
Hildegard von Bingen (1089–1179) was not just an important abbess, the founder of a convent, a mystic and visionary, a writer of letters, theological tracts and exegetical discourses, the author of numerous writings on medical and scientific questions, a diplomat and counsellor to high-ranking people of her time. Dubbed ‘the Sybil of the Rhine’ by her contemporaries and calling herself ‘a tone from the *tuba* of God’, she was also one of the few medieval women who composed music. Some scholars put her texts on the same level as the sequences of Notker, the poems and treatises of Walter of Châtillon, the hymns of Peter Abelard and the sequences of Adam of St Victor; her stock of monophonic settings known as *Symphonia harmonie celestium revelationum* is regarded by many as the most comprehensive musical work that can be attributed to any named person in the twelfth century.

But here the problems start. In recent years Hildegard has been pushed to public attention, mainly as a medieval witness to modern ecological, esoteric or alternative trends, but also as the originator of an especially individual corpus of music – ‘the visionary who is also an artist’. In countless books her music is being described as the product of a highly original thinker, turning her into ‘our’ woman in the twelfth century and presenting her as the missing link between the past and a politically correct view of Western music history. But legends more than facts are at the heart of this image of a twelfth-century compositional mentality. The legends involve notions of her compositional fame during her own lifetime, commissions by cloisters in her neighbourhood, and a special liturgy she and her religious sisters were said to have performed on certain occasions. Given that these legends contain some kernels of truth, it remains mysterious that her music did not enter the musical mainstream of her time, and that it is preserved in so few sources. The question of why the literary
and musical reception of her life and work faded relatively quickly still awaits an answer.

From what has been said so far, it is clear why most medievalists have been cautious about diving into Hildegard research. This highly ideological topic seems to be a minefield with little of substance to latch on to. The risks of being pushed, as a scholar, into ‘the esoteric corner’ are high. And there has seemed to be little interest in filling in the gaps in our knowledge with fragments of truth about her historical presence.

In this regard, 1998 was a watershed year in Hildegard research, especially for musicology. A conference, directed by Wulf Arlt, was held in Bingen, contributing for the first time important facts about the corpus of chants and about her liturgical drama, the Ordo Virtutum, while proving untenable certain notions which are now shown to be legendary and devoid of historical proof. We will have to wait for the conference proceedings to appear before moving on from here, but in the meantime, and also in 1998, one of the most important testimonials to Hildegard was made available: the present facsimile of the musical pages of the so-called ‘Rupertsberger Riesencodex’, edited by Lorenz Welker. The musical portion of the only other manuscript that preserves Hildegard’s music, Dendermonde, Abbey of St Peter and Paul, Ms. Cod. 9, has been available in facsimile since 1991 (Peter van Poucke, ed., Hildegard von Bingen: Symphonia harmoniae caelestium revelationum – Dendermonde St. Pieters & Paulusabdij Ms. Cod. 9 [Peer, 1991]). A reproduction of the musical portion of the ‘Riesencodex’ had already appeared in 1913, but this publication is largely inaccessible today, and it does not in any way meet contemporary standards for facsimile editions (Joseph Gmelch, ed., Die Kompositionen der heiligen Hildegard – Nach dem großen Hildegardkodex in Wiesbaden phototypisch veröffentlicht [Düsseldorf, n.d.]). The present edition, in contrast, represents the finest craftsmanship, and is a joy to use.

Even more praiseworthy is the contribution to Hildegard research made by the excellent commentary of Michael Klaper, a former Ph.D. student of the late Fritz Reckow. His commentary is built on a critical re-reading of all the available source material for Hildegard, on codicological and palaeographic research and on a critical examination of the published secondary material. His commentary is divided into several parts, which are presented in both German and English: ‘The Manuscript’, ‘Chronology and Genesis’, ‘Music and Liturgy’, ‘Concerning Matters of Genre’, ‘Other Sources of Dissemination, Variants, Readings, Notation’. Five helpful tables present the contents of the ‘Riesencodex’ as a whole and those of the musical section in more detail, list the antiphons contained in the ‘Riesencodex’, compare the order of chants with that found in the Dendermonde manuscript, and list the chants of Hildegard contained in the epistolary preserved in the manuscript Wiesbaden, Hessische Landesbibliothek, Hs. 2, fols. 404rb-407va, and their concordances in other sources, including Stuttgart, Württembergische Landesbibliothek, Codex theol. Phil. 4° 253 and Vienna, Österreichische Nationalbibliothek, Codex 963. In an appendix the sequence O Ierusalem is presented in a critical edition and translation. A glossary of central terms is provided,
and a selective bibliography of current literature, separating editions and research reports from more secondary publications, rounds off the book. The bibliography lacks only one important item, another 1998 publication, the bibliography on Hildegard edited by Marc-Aeilko Aris, Michael Embach et al., Hildegard von Bingen. Internationale wissenschaftliche Bibliographie, Quellen und Abhandlungen zur mittelrheinischen Kirchengeschichte 84. Finally, two musical examples, the sequence De Sancto Ruperto and the responsory De confessoribus, are presented using unstemmed noteheads and the treble clef transposed an octave lower.

A wealth of information is presented for most of the important questions concerning Hildegard’s chants and the Ordo Virtutum. The commentary is guided by caution, not diving into speculation or jumping to quick conclusions. It is a refreshingly calm compilation of all the facts one can possibly know at this moment, paving the way for conclusions to be drawn in the future. For example, the term ‘symphonia harmonie celestium revelationum’, used at the beginning of her Liber vitae meritorum, is not employed as a title in the manuscripts, Klaper stresses, and – a critical reading of the context – does not necessarily refer to her chants or prove that they were written between 1151 and 1158, as the datings of the Liber vitae meritorum and other writings have suggested to scholars in the past. Klaper leaves the question of dating open, as he does the issue of the liturgical use of the chants, scrutinizing the evidence in the text of Guibert von Gembloux, Hildegard’s secretary, and analysing her antiphon repertory, specifically with regard to psalm-tone cadences. From this the possibility of a liturgical use of the chants emerges, though it still remains to be proven. Similarly, questions of compositional procedure, including the question of whether Hildegard did compose or write the music herself, is wisely left open for further investigation. The facts presented in the commentary, and the facsimile edition itself, leave ample space for conclusions and present enough material for future research. The research in this book is a start, not an ending.

One critical question must be asked, however; it pertains to the title of the edition itself. The term ‘Lieder’ was already used in 1922 in conjunction with Hildegard (Ludwig Bronarski, Die Lieder der heiligen Hildegard – Ein Beitrag zur Geschichte der geistlichen Musik des Mittelalters [Leipzig, 1922]; edited by Peter Wagner in his Veröffentlichungen der Gregorianischen Akademie zu Fribourg 9, repr. 1992) in the only lengthy analytical study prior to the publications of Marianne Richert Pfau (among which her dissertation, Hildegard von Bingen’s [sic] ‘Symphonia harmonie celestium revelationum’ – An Analysis of Musical Process, Modality, and Text-Music Relations, published in 1990, is outstanding). The 1969 edition of the chants in transcription, by the Benedictine nuns Pudentiana Barth and Immaculata Ritscher along with Joseph Schmidt-Görg, takes up this term again, but apart from that, all serious Hildegard scholarship relating to her music avoids it altogether. Although ‘Lied’ can and has been applied to all kinds of vocal music for nearly a millennium it is used today primarily to allude to the repertory produced by mainly German composers during the Romantic era, and, generally speaking, it evokes secular music. It is misleading to apply this term
to medieval chant – especially these days, since the title could unwittingly push
the edition into a corner in which it would not want to find itself, namely with
those publications trying to support the claim that Hildegard was a modern
woman, composing ‘modern music’ with a modern meaning for us today, ignoring
the fact that she was after all a Benedictine abbess. In a field which is so
ideologically charged and filled with fantastic projections and images, it would
have been wise to use the title to separate this excellent edition from so great
a mass of inaccuracy, especially given the fact that the term ‘Lieder’ is not used
once in Michael Klaper’s introduction. A correct term could have been ‘Gesänge’,
a term open enough to include liturgical and non-liturgical music alike, even
open enough to include her Ordo Virtutum.

The reasons for the peculiar title remain unclear, and one would not want to
suppose that the editor, at any rate, was guided by a search for profits. Indeed,
this facsimile edition is again proof that a book can be much better than its
title – and ‘much better’ is a considerable underestimate in this case: the edition
presents a level of scholarship that is unlikely to be undercut in the future,
when the legends fluttering about Hildegard von Bingen will have either been
given roots or will be bursting like soap bubbles. Together with the forthcoming
proceedings of the 1998 Bingen conference, there has never been a better starting
point for future research on Hildegard’s music.

ANNETTE KREUTZIGER-HERR

Guillaume de Machaut. *Le Livre dou Voir Dit (The Book of the True Poem)*. Edited
by Daniel Leech-Wilkinson; translated by R. Barton Palmer. Garland Library of
Medieval Literature 06A; Garland Reference Library of the Humanities 1732.
1327 6.

Four years ago, Lawrence Earp remarked that, ‘At present, then, the Voir Dit is
the only work of Machaut lacking a critical edition’ (*Guillaume de Machaut: A
Wilkinson and R. Barton Palmer’s volume represents the publication of one of
the two editions mentioned by Earp as forthcoming, Machaut scholars every-
where can now heave a collective sigh of relief. (The other edition, Jacqueline
Cerquiglini-Toulet’s completion of Paul Imb’s edition and translation, appeared
in 1999 as Guillaume de Machaut, *Le Livre du Voir Dit [Le Dit véridique].*) Unlike
the other dits which Palmer has translated, the Voir Dit edition is preceded
only by Paulin Paris’s partial and inaccurate 1875 edition (to which helpful
cross-references are nevertheless included in the index of the present book). This
volume, then, is newly edited from the original manuscript sources, and incorpor-
ates aspects of the original, including its orthography, capitalization (which, cru-
ially, segments the text for a medieval reader), and miniatures. It includes no
musical notation; instead readers are referred to already existing musical editions.
The introduction is thorough and generally excellent. Its most interesting and stimulating discussion is contained in a section innocuously entitled ‘Artistic Achievement’. Here, the editors engage with the issue that has been central to research on the *Voir Dit* for at least the past century – the question of its truth. They trace the modern reception history of the poem, which has polarised into those on the one hand who believe in the poem’s truth, think that there is a real ‘toute belle’ who writes (at least some of) her own poems and with whom the sexagenarian Machaut really did have a May-to-December love affair, and on the other hand those who think that it is a ‘ludic fiction’ (p. xxiv) written entirely by Machaut using a patina of reality to foreground issues of textuality. The editors use Robert Sturges’s division of the *Voir Dit*’s modern reception history into three broad periods, ‘the scholarly, the critical, and the metacritical’ (p. xxi). The current metacritical phase focuses on the cultural context for the production of such a text and its reception by its original audience. The editors argue that some of the more elaborate recent interpretations have based much of their understanding of the work on this presumed fictionality, placing emphasis on the play with a notion of truth, such that the textuality of the work – the way in which it chronicles its own composition – is seen as its most important feature. The editors point out, however, that the original scholarly period, which ought to have witnessed the establishment of a reliable text for the work, was never properly completed, so that all subsequent analyses are hamstrung by inaccurately mediated access to the work itself.

When Georg Hanf first questioned the truth of the *Voir Dit* in 1898, his argument centred on the interpolated letters representing an exchange between poet and beloved, which, he believed, were clearly misordered and could not be assembled into a correct sequence. The present editors, however, maintain that the misordering of the letters is a good indication that they existed as real correspondence before the composition of the poem. A putative original order may be reconstructed, which Leech-Wilkinson arrived at independently of Walter Eichelberg’s similar 1935 solution, devised to counter Hanf (Leech-Wilkinson, ‘*Le Voir Dit*: A Reconstruction and a Guide for Musicians’, *Plainsong and Medieval Music*, 2 [1993], 103–40). Not only do all three manuscript sources ‘misorder’ the letters in exactly the same way, but the narrative into which they are embedded is formed around, and tries to smooth over, this incorrect order. Machaut admits to problems in reconstructing the correct order in a letter of 8 August 1368. According to his own epistolary testimony he is able to write 100 lines a day. At this rate, by 8 August he would have reached up to about line 1200 in his composition of the *dit*, the very point at which the misordered letters are found (see pp. xxviii–xxix). The editors magisterially set out these complexities, pointing out the high degree of internal chronological consistency. Of the two possible conclusions – that the letters and the reason for the *dit*’s composition are real, or that the work is an incredibly complex artistic construction masquerading as truth – only the former, the editors maintain, is believable. They also argue that the misdating of some letters bears witness to the *Voir Dit*’s verity, since were
the letters to have been written as the narrative was being composed (rather than representing a pre-extant reality around which it was subsequently constructed), the author would have got the dates ‘right’ to increase their verisimilitude.

The letters have been edited by the author for the purpose of anthologizing, the lyrics have been added to by Machaut, and the narrative embellished with the ‘subtives fictions’ he promises his lady. Nevertheless, the way in which the misordering fits exactly with the point of composition that Machaut would have reached (according to his own testimony) at the place which he complains of just such a problem, is convincing evidence that the letters preceded the poem. The editors’ argument is well made, and it required a great deal of detective work, logical thought and inspiration, for which they are to be congratulated.

In terms of the layout and presentation of the poem itself, this volume is not just another modern text edition, a transcription marked up in line with modern standards. It is presented in a format similar to the original manuscript, complete with the orthography of the base text (MS A – the closest to Machaut, and arguably the earliest of the three main copies for the poem), including its capitalization, its programme of miniatures and its absence of accents, elision markings and punctuation. (Cerquiglini-Toulet’s edition is based on the other ‘good text’, MS F. Aside from orthographical variants, the two editions are quite similar, since whenever sense is clearly lacking in either base manuscript, a reading from the other is generally used.) The editors modernize only those aspects of the original which are ‘shaped mainly by traditions of design or constraints of space in the manuscripts’ (p. xcvi). Thus, they do not separate spatially the initial letter of each line, they expand manuscript abbreviations, and they separate words. The only orthographical concession is that although the letter forms i and j are as in the source, v and u are ‘reluctantly’ modernized in line with modern usage since they are more frequently used and would give ‘too many problems for too many potential readers’ (p. xcvii).

The overall effect of the presentation, as the editors claim, is to approach the experience of reading the original. This may appear curious – why should the text not be modernized in all those aspects which, after all, do not affect the words themselves and their meanings? Whilst the editors acknowledge that Middle French and modern French have traditionally been thought similar enough for such modernization not to impinge upon interpretation, they argue strongly that the way in which Machaut’s poetry was set down was inseparable for Machaut from the poetry itself. This is perhaps the place in the book where an influence from musicology is most discernible. Although Leech-Wilkinson does not draw the comparison here, the need to base analytical interpretation on a clearer picture of a manuscript original and the circumstances of its production and reception has been of increasing importance in the making of recent musical editions. Medieval musicologists now readily recognize that, even when they are trying to reproduce as many features of the original as may be relevant, editions are essentially translations (see Margaret Bent, ‘Editing Early Music: The
Dilemma of Translation’, *Early Music*, 22 [1994], 373–92). It seems clear that it is necessary to understand medieval notation in its own terms and not as a partially successful attempt to express something better expressed in modern notation. Since both pitch and rhythm are to some degree only fixed contextually by singers, face-value transcriptions of the notation in which identical graphemes are reproduced in the new (modern) context as if they had the same meaning in fact distort the original in several respects. This seems analogous to a rejection of simply imagining that Middle French may be read, punctuated, and marked up, as if it were modern French, without any loss. This is a rare and welcome example of musicology having something to offer another discipline, countering its normal Johnny-come-lately adoption of ‘-isms’ from history or literary studies.

In terms of the editing of Machaut’s poetry, presenting original spelling and not adding punctuation, accents and elision signs allow the reader to perceive the frequent formal play with homographic constructions in which Machaut indulges. Although the editors speak merely of ambiguities, ‘for instance noir, which context will reveal as either noir or n’oir’ (p. xcvi), such ambiguity is meaningfully exploited by Machaut when graphically identical but contextually dissimilar words are paralleled in rhymes. The lack of punctuation forces a close engagement with the text and allows Machaut to set up expectations which can be frustrated; the understanding of a sentence may only become clear retrospectively. This is akin to the lack of accidental hexachordal signs at cadences, compared to the fully prescriptive notation of a later period. Singers are forced to construe the sense of their line in terms of its underlying counterpoint (i.e., in terms of its aural context with the tenor) and place semitones accordingly. (The comparison between counterpoint and grammar, and between cadences and punctuation, has been drawn in Margaret Bent, ‘The Grammar of Early Music: Preconditions for Analysis’, in *Tonal Structures in Early Music*, ed Cristle Collins Judd [New York and London, 1998].) Machaut exploits this kind of contrapuntal punctuation as much as he does the punctuation in his poetic texts – in both cases things which appear identical in the manuscripts are realized differently. Some critics, notably Leonard W. Johnson, have explored the idea of play with themes, rhymes, sounds and so forth (*Poets as Players: Theme and Variation in Late Medieval French Poetry* [Stanford, 1990]). It can now be seen that Machaut’s play extends to visual and syntactical punning, accessible to the modern reader only when these aspects of the manuscript originals are reflected in the edition. The segmentation of the text by capitalization (with different sizes of capitals reflecting the relative ‘weight’ of an articulation) and by the use of miniatures is vital to an understanding of how the text is punctuated at the level of shorter and longer sections, respectively. This approach neatly extends the current interest in the cultural context of a work’s production and reception, since it gives pointers to how the text was (intended to be) read and an indication of the scope for readerly interpretation. Without these aspects of the original, interpretation arguably represents an analysis of a text that is, in terms of punctuation, especially, already at one further interpretative remove from the poet’s original and fixed into one particular reading.
The translation in prose, although arranged in lines reflecting those of the French text, provides a readable, utilitarian and helpful adjunct to Machaut’s original, and is particularly useful for readers who have previously read Middle French in editions which modernize in line with modern French norms. It is therefore frustrating when it occasionally uses obscure or archaic English words, such as ‘nary’, ‘sorbier’, or ‘disport’. Sometimes this tendency results in the creation of noticeable repetition which is not found in Machaut’s original. For example, ‘fine fettle’ is used three times to translate three different things: ‘cointes et polis’ (l. 1171), ‘belement’ (l. 1723), and ‘en meilleur estat’ (Letter 28). Conversely, where Machaut does repeat himself word for word, the translation doesn’t. The phrase ‘prist touje la graisse dou pot’ occurs in both Letter 35 (on p. 440) and at l. 6481. For the word ‘graisse’ the translation gives ‘fat’ in the former and ‘grease’ in the latter. This example of Machaut’s exact repetition is potentially significant, since it supports the idea that the letters existed prior to their narrative frame and provided material for the poetry which was subsequently built around them.

A few translation errors may be noted. Some are typographical: there is a ‘not’ missing from Letter 29, on p. 389, where the sentence should read ‘But the more I was witness to good times and joy, the more I was displeased because I remembered I could [not] see you or send messages’; and l. 5845 should read ‘I miss his company’ (not ‘I miss the company’), since the refrain changes in the text (the translation as it stands does not reflect this change, which is even mentioned in the introduction as an argument for the lady’s real, amateur status). Some are more outright errors: the numeral ‘iiii’ is translated as ‘three’ on p. 567; and l. 8333, ‘Son cuer de moy si tost retrait’ is given as ‘She took from me my heart so easily’ when the sense clearly requires ‘She took her heart back from me so easily’.

These are small points, and in no way do they detract from the achievement of the edition as a whole. For those working on Chaucer, Froissart, Christine de Pizan, or the contemporaries of Machaut, it provides a useful background source. For the scholar of Machaut’s poetry the acceptance of the correspondence as real will open up new avenues for cultural critique, in which the letters become an interesting insight into how the fictional art of courtly love interfaced with the language of ‘real’ love, and how both interacted with artistic creation amongst the lettered elite in the late Middle Ages. For musicologists it contains interesting (if sometimes obscure) information about a composer in a period from which relatively little personal testimony otherwise survives. Much of this was previously discussed by Leech-Wilkinson in his 1993 article, but the extensive commentary offers more gems, interpretations (some tentative) and directions for further enquiry. We can assume, in its reflection of Machaut’s compositional priorities and practices at least, that the Voir Dit represents a true story and thus an incomparable asset to our understanding of his musical output.

Elizabeth Eva Leach

When Professor Berger’s first articles on this subject were published, they were widely hailed as offering a much-needed clearing of the air, clarifying our understanding of theorists’ perceptions of the relationships between mensurations and proportions for the mid-Renaissance and earlier. They were reasonably expected to offer considerable guidance, not only to other scholars seeking to establish details of related issues, but also to performers struggling with questions of relative tempi.

In many respects, the book that followed, expanding on the articles, justified our hopes. It presented clear details of how the various signs were thought to relate to each other, and how different theorists (working in different places at different times) had differing views of these relationships. I was particularly pleased to see a serious discussion of the possibility of semibreve equivalence between signs, for there are musical sources where this provides the simplest explanation.

One measure of the success of the book has been the number of recent articles pursuing issues discussed there: Professor Berger has clearly stimulated new thinking. If those articles seem to be focused on relatively few issues, that is perhaps no bad thing, although I think that there is much else that still needs to be said. Indeed, there are, alongside the inevitable (relatively) small areas of disagreement, significant issues that await serious investigation, as well as areas that the book fails to address adequately.

My two main areas of regret are that the volume seemed to spend relatively little time looking at musical sources, concentrating on a few, principally from the fifteenth century (even while theorists are included for the next fifty years or so); and that the historical coverage of theory and music is so circumscribed, chronologically. Peter Lefferts, in a long and thoughtful review, felt that the book should perhaps have been titled so that we could see that it concentrated on the century 1450–1550, and on Italy and Germany. He is certainly right that the book does not fully discuss ‘origins’, and I feel equally that its author seems to see no ‘evolution’ after the early sixteenth century. Both are areas where we badly need, not merely a collection of the data, but also informed speculation.

The question of ‘origins’ particularly needs further research. Succinctly, I do not believe Berger’s argument that the signs O and C derive from the symbols used for the various levels on a Roman abacus, nor that the Roman treatment of fractions in computation led directly to the symbols used (and even perhaps the system behind them) in French fourteenth-century notations. Berger argues that the pattern of signs on an extant hand-abacus suggests the centrality of the circle, as representing single units, and that C (used to represent 100, of course) was presented in an adapted form for fractions of the unit. This may be true,

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but the link from this to the signs adopted in the fourteenth century seems highly suspect. For one thing, Roman calculations, like those anywhere involving an abacus, would be additive in concept, building from small units (involved in commercial or other transactions) to larger ones, or using other normal arithmetical practices. When fractions were involved, they could be of several types, depending only on a duodecimal arrangement. By contrast, the emerging ‘French’ system of notation (as distinguished from Trecento notation with its use of fractions) worked on a system of hierarchical divisions with limited possibilities, of slicing a unit into tightly specified smaller ones, and then working with them again. The distinction is evident in the manner in which (while Italians could cope with notations with flexible rhythmic potential) French thinking had to describe ‘alteration’ and ‘imperfection’ as ways of producing different solutions. The earliest mensuration signs do, it is true, come at a stage when this process has already reached more than one level, but this does not invalidate the mode of thought.

We need to know much more about medieval approaches to number and measurement, and to time and space, before we can argue as does Berger, or even speculate confidently about the roots of either system. We need more studies like the forthcoming volume by Dorit Tanay, looking into how the craze for measurement (even for speculations about measuring the inaccessible) was received by musical theorists of the time. Such research would surely show that speculative thought and its impact on music had very little to do with commercial arithmetic.

Coupled with this, it must be significant that the first mensuration signs to be generally accepted were apparently those for tempus, O and C (see Berger, p. 13). If this is indeed true, and if, at the same time, the signs for modus were much less standardized, we may have to assume that the tempus sign was the more immediately necessary, and the earliest to be widely used (while modus signs served a more theoretical function, or were only developed in specific places or for specific repertoires). Then, easily (and perhaps naively), we are faced with the relationship between a circle and perfection, and between a broken circle (C) and imperfection. This may be simplistic, but it helps to explain why circles or dots were used at various times for other levels of the mensural system. It is also more satisfactory than positing a connection with Roman symbols and then requiring a significant change in the interpretation of the symbol C vis-à-vis O.

A similar problem concerns Berger’s treatment of the emergence of a distinctive

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2 I am grateful to Professor Tanay for allowing me to see her work before publication.
3 On pp. 46–9 Berger cites Johannes Vetulus de Anagnia, who related the Roman uncia, or fraction, to tempus, apparently going on to describe the tempus as square (perhaps a breve) and divided into three. However, Vetulus, as an Italian theorist, is evidently heavily influenced by French theories, and it is easier to see him working with a combination of Italian thinking about fractions of a breve and French thinking about hierarchical relationships. See F. Alberto Gallo, ‘Die Notationslehre im 14. und 15. Jahrhundert’, Die mittelalterliche Lehre von der Mehrstimmigkeit, ed. Frieder Zaminer, Geschichte der Musiktheorie 5 (Darmstadt, 1964), 322–5.
Italian mensural practice in the Trecento. The French influence already evident in early treatises (such as that of Guido frater), coupled with the different character of others (describing rather than justifying notational systems), means that it is difficult to see any contemporary view of the reasons for Italian practice. Berger seems to regard this practice as something of a byway in her discussion — she is able to say, for example, that ‘Johannes Vetulus distinguished between a theoretical and a practical mensural system’ (in which the theoretical was French and the practical Italian), and that ‘Italian theorists adjusted their mensurations to those described in French treatises’ (p. 48). It is as if an international invasion and conquest rendered the history of the conquered of little interest. A more recent attempt to document the conquest, collecting and tabulating the various references by Italian writers to French practice, also has not addressed this issue. Since it was in Italy that accounting practice was developed, under the influence of Arab thinking and of commercial necessity, it might be profitable to explore how far Italian theory can be reconciled with this different approach to number in music.

If, then, I find Berger’s book unconvincing on the origin of the mensural signs themselves, there is no doubt that it is thorough and well documented about mensural practice, particularly once she reaches the fifteenth century. Before addressing some of the larger issues raised by her book, two trivial points need mentioning: (1) Berger writes throughout of ‘inverted C’, when she means ‘reversed C’. (See, for examples, pp. 33, 43 or 62 – where the Latin she quotes uses the word ‘converso’, ‘turned around’.) The first would be valueless, for it would be indistinguishable from the original sign. (2) Example 6.10 (b), on p. 189, erroneously presents a copy of the following example.

A more significant problem here does not concern our interpretation of theoretical writings, but rather an apparent confusion between the activities of composers and those of scribes, and between their practices and those described by the theorists. Berger suggests that composers used the signs in the way that theorists prescribe (p. 26). While, no doubt, this is often true, the implication seems to be that the surviving sources normally indicate what a composer wrote. A vast literature, inside and outside music, shows that this is only partly true: we do know that certain traits of English mid-fifteenth-century notation survive in copies of English pieces made by Continental scribes. On the other hand, scribes will often have adapted details of notation, including the patterns of using mensural

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5 Berger herself notes (p. 82) that the practice, argued for by Tinctoris and Gaffurius, of making consecutive proportions cumulative, probably derives from commercial arithmetic.

6 The extent to which composers and scribes actually did follow theoretical prescriptions, or how far theorists were trying to describe what ought to have happened, seems not to have concerned Berger very much. The codifying habits of Tinctoris, and the way that other theorists did or did not follow them, form central issues in her book, while practical sources are generally neglected. In what follows, I shall touch on this point more than once.
and proportion signs, to fit their own usage. Berger herself shows examples of this.\(^7\) It means that the extant sources are potentially very important for details of the picture that Berger draws more broadly, showing local practices and demonstrating where and when changes in practice actually took place.

I am not entirely happy with her assertion (pp. 89–90) that, by the early sixteenth century, C is virtually obsolete as a signature for all voices simultaneously.\(^8\) The statement might seem unexceptionable: there are relatively few appearances of either of the signs implying major prolation in sources dated after somewhere around 1500, and even fewer of either used in all voices. However, one certainly cannot say that the signs are obsolete. I do not mean the numerous references in sixteenth-century theorists: here, Berger is right in implying a distinction between practice and theory. But it is notable that the sign appears in places where we cannot argue that it has a purely archaising or historical implication. It appears in, for example, Palestrina’s first book of masses, alongside two occurrences of O in all voices, so that we have to assume fluency in its interpretation on the part of the singers using the printed copies, not only in Rome, but also elsewhere.\(^9\) It may be that this represents the last fling of the mensuration in Italian usage, for there are no later examples known to me.\(^10\)

Even more significantly, it can be found in English music throughout the century and later. It appears, for example, in Fayrfax’s music as transmitted in the Gonville and Caius books, in Tallis and Byrd’s Cantiones sacrae of 1575, in settings of Browning in Baldwin’s manuscript collection (GB-Lbl, RM.24.d.2), in Ravenscroft’s Pammelia of 1607 and Melismata of 1611, and even in Simpson’s The Division-Viol (the edition assigned to 1667).\(^11\) The last two of these are particularly

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\(^7\) See, for example, her comments on mensuration in Isaac’s Missa solenne (p. 26) and on the surviving mensuration signs for Ugolino’s music (p. 70). I have shown the same practice in Petrucci’s editions, and Ruth DeFord mentions similar cases of Heyden and Buechmaier transcribing works of Agricola, Isaac and Josquin with different mensuration signs and changed note-values. See DeFord, ‘Zacconi’s Theories of Tactus and Mensuration’, The Journal of Musicology, 14 (1996), 151–82.

\(^8\) Pp. 89–90, drawing on Arthur Mendel.

\(^9\) Curiously, there does not appear to be a continuous assumption in these pieces that major prolation implies augmentation. It does seem implicit in his Missa ecce sacerdos magnus, but we have no evidence for other works in the volume, which use the same note-values as surrounding sections, with the notational quirk that semiminims are void and flagged. See the discussion of cut signs, below.

\(^10\) Uwe Wolf, in Notation und Aufführungspraxis: Studien zum Wandel von Notenschrift und Notenbild in italienischen Musikdrucken der Jahre 1571–1630 (Berlin, 1992) is unable to cite any of the signs in Italian musical editions during those years. This is in line with Banchieri’s view, in his Cartella ovvero regole utilissime of 1601, where he says that ‘gli musici moderni quelli [dot mensurations] hanno dismessi, e per maggior docilità, gli hanno ridotti à due, l’uno diremo tempo perfetto maggiore; il secondo tempo perfetto minore’. I take this from Tevo, Il musico testore (Venice, 1705), who, despite the avowedly historical approach that he adopts, seems to have had much in common with other precursors of Berger. He says that ‘Grande confusione si scorge ne Scrittori antichi circa la variata significatione delli circoli, e semicircoli tagliati, e puntati…’ (p. 92).

\(^11\) We should add, while partially discounting them, a number of other appearances of both major prolation signs (among other complexities) in Baldwin’s commonplace Book (facs. edn. by Jessie Ann Owens, in Renaissance Music in Facsimile, 8 [New York: Garland, 1987]). Although most of these compositions are from the later sixteenth century, Baldwin had a clear academic interest in
interesting, for neither was aimed at the polyphonic singer, the sort of musician who might have been expected to have studied Morley’s _Plaine and Easie Introduction_, or some similar treatise giving all possible permutations of signs. The appearance of this mensuration in these sources must imply that practising musicians had retained an understanding of some sort of specific meaning for C, at least.

Yet, on the other hand, as Berger points out (p. 30), Heyden, writing in 1537, ‘essentially assumes that musicians no longer knew how to interpret perfect mensurations or _modus cum tempore_ signs’, i.e., signs much more generally still in use than C. Heyden is writing, of course, for a very different market from that represented by Palestrina’s audience, or (even more clearly) the destinations of English sources of the time. We have therefore to modify Berger’s statement, to assert that Heyden makes this assumption for his audience, and that it cannot be made for others.¹²

For Heyden’s audience, and indeed for those in other areas of the Continent, the assertion does seem to be true. A fairly widespread assumption that all notes were imperfect, unless otherwise instructed, appears to emerge by the middle of the century.¹³ It is evident in the extensive spread of ‘redundant’ points of addition, in the use of two breves instead of altered semibreve-plus-breve (sometimes explicated, especially in Germany, by coloration), in the increasing habit of annotating sources with small numerals showing the duration of ligatures and breves or longer notes, and the rare habit of inserting ‘bar-lines’ in individual parts.¹⁴ Indeed, the mode of thought and the lack of understanding that Heyden laments, were certainly present soon after 1500. (Their emergence may be a

what he was copying, and might well have preserved (and composed) a number of his examples simply because they included these notations. These pieces have been discussed in David Fallows, ‘The End of the _Ars subtilior_’, Basler Jahrbuch für historische Musikpraxis, 20 (1996), 21–40. I think that Fallows, in trying to relate Baldwin and his contemporaries to the works by Bedyngham and others copied into Baldwin’s book, ignores the evidence for a continuing interest in these signs, and for a completely different view of mensural and proportional practice, in England well into the seventeenth century. Baldwin’s examples probably do represent an interest in notational complexities of the _ars subtilior_ type, as Fallows suggests (and as would fit the view advanced by Anne Stone, in ‘Che cosa c’è più sottile riguardo l’ars subtilior?’ Rivista italiana di musicologia, 31 [1996], 3–31), but the other examples, as I say here, seem to suggest that the notation continued in use even in relatively simple musical styles. The English tradition may start from an attitude closer to Heyden’s position (cited by Berger, p. 113), whereby all note-values were binary unless a proportion sign was added. This certainly seems to work for examples in the Gonville and Caius versions.

¹² Again, Berger’s neglect of practical sources has produced problems in the application of her arguments. As Lefferts implied in his review, she neglects England and France in general; this is, of course, partly a reflection of the patterns of survival of theoretical, as opposed to practical sources.


¹⁴ In this respect, they are following the practice, remarked by Berger, whereby the late fifteenth century tended to use fewer perfect-mode rests, regardless of the mensuration sign.
belated corollary of the manners of composition outlined by Jessie Ann Owens, for writing in parts would be easier the fewer cases of perfection or imperfection there are.\textsuperscript{15} It is certainly likely that the changes in notation parallel the growth in the number of performing institutions trying to hire competent singers, and the apparent decline in rehearsal, both developments which preceded Heyden’s complaint by some decades.

This probably has much to do with what Berger sees as a transition from breve equality when transferring between signs, to minim equality, for the implication is a pattern of thinking in terms of minim movement, whatever the level of the tactus; this itself would then lead to thinking of the minim as the controlling factor in setting relationships or tempi. However, there are practical problems in thinking this way: there are still shifts between O and C, which would be redundant if there were a strict minim equivalence, and if (as occurs at the end of the fifteenth century when no cantus firmus is present) the music does not regularly require triple-metre sensitivity at the tempus level. Further, the decline of C does not argue for a minim equivalence: music could be written in the easier O, and there would be fewer instances of augmentation or imperfection for the singer to navigate. In practice, given a minim equivalence, both these changes would seem to be largely redundant, only providing for an easier notation to read. Both might suggest the presence of a semibreve equivalence, acting between O and C, and explaining the decline of C when not used for augmentation.

Parallel events are important here. Among them is the possibility, mentioned by Gaffurius (and discussed by Berger), that pieces in C did not need an introductory mensuration sign. Another is the use of C for pieces which are not really in imperfect tempus. It is well known that some frottole from the end of the fifteenth century have musical structures that fall into groupings of three minims, and the same can be said for a number of villancicos from the same period (others even seeming to be in five).\textsuperscript{16} The beginning of Josquin’s Christum ducem is another clear example of triple metre notated in C. These phenomena apparently reflect a major step in the evolution of mensuration signs – they are no longer used solely to indicate the hierarchy of durational relationships; nor do they always indicate some metric structure to the music itself. Instead, it seems to be an early step in their gradual conversion towards tempo indications, and ultimately something akin to our modern time signatures.

We have, therefore, to explain the retention of the perfect tempus O into the sixteenth century. I believe that the only possible answer is that the presence of O alongside C implies different tempos for the semibreve, with that of C being


\textsuperscript{16} Perhaps a reflection of the same loss of concern about the implications of mensuration for the grouping of notes is the number of works in which the composer himself seems to have lost his place in relation to the tactus, adding or losing one or two ‘beats’. See Theodore Karp, ‘Mensural Irregularities in La Rue’s Missa de Sancto Antonio’, Israel Studies in Musicology, 5 (1990), 81–95.
either faster or slower than that of O. This, rather than a sense of tradition, explains the continued use of different mensurations in, for example, the different sections of Kyrie or Sanctus settings well into the second half of the century, when the notation hardly ever requires any sense of perfection. (Which one is faster does not really matter for the present argument.)

This raises one of the most thorny issues touched on by Berger and followed up elsewhere: the question of what to do (in performance) when mensuration signs change in the course of a composition. When the changes are only in one voice, or are not simultaneous, the solution can easily be worked out, of course, and confirmed in performance. Indeed, this is one way to test the extent to which theorists’ prescriptions were applied. But when, as often, all voices change from one mensuration (or even proportion) to another, it is questionable whether the ‘rules’ have to be followed precisely. Recent discussion of issues raised by Berger’s book has centred on this issue, and has concerned the question whether the same sign may be interpreted in different ways, according to the visual (and therefore musical) context in which it appears.

Assertions that one symbol could have two very different (even contradictory) meanings, or that two distinct signs may have meant the same thing, assume completely different types of criteria.

In the first instance, where the single symbol is presumed to have different possible interpretations, the different readings may easily be acceptable if they inhabited completely different contexts, geographical or temporal. There are several such cases, of course, and they represent a normal historical process. More dangerous, and relevant here, are those cases where the two possible interpretations are thought to apply within the same document (or composer’s output, or performing institution): then we ought also to assume that the different interpretations were signalled to the performer. The musical context within which the symbol appears must have told the performer which interpretation to use, immediately and without any possible confusion (especially once performing from the book with little rehearsal became the normal practice). At the same time, the two interpretations should be evident to the modern scholar in some similar manner: if they are not, then we are indulging in wishful thinking, constructing different meanings to suit some other end of our own.

The most obvious example of this situation is raised in the discussions of changes in mensuration. We assume, confidently, that the relationship between consecutive signatures, or proportion signs, is not the same as that between simultaneous ones. Yet there are many cases where singers could not know for sure whether the change from C to O, for example, was actually occurring

17 In her Chapter 4, Berger points out some potential examples where confusion apparently results, and she argues that some changes in notational practice were developed to help performers who had been facing ambiguous situations.

18 Another example appears in the recent discussion by Margaret Bent of the early appearances of consecutive O and Φ (in ‘The Early Use of the Sign Φ’, Early Music, 24 (1996), 199–225). Rob Wegman is currently preparing a detailed discussion of this case.
simultaneously in all voices. This is even more evident in the use of the sign ‘3’, which may be simultaneous, may occur only in one voice as a series of triplets against other voices, may actually mean ‘tripla’ or ‘sesquialtera’, and may mean no more than a brief excursion into a triplet (although this last one is usually evident to both the singer and the modern scholar). Perhaps we can only assume changes in tempo that are less extreme than are theoretically approved when they occur where there is a bar-line, a *pausa*, or some similar sign in the source.

Yet, this problem, how to relate consecutive signatures, has occupied more of the ink of recent writings (since Berger) than any other. This is, of course, a compliment to the author’s work. It is a reflection of a laudable concern with ‘solving’ performance problems; but it is also a largely intractable issue, in which the rate of movement of the notated music (the preponderance of minims or semibreves) is assumed to have some connection with the speed of the pulse or tactus.¹⁹ This is obviously a specious argument, circular in intention, and derived from present-day aesthetic views. It is stated in the form of an equation between the change in mensuration sign, and any change in the range of note-values on either side. Thus, Rob Wegman has argued, for example, that the presence of the same note-values in two sections signed C and O₂ implies that the two sections moved at the same speed; and Margaret Bent has recently studied the mensurations and note-values in a number of works and similarly argued against a direct proportional significance for the two signs concerned.²⁰ This position seems to me to be suspect: the more that the note-values under a simple and a cut mensuration sign seem to be similar, the more we should assume that the function of the change in sign is to enforce a change in the rate of movement.²¹ Otherwise, there would be little reason for the change. But the absence of any visual change in the rate of movement of the music must mean that the new sign implies some audible change.

This has considerable bearing on the discussion of the use of cut signatures, particularly C, where there is no change from the hierarchic relationships between note-values from that found in C. On one hand are writers, primarily Professor Berger herself, who argue for a constant 2:1 relationship between C and C.²²

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²¹ If the values had been different before and after a change in sign, we would have no idea (from the notation) whether there was a significant change in audible tempo or not.

The argument derives from situations in which the two signs appear simultaneously, and in which one moves twice as fast as the other. It is assumed that this relationship should also apply to consecutive uses of the two signs.

Other writers have argued for a more flexible approach, in which the change of mensuration would involve a definite change in tempo, though one much more subtle than merely doubling (or halving) the duration of all notes. To a large extent, these arguments rely on musical criteria, the perceived ‘feel’ of the music, plausible speeds for the smallest notes, or the relative ranges of values in the two sections. As I say, these seem to me to be entirely subjective criteria, and there has seemed to be little prospect of a definitive solution.

However, Rob Wegman has produced an elegant argument, working against a direct 2:1 relationship between the two *tempus* signs and their cut alternatives.23 Pointing out that Tinctoris regarded the two cut signs as ‘acceleration of the measure’ and as lying outside the normal sequence of proportion signs, Wegman argues that Tinctoris was unable to define the extent of the acceleration. This, with attendant illustrations, seems persuasive: the cut signs do not correspond to duple proportions – although they might happen to do so in specific instances.

A second assertion, that two signs could have meant exactly the same thing, requires that we ought to assume that they operated in different geographical and/or temporal contexts. That is, it must be implausible that any one performing group (or composer) would want to have two such signs operating simultaneously: it would imply too high a level of redundancy. Indeed, it is usually possible to argue that two sets of signs that seem, to us, to have meant the same thing, did in fact carry subtly different instructions for the performer. Examples include ligatures as opposed to the standard notation, and *minor color* in void notation.

In the present context, the best example is the co-existence of the two signs C2 and Ç. As Berger shows in her Chapter 5, signs such as C2 or O3, *modus cum tempore* signs, seem to have gone through a gradual transition in meaning.24 But by the end of the fifteenth century they were largely being seen as diminution signs. This would certainly be true for C2, for its application did not involve any change from imperfect to perfect values. This sign then might have worked in the same way as would Ç: they both might have implied, theoretically, a simple diminution by half. If Wegman is and the relative insignificance of a German tradition for reduction to 2/3 in the case of Ç. The case is built almost entirely on evidence from theoretical writings. Yet, it is unfair to assume that Professor Berger is unaware of the possibility of a different solution, or unsympathetic to it. As early as p. 3 she admits that the problem of consecutive signs is more complicated than is implicit in the preceding sentence.

24 She makes the interesting point, on p. 156, that the invention of the various diminution signs provided the composer with increased control over musical metres. This control became less necessary – or at least required fewer sophisticated signs – as musical style changed in the later fifteenth century, so that detailed rhythmic counterpoint was replaced by harmonic and textural configurations. Tempo changes for the whole texture became more important, and the *modus cum tempore* signs adopted new meanings, while the new cut-mensuration signs were designed to cope with such relationships.
right, the performer would not have thought of them in this manner. He would have
seen the cut sign as requiring some (largely unspecified) acceleration, and the pro-
portion as requiring a specific proportional change. This becomes the best reason for
the survival of C2 in practical sources well into the sixteenth century. There are many
examples where C is used as well as C: one of the more interesting is that of Festa’s
Missae se congie prins, where C2 is used (although C would have worked just as well),
probably because the music is to be sung simultaneously with another voice notated
in C. On the other hand, the distinction was not always preserved: examples where C:
is used in one source and C2 in another for the same music, would include Regis’s
Missae ancilla domini and Brumel’s Magnificat II (‘Quia fecit’).

My final point concerns the way in which ‘evolution’ – as stated in Berger’s title –
seems to take no account of any changes to the system after the middle of the sixteenth
century (despite references to later theorists, most of whom are merely presenting
the same hierarchical and theoretical modal patterns). There is here no discussion of
the second most important stage of evolution, the emancipation of numerals, and the
strange sequence of stages that leads to the middle of the seventeenth century. This is
an area that badly needs extended study, of the depth that Berger has employed for
the preceding hundred years. The few studies of specific parts of the repertoire have
hardly begun to reveal more than details of what may be a general pattern: their
detailed conclusions have of necessity used as a basis the foundation that we already
have, and that is the system discussed by Berger as active during the middle of the
sixteenth century. Thus, Roger Bowers can assert that proportions in some Monte-
verdi works are to be interpreted according to the rules of late Renaissance music,
while in another article he can show where the same composer had problems with this
approach.25 Paul Brainard argues that proportion signs are cumulative, while Roland
Eberlein maintains that they relate to a fixed temporal unit.26 Similarly, Gordon Paine
argues that, for Praetorius, proportional signs do not actually follow their pro-
portional roots, but were intended to indicate tempo, while Margaret Murata suggests
that neither tactus nor proportion had fixed connotations of speed, at least for one
Italian theorist.27 None of these situations can be related to the examples of English
notation that I mentioned earlier. This sort of game of comparison and contrast is easy,
of course: I do it, not to belittle any of these scholars or their work, but to stress that
our understanding of the theory of mensuration and proportion for the hundred years
following the early sixteenth century is in the sort of preliminary stage that preceded
Berger’s work on the earlier period.

25 Roger Bowers, ‘Some Reflections upon Notation and Proportions in Monteverdi’s Mass and Vespers
of 1610’, Music and Letters, 73 (1992), 347–98; and ‘Proportional Notations in Monteverdi’s Orfeo’,
26 Paul Brainard, ‘Proportional Notation in the Music of Schütz and His Contemporaries’, Current
Musicology, 30 (1992), 21–46; Roland Eberlein, ‘Die Bedeutung der Proportionsangaben von
of Howard Swan, ed. Gordon Paine (Stuyvesant NY, 1988), 167–215; Margaret Murata, ‘Pier Francesco
Valentini on Tactus and Proportion’, in Frescobaldi Studies (Durham NC, 1987), 327–50. The nearest
approach to a synthetic view can be found in a number of older articles by Carl Dahlhaus, cited
(though not used, I think) in the present volume.
In expressing my two principal regrets (about the lack of discussion of enough music, and about the lack of a study of the ‘downhill evolution’ of mensuration and proportions), and in drawing attention to some problems, I have not wanted to suggest that Berger’s work is anything other than very valuable and stimulating. Subsequent writing has already demonstrated that, both in its extent and in the manner in which it has not been directed at destroying many of her conclusions. Further study of the music, of its provenance and that of its sources, will only refine many of her points about local and chronological change in theoretical thinking, demonstrating its connection with practical music.

Stanley Boorman


The subject of this book is the fragmentary manuscript, K3:H3 Düsseldorf, Universitäts- und Landesbibliothek, from the abbey of Werden, that preserves the oldest surviving copy of the ninth-century Scolica enchiriadis. The study is written with great enthusiasm and appreciation for the didactic methods of the treatise. Along the way, Torkewitz provides some useful new information on the abbey and on Hoger, its abbot from 892 or 898 to 906. This information suggests the possibility of a close relationship between the early history of the Enchiriadis treatises and Hoger, for a person of that name is indeed cited as author in the incipit of two early copies of these texts. However, the book also has some significant problems, which will undermine the scholarly acceptance of its content as a whole; a few examples will be given following a description of its contents.

Torkewitz’s study begins with a brief survey of the history and content of the Enchiriadis treatises. Unfortunately, the presentation of the content is often inaccurate. Then comes a description of the manuscript fragment from Werden and the text it contains, followed by adequate colour facsimiles of the complete fragment. Next there is an edition and translation of the text in the fragment, supplemented by text from other sources in order to situate the content of the fragment within the complete first part of the Scolica. This section concludes with an analysis of the content of the Werden fragment that at times approaches naïveté in its attempt to find meaning in every detail (see, for example, the discussion on p. 64 of the diagram on fol. 1r, or the description on p. 73 of the diagram on fol. 3r). The book concludes with a section describing scholarly life at the abbey at Werden, its close ties to Northumbria during the late eighth and ninth centuries, the possible existence there of a form of improvised parallel organum (a practice alleged to have existed in Iceland as well) and the contacts between Werden and other monastic centres (this discussion includes an excursus on the neumes from Essen). There is mention of the scriptorium at the abbey
and the contents of the library there. Torkewitz cites specific works of Boethius, Augustine and Cassiodorus that were contained in the library, but none of the writings he identifies are among those that are actually used as sources in the *Enchiriadis* treatises. Torkewitz believes that Hoger was the author of the treatises, but thinks that a student or colleague may have completed his work after Hoger’s death. These are just a few of the wide variety of topics that appear in the final section of his book, some barely mentioned, others discussed in detail.

Now, the problems. There is often a lack of clarity in the text; Torkewitz does not always provide a clear exposition of the facts to enable the reader to follow his train of thought. For example, he cites Valenciennes, Bibliothèque municipale, MS 337 and Cambridge, Corpus Christi College, MS 260 as two of the earliest sources that name an abbot Hoger as the author of the treatises. (These two manuscripts are related in other ways as well; for example, both of their incipits use some form of the word *excerptio* in the title.) But there are problems with the attribution to Hoger in both sources that Torkewitz fails to consider. In the case of the Valenciennes manuscript, the incipit appears twice, both times with a mention of Hoger. But the more conspicuous entry, on the first page of text, is not the important one, for it is inscribed by a much later hand, not earlier than the twelfth century, and thus has no significant bearing on the discussion of authorship. The incipit that Torkewitz is in fact describing is found on the formerly blank recto preceding the first text page, which occupies the verso of the same leaf. This recto evidently was left as a cover page, and its worn condition would indicate that it functioned in that capacity for some time. The *Enchiriadis* incipit with the attribution to Hoger that it contains is in a different hand from that of the treatise proper, and is immediately followed by additional matter that I believe to be in the same hand as the incipit, including the text of the *noane* formulas and even some neumes (!). It is not certain at what time this material was added, but what is clear is that none of it bears any direct relationship to the *Enchiriadis* treatises. In respect to the Cambridge manuscript, its incipit is not placed at the beginning of the *Enchiriadis* treatises, but rather several folios earlier, where it actually precedes an excerpt from Boethius. While none of these facts negates the Torkewitz thesis that Hoger was the author of the *Enchiriadis* texts, they should certainly have been set forth more clearly, especially on p. 114.

Torkewitz spends much time on the notation in the *Enchiriadis* treatises, but he makes statements about it that seem to indicate little knowledge of the manner of its use in the treatises or the inspiration for the notational symbols. In his general introduction he describes the content and notation of the two treatises as identical (‘verdoppelt’); however, the six pairs of examples on fols. 1v-2v of the Werden fragment, in the form of an inverted letter V, are quite exceptional in the treatises as a whole. They use a notational procedure not found anywhere else in the treatises: the symbols are perceived here as intervallic only, they do not represent a fixed sequence of pitches, as they do elsewhere. If the first and third symbols are used consecutively, for example, they do not represent the
equivalent of \( d \) and \( f \), but rather \( d \) and \( e \)-flat. Another significant difference between the notational usages in the two treatises appears in their deployment of the letters of the Roman alphabet in conjunction with musical examples. In the first treatise they are always used with \( A \) at the top followed by \( B \), etc., in descending order, thus as numerals rather than as a notation per se. In the second treatise they are used in ascending order with \( A \) at the bottom, and can thus be perceived as a true notation. (These are but a few of the many differences of content that I believe suggest the possibility of different authors for the *Musica enchiriadis* and the *Scolica*.)

In respect to the inspiration for the basic dasia symbol, Torkewitz ignores the influence of the notation in the *De institutione musica* of Boethius. To be sure, the word ‘dasia’ is not found in Boethius, but the configuration of the dasia symbol itself is: it is the \( T \) jacens (a \( T \) lying on its side). The most explicit evidence of the relationship between the two is found in the Hucbald *Musica*, where Hucbald uses only the Boethius symbols in his notation, albeit in simplified form, but identifies all of them with their Greek names except for the lowest symbol, the \( tau \) jacens, which he identifies as a *dasia rectum*. The word *rectum* is unnecessary in his exposition, for the dasia is upright. The dasia as a symbol and with this orthography is found in the Latin grammarians, among them Donatus, who also provided the model for the concept of a pair of treatises, one in expository form, the other in dialogue. Torkewitz, however, finds the origin of the *Enchiriadis* dasia in Isidore of Seville, in particular, in Isidore’s table of the Greek grammatical signs (reproduced from Isidore on p. 107). Weakening this hypothesis is the fact that there is no use made of Isidore elsewhere in these two treatises, and also the circumstance that there is not one single complete Greek word to be found in either of them, unusual for the Carolingian period, when it was fashionable to parade a bit of Greek. (Some copies do use the Greek letters for the dialogue, \( \Delta \) and \( \mathrm{M} \).) Thus for Torkewitz to use the Greek form and spelling of the word is not suitable in reference to the *Enchiriadis* treatises. The two symbols which are not derived from the dasia, the \( N \) and \( / \), provide some difficulty for Torkewitz, and he says they are apparently from the Greek/Classical notations (without making any reference to the Latin intermediary, Boethius). However, in the same Boethius notational table in which the dasia as a *tau jacens* is found, the \( N \) and \( / \) outline a semitone. In respect to the unusual shape of the reversed \( N \), Torkewitz resurrects the rune as a possible source (pp. 78 and 121). Moreover, this same Boethius table and its descriptive text provide the source and descriptive text of the inverted \( V \) diagrams on p. 71 of Torkewitz.

Torkewitz’s lack of familiarity with the sources used by the *Scolica* author is also evident in his exposition of the passages on *numeros caere* (rhythmic singing). Torkewitz finds that the symbols for longs and shorts are inspired by those in the Greek table from Isidore mentioned above. However, the true source is Augustine’s *De musica*, from which much of the terminology in this passage as well as the three-fold repetition of an example and the long and short symbols
are drawn. The Latin intermediaries in the transmission of Greek grammar are too often overlooked.

There are other problems, but just a few can be mentioned here. The translation of the title, *Scolica enchiriadis de musica*, as 'Die Scholien [sic] der Musica enchiriadis' (p. 35) is certainly incorrect; it is now universally accepted that Gerbert’s reading ‘scholia’ ('glosses') was wrong. This is but one more error that, like the theory involving the rune, seems to put us back into the nineteenth century. I suspect there are some citations from the scholarly literature taken out of context – at least, several references to my own work are. To mention two: On p. 67, n. 5, in respect to the manuscript from Tegernsee, I said just the opposite of what Torkewitz suggests. And on p. 78, n. 26, there is a quote out of context: I was pointing out that the choice of the two symbols N and / seems [sic] inconsistent when compared with the principles used to configure the other dasia symbols; but unlike Torkemitz I go on to say that when the Boethius table in *De institutione musica* IV 16 is examined, those exact symbols are used to outline a semitone, and the logic behind their choice becomes self-evident.

Overall, then, there are serious deficiencies of scholarship in this book, particularly with regard to the control of sources and research method. I do, however, also find many ideas of value in it, and I am now convinced that Hoger and Werden may indeed have had an important place in the history of the *Enchiridion* treatises. Hoger could have written the *Scolica*; the most important source for its name, the Fortunatianus *Rhetorica*, was indeed known in the region. But until I find a source with the same expository style as the *Musica enchiriadis*, the *Scolica*’s companion treatise, which never reveals its author’s personality, I cannot assume *Scolica* and *Musica* to have been written by the same author.

At the conclusion of this study (p. 121) appears a *speculatio* that I find enchanting. Torkewitz speculates that if Hoger is responsible for the treatises and the dasia notation, then many of the problems scholars have noted might be explained as a private ‘Spiel’ with the letter H of his name: the dasia as half H, the frequent reading of Ν as H, and the reverse N (rune-formed) as a half H. In addition, he points to the confusion between the letters Μ and Δ in the dialogue, proposing that there could even have been some play with the configuration of the M as well. I cannot agree, but this speculative bent betray a sensitivity towards, and love of the Carolingian era that I thoroughly understand. I feel the same passion for these treatises that is everywhere evident in his writing. However, I have known them for so many more years, since I first learned of them in the history of theory classes of Paul Hindemith that I took as a student at Yale, almost a half century ago. (Our newspaper for the early music performance group was even named *Commemoratio brevis.*) I believe that Torkewitz may add much to our knowledge of these treatises by the time another half-century has passed, and I wish him well.

Nancy Phillips