And is it necessary to spell it out that no one's tastes or practical solutions—whether mine or Wegman's, whether Turner's or Tinctoris's, and whether we are talking about the King's Singers or the Duke's—are of the slightest value as evidence with regard to the symbolic structure of this or

Example 2

Obrecht, *Missa L'Homme armé*



any other work of the period? I am under no illusions that in an ideal performance the Pythagorean groundplan of Busnoys's *Missa L'Homme armé* would emerge with perfect clarity to the ear; for that very reason I would not hesitate as a performer to make whatever local compromises "comfort or apparent sense" might decree. For the sake of practicability Bruno Turner may have been justified—according to his tastes and in light of his singers's abilities and preconceptions—in equating ○2 with C. But his pragmatic decisions can lend Mr. Wegman's theoretical argument no logical support, nor have they any bearing on the actual "counting of durations."

On its own terms, moreover, the theoretical argument is tenuous. Beginning with Besseler and proceeding through Hamm, many scholars have attempted to solve problems of mensural relationships through "note profiles," meaning the statistical surveying (or, in Mr. Wegman's case, the averaging) of notated durations under various mensuration signatures. It has never been clear to me what such procedures were supposed to prove, and Mr. Wegman does little to clarify the matter, especially as he withholds his evidence (see his fn. 7). But nothing in his communication (aforementioned irrelevancies apart) casts any doubt on the fundamental assumption that a tenor cursus under a single mensuration sign must represent a single speed. That is the basis of my contention that ○2, succeeding ○ over an unchanging augmentation signature in the tenor, represents a duple proportion for "counting" purposes, regardless of how we may feel about dizzy prestissimos.

In advocating ○2 as preferable to C in the tenor tacet sections, Mr. Wegman appeals to the *lectio difficilior*. But while ○2 is in general a rarer signature than C, C is exceedingly rare for a *tenor tacet* movement. A scribe might well assume that a diminution signature was needed in place of the uncustomary integer valor; and, if the scribe knew his Busnoys, he could easily jump to the conclusion that the signature should be Busnoys's favorite, ○2. Although I was mistaken, as Mr. Wegman points out, in citing Obrecht's Mass as a confirmation of Chigi's reading (it was the result of using Wolf's ambiguously-notated edition; I was already aware of my error thanks to Thomas Noblitt's new one), the anonymous Mass Mr. Wegman has discovered does support Chigi (and me) and neutralizes the effects of my mistake. But please note that I did not (and do not) insist on "my" way of interpreting the mensuration signs as a bulwark to prop my Pythagorean edifice. On the contrary, the edifice was serendipitously revealed by a mensural interpretation I had assumed experimentally (as one of many tests to decide upon a primary source for the Mass) and with no premonition of discovery. On the importance of this distinction I do insist.

Before getting too deeply embroiled in a discussion of that edifice, brief comment is in order on three of the five consequences Mr. Wegman threatens should the edifice fail to withstand his onslaught, for they will be pertinent to the concluding discussion of his premises. I shall tick them off with his alphabet letters:

(a) Not all reasons for preferring Chigi to the Roman sources have to do with the mensuration signatures, as I somewhat casually remark in the article under discussion, but as I elaborate fully in the critical notes to my forthcoming edition.

(d) The case for Busnoys as author of the Naples *L'Homme armé* Masses was just as speculative after I completed my article as it was before I started it; Mr. Wegman is having his characteristic difficulty distinguishing facts from hypotheses.

(e) Sensitive to the circularity of using the position of the "Tu solus" in the Pythagorean scheme as evidence for an interpretation of its mensuration signature (since, as I point out in the article, it was only a certain interpretation of the mensuration signature that revealed the scheme), I made an effort to turn up corroborating passages in other compositions. I never used its length as evidence in the way Mr. Wegman implies; again, he has failed to attend to my process of discovery.

(3) Mr. Wegman's arguments against my interpretation of the unusual signatures in Busnoys's Mass all come down to a single one: simply that alternatives are possible. In every case he acknowledges the feasibility of my interpretation, but in some cases he professes to prefer others, although doing so sacrifices the Pythagorean groundplan. By the end he is ready to state with full confidence that the groundplan is a figment of my imagination. Clearly a great deal depends on the basis for his preferences.

It is here that we come to the fundamental conceptual and methodological disagreement between us. Mr. Wegman looks above all for authorities, whether in theoretical treatises or in other compositions, and is unwilling to arbitrate between conflicting ones. I feel it is important to be attentive as well to patterns of evidence, and, above all, never to abdicate the responsibility of choice.

This difference is most palpable in Wegman's footnote 9, where he cites a competing authority for the interpretation of the sign C 3, on which I base not only my hypothesis concerning the tempo of the "Tu solus" (crucial, in turn, for the integrity of the Pythagorean groundplan), but also my speculations concerning the authorship of the chanson *Il sera pour vous/Lome armé*. Wegman admits that Domarto is geographically and chronologically remote from Busnoys and his Mass; yet for him, apparently, any authority is ultimately as good as any other. Thus he finds the matter undecidable: "Since the sign is not vertically aligned here with another mensuration, who can say which interpretation [Busnoys] did adopt?" I can say, say I. And I say so precisely on the basis of that Pythagorean plan. When most of a pattern is independently known, the pattern itself (or rather, its completion) can become an arbiter of choice among otherwise plausible realizations of its components. This is no more circular a method than that of solving double crostics.

Since I happen to be writing this a couple of days before Christmas, and since double crostics may not be popular in Holland, another analogy occurs to me. Imagine a decorated Christmas tree with lights attached to the wall socket by a cord that has several switches in series. You do not know in what position each individual switch has to be in order to have the tree light up.

After some experiment the tree does light up, from which fact you conclude that all the individual switches are now in the right position. Now enter Mr. Wegman with two editions of the manual that came with the cord. Refusing to acknowledge the evidence of the lighted tree, he points out that the two manuals disagree on the settings, and (since we do not know which of the two came with this particular cord) we have no basis for deciding which of them is right. He goes out vowing not to return until he has found an incontrovertible and independent authority for the setting of each of the individual switches. Is this absurd? No more absurd than Mr. Wegman's refusal to allow the evidence of the "edifice" to decide whether the anonymous author of Naples Mass III or Domarto is the better authority for deciding on an interpretation of C 3 as it appears in Busnoys's "Tu solus."

By the time we reach his discussion of the "Confiteor," it is hard not to suspect that, disclaimers notwithstanding, he is determined after all to bring that edifice down by hook or crook; for the argument becomes impossibly strained. In the *Missa L'Homme armé* by Dufay he finds the same vertical alignment of signatures—except, of course, that they are not the same signatures (omitted are the strokes over whose interpretation, as Mr. Wegman must realize, rivers of ink have been spilled both then and now), and except that they are embedded within a larger context Mr. Wegman has deliberately ignored. As everyone knows who knows Dufay's Mass, there are two additional mensurations running concurrently in that unique and famous mensural pile-up at "genitum non factum," including one (coloration within O) that is so anomalous in its realization that Charles Hamm (as quoted in my original article) assumed it had to be a corruption. All of this must certainly (and uniquely) affect the way Dufay's mensuration signatures must be interpreted.

But Mr. Wegman's argument is not only specious; it is wrong. As I was careful to point out in my article, Dufay never used the major-prolation signatures to denote augmented note-values. This simply eliminates him from consideration as a precedent for composers, like Busnoys and Ockeghem, who did adopt that usage from earlier English practice. Mr. Wegman is saying in effect that *if* Dufay had put a stroke through the signature C *as he understood it* (i.e., as equivalent to O at the levels of the minim and the breve, but not the semibreve), then he would have arrived at a speed double that of the *integer valor* of the sign. And, he argues further, we are to construe Busnoys's usage as if it were a realization of Dufay's potential.

This is sheer fantasy. From what we know of their work and its transmission we are bound to conclude that Busnoys was reared according to a different mensural propaedeutic from Dufay, and that the sign C had a different primary meaning for him than it had for his older contemporary. Busnoys used the sign (I emphasize) only in tenors, and for him it automatically denoted augmentation. Stroking it was the only means at his disposal to indicate unambiguously the tempo Dufay's ordinary usage took for granted.

Quite the most farfetched aspect of Mr. Wegman's presentation is the way he insists on the Dufay as a precedent for the Busnoys (what else can it mean to say that "Busnoys went one step further"?) even as he acknowledges that Dufay's *Missa L'Homme armé* may not satisfy the first requirement for a

precedent: viz., chronological priority. He creates a singular category for Dufay's mensural usage in relation to Busnoys's—"the intermediate logical step between perfect tempus and the signatures of Busnoys's 'Confiteor' " —yet it will be apparent to every reader that the argument here appeals not to logic but, as always, to authority. Indeed it is the height of illogic for the same man who argues against my "dizzy prestissimos" on what he calls "empirical" grounds to turn around and advocate (on "theoretical" ones) what I guess we must call a vertiginous velocississimo, fully twice as fast.

Nor am I much impressed with an argument that envisions Busnoys content with total conformity in the length of all the final sections of his liturgical units except one. It is not just that our Christmas tree has failed to light up; "four out of five ain't bad" simply is no viable fifteenth-century attitude toward composition when one considers the intellectual traditions and the genres out of which the cyclic Mass Ordinary emerged. What irks me in Mr. Wegman's presentation is his readiness to see Busnoys diminished as a composer and his Mass diminished as a composition for the sake of an arbitrary—and, when all is said and done, a sterile—methodological purism.

While I am the first to agree that further study and discussion of fifteenth-century music is urgently needed, and the more the better, I deplore the implications of Mr. Wegman's concluding homily. If we must learn all the facts before we can even begin to assemble them into hypotheses, we might as well give up on the project right now; for there can never be an end to facts, and Mr. Wegman's "fundamental questions" will never be answerable to his exigent satisfaction. Ultimately, it is plain hostility to hypothesis that appears to motivate my critic. This would at least explain his seeming inability to distinguish hypotheses, whether mine or his own, from facts. Facts are what interest him. He collects and displays them lovingly (as in footnotes 9 and 10), vigilant against any propensity to discriminate among them or to sully them with use. But these are precisely the acts that turn facts into history.

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To the Editor of the JOURNAL:

IT HAS COME TO MY ATTENTION that there is an omission in my article in this JOURNAL 36 (1983). In my discussion of several settings of "Ecce quam bonum" I stated that the appearance of a popular-sounding tune with possible Savonarolan overtones had escaped notice (p. 428). In fact, Norbert Böker-Heil had discussed the similarity of the tune in Verdelot's "Letamini in Domino" and in "Ecce quam bonum," a motet attributed to Gombert in the MS Kassel 24. See "Die Motteten von Philippe Verdelot" (Inaug. diss., Johann Wolfgang Goethe-Universität, Frankfurt am Main, 1967), p. 74. In the original draft of the article I had taken note of Professor Böker-Heil's remarks regarding the Kassel motet, but in the course of revising the manuscript for publication I omitted the reference to him. Additionally, it escaped my notice that Professor Böker-Heil, in his "Nachträge" (p. 411),