REVIEWS


These are heady days for the study of Greek music. When R. P. Winnington-Ingram published his still useful, indeed often brilliant, research into Greek musical theory and practice (largely before the Second World War), it was a subject very much reserved for a few consenting and obsessive adults.1 The technical requirements of the subject remain fiercely rebarbative – there are not many scholars who can claim to read Aristides Quintilianus with much fluency, pleasure or easy comprehension in Greek or English – and the tendency to retreat to abstruse and highly contentious topics of debate is still readily succumbed to. And delighted in. The fact that not a single note of ancient Greek music can be heard in wholly reliable reconstruction, despite the preservation of more than forty papyri of texts with musical annotation, has limited the attraction of the subject for mainstream musicologists and musicians. (M. L. West in what is the best-known recent study of ancient Greek music (see below) is, as ever, wholly convinced about his own correctness, and, for all their evident qualities, his many transcriptions may give the modern reader a misleading impression of certainty or of scholarly agreement.) The glory that was Greek music has flickered with a privilege and fascination at moments throughout Western culture, of course, most notably in

Renaissance Italy (where a few highly academic figures also rediscovered the lure of ancient theory, as has been ably discussed by Claude Palisca). So, too, Plato’s ethical understanding of music has repeatedly acted as a prooftext where cultural censorship and music have gone hand in hand. Yet where a serious philosophy department, say, could scarcely exist without a commitment to ancient texts (and a serious classics department could not survive without some philosophical expertise), which music department or indeed which classics course has ever felt a pressing need to make ancient music or ancient music theory a necessary requirement? This scholarly and cultural marginalisation is in striking contrast with ancient Greece itself, where Plato could sum up standard educational thinking with the catchphrase *achoireutos apaideutos*, which means – to give the full translation – ‘if you have not experienced the individual and collective training in music and dance that is provided by performing in one of the religious and civic choruses, you cannot count yourself to be educated or cultured’. Or, more sharply, ‘no choruses, no education’. *Mousikê* was an essential and integral element of ancient social practice, visible at all levels of social interaction from symposium to sacrifice, theatre to procession, schoolroom to spinning-wheels, and at all levels of social class from shepherd to the grandest private soirée. And so finally, remarkably, the last dozen years or so have witnessed the most extraordinary growth of interest in and publication on ancient Greek music, which has begun to reflect properly this central aspect of Greek life and learning.

Gentili and Pretagostini in 1988 edited a collection in Italy, *La musica in Grecia*, which contained articles by several European scholars who have gone on to open up the field (and Gentili and Perusino put together a further volume focused more closely on metre and music). This last volume is dedicated to Giovanni Comotti. His *Music in Greek and Roman Culture* (1989) is an expanded translation of his earlier Italian monograph. The per-

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ceived attraction of such a translation for an American public is immediate testimony to a sea-change in the status of the subject. In the same year, Maas and Snyder offered a fine, extended monograph on the stringed instruments of Greece.\(^5\) (It is fascinating that the standard – and extraordinary – catalogues of Greek vases by J. Beazley never distinguishes in its descriptions of images between different types of stringed instrument, but calls them all ‘lyre’.) Bernhard Zimmermann has produced a learned volume dedicated to the dithyramb (Dionysiac abandon, carefully reconstructed and dissected).\(^6\) Warren Anderson has offered a rather straightforward synoptic overview in his *Music and Musicians in Ancient Greece* (1994), as, more recently but with equally restricted cultural understanding, has John Landels with his *Music in Ancient Greece and Rome* (1999).\(^7\) Both books are aimed at a new market for ancient Greek music, the student and general reader. Two English scholars, Andrew Barker (*Greek Musical Writings*) and M. L. West (*Ancient Greek Music*) have done perhaps more than anyone to make the texts of ancient music theory and appreciation readily available, and to provide a solid and intelligent framework for the comprehension of that theory, and its relation to musical production.\(^8\) These books are undoubtedly the best starting point for any scholar seriously interested in the technical aspects of Greek music theory and production. And further important work on fifth-century BC culture and music from R. Wallace in America and from P. Wilson and P. Murray in England is eagerly anticipated;\(^9\) it is now clear that there is still much to be done on the politics and cultural valuation of musical performances from the great choruses of Greek tragedy in the midst of the civic rituals of Athens.

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to Xenophon’s shocking and amusing tale of Socrates dancing alone in his bedroom. Music formed a crucial thread of Greek cultural life, and although Aristides Quintilianus will not be finding his way onto too many student reading-lists, it is now possible to study Greek music in a way which was impossible even fifteen years ago. When Winnington-Ingram at the end of his career reviewed Mathiesen’s translation of Aristides Quintilianus in this yearbook in 1984, he had to frame his remarks with a sad recognition of the gaps in our knowledge and the lack of scholarly tools to help students of Greek music. Things look astonishingly different now. Heady days, indeed.

Such a rapid growth has inevitably opened to view the fissures in the subject. For the sake of rhetorical clarity, let me offer two opposed starting points, each something of an exaggeration of a trend in current thinking on ancient music history, and neither adopted in its most aggressive form by any particular scholar. Such a polarisation, however, will provide the sharpest indication of what is at stake in many more nuanced debates.

(1) ‘The Greeks invented music and music theory.’ This is the familiar and standard opening in traditional scholarship. While music, like rhetoric or politics, can be said to be always with us, the recognition and theorisation of the subject as a subject is a Greek achievement. Aristoxenus in the classical city, Ptolemy in Hellenistic culture, and Aristides Quintilianus in late antiquity provide high points of an unbroken tradition from Plato down to today. This tradition lays out the theory of modes, catalogues and theorises intervals in scales, and brings music and mathematics close together, as depictions of an ordered cosmos. The task of the historian is to establish and explicate the texts of music theory from the manuscripts that are transmitted to us; to explore the construction and sound of musical instruments; to transcribe the music notation; in short, to understand music as part of a continuous tradition. Greek music is a chapter of the history of music in the West, and stands as the *fons et origo* of what is still recognisably studied in the modern musical academy. The theorists are to be understood in relation to one another (and in relation to modern comprehension of harmonics, etc.). Although there are many representations and discussions of music in ancient Greece, the historian’s primary task is to construct the history of the grand-
est forms of musical composition and the most developed theoretical expositions.

(2) ‘The Greeks had no “music”.’ What is the ancient Greek for ‘music’? *Mousikê* – the science or practice of the Muses – includes many forms of literary and cultural production, and many forms of exercise. Singing is hard to separate from poetry and other forms of recital (‘Where burning Sappho loved and sang . . . ’). Instrumental playing is but one branch of *mousikê*: some instruments are reserved for slaves, some for gentlemen: to postulate ‘instrumental playing’ as a unified concept and to privilege it over other aspects of *mousikê* is to indulge in an act of cultural appropriation. *Choros*, for example – usually translated ‘chorus’, as I did above – necessarily involves dance and other rhythmic movements: singing and dance go together in ‘the choral tradition’ – as indeed do politics, state activity and the conflicts for status between men and families in the ancient city.10 It is the chorus as social and musical event that must be discussed if we are not to miss what really counts in the ‘choral tradition’. What is more, there is a fundamental disjunction between the world of performance and the world of theory. (The model of Bach or Beethoven studying, producing studies, playing studies is quite out of place here, hard though it is to escape from it.) Theorists are high-level intellectual magi, philosophers and scientists, often marginalised, and always competing for space in the intellectual marketplace. They may have little or no engagement with the world of performance. Aristides Quintilianus explains that he wrote his treatise because of the ‘contempt most people show for this subject’: he wants to reverse the ‘lack of respect’ in which scholars of music are held. This may sound like a familiar academic gripe – but it indicates rather the lack of any institutional framework or a ready-made intellectual environment to receive his work (and the marginal status of high-level theory). He knows he is writing for the (very) few, and takes some pleasure, I suspect, in his uses of obscurity. Ancient

music theory is not expected to result in musical compositions, nor does it influence performance styles. When Aristides Quintilianus assigns each of ‘the five Tetrachords in the Greater Perfect System’ to one of the five senses, or demands that the order of a musical system must be understood via the categories of gender, this is not just ‘sheer silliness’ (as Winnington-Ingram calls it),11 but the bold extension of a theoretical project concerned with the magic of numbers and the ethics of education. What is at stake in this music theory is the secrets and morals of world order. Music theory is not a guide to ancient music, but a hermetic project that grows from it. What is required is a cultural history of the ‘otherness’ of ancient ‘music’ and of its theory (which are very different things).

The tension between these two extreme positions will be familiar, mutatis mutandis, to those who work in other periods of music history, I expect. However, the very rapidity of the development of ancient music studies has resulted in far too little informed debate about the different types of question available and how they may interrelate. Technical knowledge of the treatises, the instruments, the mathematics are all necessary for sure, and hard-won aspects of the subject. But what is the cost when such technical awareness is utilised without a cultural anthropology or without a developed historical perspective? What are the misrecognitions that haunt a project where ancient music and its study are assimilated to ‘world music’?

Thomas Mathiesen’s huge new book is a telling monument to this moment where different approaches to ancient music fail to engage with each other in a serious and informed way. Mathiesen is best known for his sterling work in cataloguing the manuscripts of Greek music theory, and he has also produced a less well-received translation of Aristides Quintilianus (although the extensive introduction and notes to the translation are often cited and form the basis of a large part of section 6 of this book).12 This mas-

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Mathiesen, Apollo’s Lyre

disse volume, begun, he tells us, in 1979, and continued with extensive gaps over the next twenty years, runs to more than 800 pages (including 100 pages of bibliography). Its grand title and daunting scope should not be taken as a promise of a synoptic or even a grand vision, however. ‘No single discipline or method can claim sole authority in this field’, he declares piously in his preface, while he announces that his dominant interest will be in musical typologies, musical instruments and the medieval transmission of ancient theory. In this project, he seeks to distinguish himself from Martin West, Warren Anderson and John Landels, whose work he sees as ‘fundamentally different’ from his own, because they each write ‘from the perspective of the classicist’, whereas his is the project and viewpoint of the musicologist. Mathiesen wants to bring ancient Greek music under the sway of musicology. However, his understanding of ancient music and of modern musicology proceeds unruffled by cultural history, with barely a glance at the history of performance, with scant concern, indeed, for any history except his own most narrowly circumscribed account which tracks from theorist to theorist (as if it could be the same thing to write music theory in the classical city, the Hellenistic library of Alexandria, or the Christian world of late antiquity, and as if social and intellectual environments had no significant impact on the work of theory!). This is undoubtedly the longest and most detailed treatment of musical typologies available – but it is also the most extreme in the selectiveness of its viewpoint.

The book has seven sections (or ‘strings to its lyre’, as he coyly puts it). First, there is an introduction to the sources and methodology. Regrettably, I found this briefest of the sections to be rather perfunctory (especially as the opening to so committed and large-scale a project). The range of questions to be broached is inadequately considered and too swiftly justified, and, above all, the range of materials is ruthlessly circumscribed. That he dedicates a mere four pages to ‘literature’ – that is, to all of Greek and Graeco-Latin writing – indicates clearly enough that the representation and discussion of music as an active force in society is not his concern. As so often, the description of available sources and the account of methodology actively masks the tellingly unasked questions. From the outset, it is clear that this will be a very partial view of what can be said about ancient music. We cer-
tainly won’t be watching Socrates dancing, we won’t be wondering about god and music, or about the social and political concerns of ancient or modern music writers. The second section is entitled ‘Musical Life in Ancient Greece’. It is here where the poverty of cultural analysis is most damagingly obvious. He lists and briefly discusses some types of performance, some genres and some institutions. ‘Greek life’ seems to mean any place in the Western Mediterranean at any point over a span of 1,000 years. He does not care to distinguish between centuries. Proclus from the fifth century AD, a Neoplatonist in the Christian era, is used to gloss the idea of a ‘hymn’ exampled from Homer in the seventh (?) century BC! Aristides Quintilianus from the fourth century AD is offered as an authority on Plato’s discussion of nomoi from the fourth century BC. There is no attempt to see why fourth-century writers like Plato should have become so concerned with the ethical content and effects of music; there is no adequate treatment of such important crises as the celebrated battles over ‘the new music’ in the classical city. Dance receives scarcely a mention, nor do the different types of musical training. It is significant that Mathiesen does not appear to know Claude Calame’s absolutely standard work on choral performance,13 nor other studies of, say, Spartan performance, or lyric or tragic music and song,14 nor the history of changing educational curricula over the ancient world.15 This is an uninspired and uninspiring treatment of the role of music in Greek society, and it uses its narrow formalism to ignore almost every question about music as a cultural force. Drafted in 1979, completed in 1990, this chapter unwittingly demonstrates just how rapidly and just how far the field has moved in a short period.

The remaining sections seem much closer to Mathiesen’s heart and have more to them. Section 3 is on musical instruments (cat-

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egorised according to the modern schema of Hornbostel–Sachs), with largely sensible comments on construction, tuning and terminology. He does not hazard, however, any sense of the role of any of the instruments in Greek culture of any period. The aulos, for example, a double-reed wind instrument (often mistranslated as ‘flute’ in older books), has recently been superbly discussed by Peter Wilson (in *Performance Culture and Athenian Democracy*, ed. S. Goldhill and R. Osborne, to mark my *parti pris*). The aulos is used across Athenian institutions from every tragic festival to every sacrifice. It is much discussed in ancient literature and much represented. Wilson shows how an excellent technical knowledge of the instrument can be linked to a sophisticated understanding of its role in the discourse of the city. He analyses not just the different places the aulos is used and the different types of music it is used for, but also how those different performances are part of a system of thought, and how the aulos enters cultural debate – and becomes *bon à penser*, ‘good to think with’. While it is important to understand the guitar’s construction and stringing (to make an analogy), a musicology that discusses the guitar solely in such terms will not be adequate to comprehend the guitar in Spanish music or in rock and roll. What is worrying about Mathiesen’s project is not so much that he does focus exclusively on such a materialist account, but rather that he does not consider what difference a wider set of questions would make to his musicology, nor recognise the need to consider such systems of thought if the theorists he treats are to be placed properly within their intellectual milieu.

The fourth, fifth and sixth sections look at music theory, through the three greatest surviving exponents, and their lesser contemporaries, namely, Aristoxenus, Ptolemy and Aristides Quintilianus. (It is good also to see less well-known figures getting an airing here, such as Theon of Smyrna and Bacchius.) This is the heart of the book. In each case, after a brief introduction to the writer in question (though not to his intellectual or social environment), each theory is laid out, explained and explored, largely in its own terms. The Greek texts (with the exception of Aristides Quintilianus) are fragmentary; they are all fiendishly difficult to

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understand, contested by scholars at almost every turn, and even more difficult to explicate with any clarity or grace. It is undoubtedly the case that scholars in the field will have to read Mathiesen with care – few have spent as much time living with the details as he – and it is in these three sections of the book that its claims to attention most forcibly lie. Where West was content to offer one carefully controlled chapter on scales and modes, and Anderson relegated the whole thorny issue to a brief and rather apologetic appendix, Mathiesen has produced hundreds of pages of recuperation, interpretation and diagrams. It is not the best place to start your journey into Greek theory – Barker and West remain that – but for the advanced student there are many interesting and some rather provocative claims. He certainly thinks that Aristides Quintilianus is a better and more consistent and more intellectually satisfying writer than most other modern students of him have done.

This is not the place to argue over details (especially when the framework is still a matter of contest). But it is worth pointing out here that the engagement with the Greek texts can be rather misleading. First of all, the same marked phrases are occasionally translated in quite different ways, without adequately alerting the Greekless reader. So Aristoxenus’ definition of ‘pitch’ is given as ‘a certain hesitation and positioning of the voice’, but when Gaudentius uses the same words in his definition, they are translated as ‘a tarrying and standing of the voice’. This makes continuities and differences much harder to appreciate. (It probably should be added that the phrase may be difficult to translate, but neither ‘tarrying’ nor ‘hesitation’ is particularly apt for monê, which has an implication of ‘continuance’; and ‘standing’ is a strange term for stasis, and hard to construe in English: what is ‘the standing of a voice’? Barker renders ‘What we mean by pitch is something like the voice’s stability or standing still’, which has the benefit of being more accurate, more comprehensible and more reasonable English.)

Second, there is a certain flattening of the Greek to the point of mistranslation. Dionysius of Halicarnassus comments on the difference between old style dithyrambs and ‘the new music’, concluding, according to Mathiesen, ‘since among the ancients the dithyramb was orderly’. What Dionysius wrote, however, was ‘since among the ancients at any rate, even the dithyramb was orderly’.
Mathiesen has just left out Dionysius’ qualificatory terms [the *ge* and the *kai*], which, though small, matter. Dionysius is not just offering an objective description of how dithyrambs really were (what evidence could he have had for musical performances of hundreds of years earlier?), but constructing a typical rhetorical opposition between the good old days of the noble past and the corruption of more recent times. The dithyramb is a byword for over-the-top musical wildness, but in the old days, even such Dionysiac fervour had some self-control. Mathiesen doesn’t like to discuss such self-implication among his theorists – it would lead into a different set of questions about what theorists thought they were doing and who they were debating with – and consequently he lets his translation hide the problem.

Third, there are places where his English is incomprehensible without the Greek. Partly this is a consequence of over-literal translation. He specifically defends this, his version of a sentence from Aristoxenus, because it is ‘closer to the Greek’: ‘In general, it is necessary to look closely into the establishment of a principle, lest we fall into a foreign land by making a principle out of a certain sound or a motion of the air, and lest, in making a sharp turn, we lose many of our friends.’ Literalism is not translation’s best, or most graceful handmaiden. Barker translates ‘in general we must be very careful, as we set out, not to slip into extraneous territory by beginning from a conception of sound as a movement of air, and equally not to turn back too soon and leave out many things which belong to the subject’. ‘Lose many of our friends’ simply doesn’t mean ‘leave out many germane topics’ (which is what the Greek means). (It will also be noted that Mathiesen, along with his bizarre and distorting metaphors, seems to have simply misread the Greek he prints when he translates ‘or’ rather than ‘as’ (η for ή) with inevitably baffling results.) Partly, however, this awkwardness in Mathiesen’s English stems from his willingness to be unnecessarily off-putting to the Greekless reader. The irrational soul has ‘epithymetic’ and ‘thymic’ parts he tells us, as if someone unfamiliar with Aristotle would know what such untranslated terms could mean. Giving the Latin titles for Greek works is traditional in some circles: but most modern musicologists, I suspect, would recognise Aristophanes’ *Frogs* before his *Ranae*.

The final section of the book moves beyond the ancient world
and traces the medieval transmission of these theorists (often with surprisingly large numbers of manuscripts). This is a clear, and for me at least, an instructive account of an underdiscussed area. Like the other sections, its story of reception focuses primarily on theorists and their manuals, and Mathiesen is uniquely placed from his earlier work on these manuscripts to produce a nuanced and fully informed treatment. Neither West nor the other general discussions I have cited has broached so wide a chronological span as this, and, consequently, this is a good starting point for students interested in this topic.

This is, then, not an easy book to read. Nor is it clear who its intended audience is. Its length, its detail, its insistence on transliterating obscure technical terms, the sheer inelegance of its prose are all barriers which might seem superficial if there were a historical narrative that respected the massive shifts of cultural expectation and institutional form from the classical city to the Hellenistic library to the Christian monastery. Even well-constructed conclusions to sections, chapters, arguments would help the reader along. As it is, the obscurity of the primary sources fosters a relentless chore of explication, undertaken for its own sake. Like his hero, Aristides Quintilianus, Mathiesen passionately wants to show that the lack of respect and scorn with which this material has been treated are misplaced. Unfortunately, like Aristides Quintilianus, Mathiesen provokes the very reactions he wishes to banish. This is a book for the (very) few.

The major problem with the conceptualisation of the project is not just that the opening chapters offer so jejune a cultural history before rushing headlong into the highgrounds of theory (though it certainly is no longer enough to list performance venues and genres of composition and call it a ‘history of musical life’). Rather, it is Mathiesen’s unwillingness to consider how musical theory itself must be part of cultural history. ‘Theory’ does not exist in a vacuum. Sir Geoffrey Lloyd in particular has amply demonstrated over many years’ publication how Greek science – Greek knowledge – in all its forms is part of a broad intellectual debate and cultural conflict, a debate not just within subjects but between subjects and sources of authority.17 The role of such ‘intel-

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lectual discourse’ in the power games of ancient society has been the focus of much brilliant critical analysis in recent years – from astrology to hydraulics, from philosophy to hunting manuals. What is finally most dispiriting for me about Mathiesen’s book is that for all the wealth of detail he offers, he never faces the simple but essential question of what music theory is for. Or shares with us why he believes we should care about it.

Mathiesen writes in the name of Musicology. But it would be a pity if classicists were to think that this very particular and very partial view represented the full scope of the discipline. These are heady days for the study of Greek music. Unlike Winnington-Ingram, we do not still have to long for the basic tools to be made available for the student. Hard though it is to tell from Mathiesen’s volume, there is an immense intellectual excitement and real progress in our understanding of what is a crucial element of ancient Greek culture – which can also teach us a good deal about the role of music – the very definition of music – in modern society. What is needed still – and is neither provided nor attempted by Mathiesen’s volume – is further work which brings together the sophisticated technical understanding of ancient music and music theory with the sophisticated cultural anthropology and history necessary to understand the role of music in Greek society.

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As most readers of Early Music History in the year 2001 know, in April 1998, shortly after finishing a comprehensive draft of The Advent Project, James McKinnon was diagnosed with terminal cancer, the cause of his death ten months later (23 February 1999). Under distressing and painful circumstances, without the leisure of being able to reconsider argumentation or conclusions, a book

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18 See e.g. T. Barton, Power and Knowledge: Astrology, Physiognomics and Medicine under the Roman Empire (Ann Arbor, 1994); Ancient Astrology (London and New York, 1994).
which is sure to spark new debate about the music of the Mass and its history was destined to be completed. It says much about the courage and character of the author, his devotion to learning and to the music of the liturgy he loved so well, that he persevered in his determination to present this volume to the scholarly world. Some discussions might have been amalgamated or rewritten, a procedure that arguably would have produced a more ‘perfect’ book, but one deprived of the personality of the author, who speaks so directly to his readers from these pages.¹ Copies of the book were sent to various scholars (though not to the present reviewer) previous to a conference held at the University of North Carolina at Chapel Hill in honour of the author shortly before his death. The conference elicited a number of distinguished papers, and the proceedings will soon be published.

The book’s subtitle gives some idea of the scope of McKinnon’s endeavour: no less than a comprehensive history of the genesis and definitive shape of the six chants (introit, gradual, tract, alleluia, offertory and communion) that constitute the Roman Mass Proper. Although Rome is the main focal point of The Advent Project, its chronological range stretches from late antiquity to the eleventh and twelfth centuries, the period when Old Roman chant was first committed to notation. Geographically, it ranges from the ancient Christian East to the Carolingian realm. It also covers all seasons of the liturgical year, since the ‘Advent Project’ of the schola cantorum is regarded as a paradigm for the organisation of the entire Temporale and Sanctorale.

The Advent Project sets for itself the formidable task of writing the history of a liturgical book, the gradual, without a history. Unlike the other books used in the celebration of the Mass (sacramentary, capitulare, epistolary, evangeliciary, lectionary, missal), whose long and complex history can in some cases be traced back to the sixth century, the gradual emerges fully formed at the end of the eighth century, documented for the first time not in Rome but north of the Alps. No trail of evidence – fragmentary manuscripts, marginal notes, lists of chants – exists that would enable scholars to trace the evolution of the gradual from tentative begin-

¹ Doubtless the bibliography, which is very small for a monograph of this scope, would have been expanded. The notes of the present review will include references to useful studies not cited in The Advent Project.
nings to the earliest extant specimens. The earliest graduals have no musical notation, but with the exception of the cantatorium of Monza, containing only the cantor’s music for the responsorial chants of the Mass (gradual, alleluia) and the tract, they transmit a complete cycle of Proper chants for both the Temporale and the Sanctorale. Only a few details, like the choice of alleluias and some provisions for the Sundays after Pentecost, are left unresolved. The history of the gradual must, furthermore, take into consideration not only texts but two complete musical repertories, Old Roman and Gregorian. McKinnon prefers ‘Roman’ for the first (a preference that will be followed henceforth in the present review); some scholars apply ‘Frankish’ to the latter repertory.

Since there seems to have been a general ferment of liturgical renewal in the last few decades of the seventh century at Rome, a more extensive background treatment of the sacramentaries, papal and titular, and lectionaries would have been desirable. I believe that integration of the broader urban liturgical context of the time would have enhanced McKinnon’s presentation, which generally treats the chants as isolated from other parts of the Mass.

McKinnon proposes that there existed at one time at Rome small text collections (libelli) of introits, graduals, etc. not assigned to specific liturgical observances. Singers would have chosen from this repertory to meet the needs of various occasions for which they were expected to provide music (p. 124). Nothing of this nature survives, and its composition would be atypical of the present scholarly definition of a libellus as a booklet containing material for a single liturgical celebration. Apart from a few notable examples of coordination like the Advent–Christmas block of Propers, the chants of the Mass seem to have been organised ‘ver-

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2 The earliest witnesses of the gradual have been published in René-Jean Hesbert, *Antiphonale missarum sextuplex* (Brussels, 1935).

3 Chapter 5 of *The Advent Project* offers a summary overview of the Mass books, but it does not go far beyond the information provided by secondary sources. For further bibliography I would refer the reader to ‘Liturgy and liturgical books’, *The New Grove Dictionary of Music and Musicians*, 2nd edn (London, 2001), xv, pp. 1–10. The Sacramentary of Trent, a Gregorian sacramentary revised for the Lateran in the early 680s, has been published in a modern edition: *Monumenta liturgica Ecclesiae Tridentinae saeculo XIII antiquiora*, ed. F. Dell’Oro and H. Rogger, 2 vols. (Trento: Società per gli Studi Trentini, 1983–7), i, pp. 73–461; the existence of this edition is not mentioned in *The Advent Project* (p. 421, n. 6).

4 See M. Huglo, *Les livres de chant liturgique* (Typologie des sources du moyen âge occidental, 52; Turnhout, 1988), pp. 64–75; Huglo dedicated several valuable pages to the *liber gradalis* (pp. 99–108).
tically’ by genre in smaller or larger groups covering specific portions of the liturgical year rather than ‘horizontally’ as groups of diverse chants for a specific liturgical celebration.

The chant texts frequently deviate from the biblical text by inserting words, altering grammatical constructions, or bringing scattered phrases together in a cento (see the offertory verse ‘Ego autem’ on p. 304). This would have required a special written collection of chant texts, because singers could not have simply referred to a Psalter to refresh their memories. How the Roman singers archived the ‘edited’ texts which they sang will remain a mystery, though we know from other sources that a cantor carried into the ambo with him a book as a symbol of office, whether or not it served a practical necessity. One cannot discount the existence of chant libelli – perhaps the definition of this type of book needs to be expanded.

McKinnon identifies the Advent Project as ‘the beginning of the Mass Proper’s final revision’ (p. 137). He argues that all the Proper chants of Advent ‘display a level of compositional planning and perfection of execution not met with elsewhere in the annual cycle’ (p. 137), and he attributes this undeniably fine achievement to the singers of the Roman schola cantorum. The remainder of the annual liturgical cycle does not manifest similarly comprehensive patterns of organisation. He must frequently admit that the schola did not persist in realising its lofty goals: ‘for the rest of the year, [the singers of the schola] were able to maintain their high standards only with the more manageable introits and communions’ (p. 142). There are only a few examples of obvious large-scale planning to be found among the Mass Propers. A seamless numerical unity is never encountered except in the case of the original twenty-six Lenten weekday communions, derived from Psalms 1–26. Even this flawless organisation was subsequently disrupted by the addition of the Lenten Thursdays c. 720 and, less explicably, by five communions set to Gospel texts that replaced the psalmic communions.

5 The Monza cantatorium mentioned above was not a primitive libellus but a Frankish confection for the convenience of the solo cantor. See Huglo, Les livres, Tableau x (p. 100).
6 Reliable information about liturgical practice can rarely be extracted from the Liber pontificalis. It does record, however, the promotion to liturgical status of the Thursdays in Lent by Pope Gregory II (715–30). Chants for these days were taken from the repertory of pieces assigned to the Sundays after Pentecost, a procedure long considered by scholars to signal the end of the ‘creative’ period of Roman chant.
The Advent Project is divided into three large sections: (I) The Prehistory, (II) The Seventh-Century Roman Background and (III) the Advent Project. The first section is occupied with the early history of psalmody. Psalmody was not an essential component of the Mass from the beginning, and the loci appropriate for singing were only gradually occupied, a process of accretion that McKinnon traces in the first four chapters of the book. Notwithstanding, McKinnon states his conviction that ‘the creation of the Roman Mass Proper was more a single concerted act than one that took place in stages’ (p. 361). Instead of taking centuries to evolve, the musical shape of the Mass chants was defined not during a single generation or over several decades, but within the space of a few years. This ‘concerted short-term project’ combined not only decisive reorganisation and reworking of inherited musical traditions but also the creation of entirely new chants.

The Roman schola cantorum, well established by 675, commenced a vast enterprise to (re)shape and complete the music of the Proper of the Mass for the entire liturgical year. Since there are no archives, letters, or contemporary reports that allude to the schola’s grand undertaking, the seventh-century circumstances must be deduced solely from the musical documents. This obliges the author frequently to have recourse to supposition and hypothesis. The ‘Advent Project’, so called because the organisation and creative process began with that season, extended to the remainder of the liturgical year with mixed results. The schola cantorum planned a reform of the Sanctorale, but this too was never realised.

‘Properisation’ – the assignment of a chant to one specific date in the liturgical year – was allegedly the goal of the Advent Project and its continuation. Deviations from that ideal are perceived as flaws and judged critically. McKinnon was of course aware that demonstrating truly comprehensive full-year planning would be a

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7 The frontispiece depicts the ‘schola cantorum’ that occupies a dominating position in the nave of the Roman church of San Clemente. Similar structures, once present in various churches of the city, seem to have received this name only in the seventeenth century. They were monumental choirs erected for the singing of the Divine Office in the high Middle Ages and had nothing to do with the papal choir. The Advent Project has few typographical errors that could mislead those equipped to read this book. I mention only a few for the convenience of the reader: Caspar Ott should be Carolus (Carl), Jean Hanssens’ first name is not John (p. 422, n. 21), the Roman gradual Vat. lat. 5319 is not deposited in the Archivio di San Pietro (p. 423, n. 3), Laetatur cor should be underlined in Table 6.
difficult exercise. The more obvious design module characteristic of the Roman liturgy seems to be an organisation by seasons (Advent–Christmas, Lent, Paschaltime, post-Pentecost) rather than by a calendric layout covering twelve months. Indeed, this very fact is reflected in the author’s subdivision of the chapters on the individual chant genres.

Chapters 1 (The First Centuries) and 2 (The Later Fourth Century) find McKinnon on terrain where he is the acknowledged master. While singing at banquets was an ancient custom followed by second- and third-century Christians, music was not an essential component of their morning eucharistic assemblies. By the end of the fourth century, however, a psalm had become a significant, if not indispensable, part of the service of readings that preceded the eucharistic prayer and communion.

Chapter 2 focuses on the introduction of psalmody to the Mass, principally the responsorial psalm between the reading of the Epistle or Hebrew Scripture lesson and the Gospel. McKinnon assumes that the Psalter was initially placed on a par with the prophetic books of the Hebrew Scriptures. While it is by no means unusual for the psalm to be called a lectio, the mention of a response by the congregation necessarily implies that the rendition was something we would consider musical. Part of the difficulty with sorting out the psalm-as-reading vs. the psalm-as-song is the prevailingly ambiguous way in which ancient authors refer to the ‘performance’ of the psalm. They called the psalm between the lessons a ‘reading’ even when it was unambiguously sung. As McKinnon observes, ‘ecclesiastical authors of the later fourth century . . . still speak of the psalm as biblical reading. At the same time, however, their language reveals that they also looked upon it as a distinctly musical event’ (p. 59). The psalm-as-reading hypothesis needs to be viewed from the standpoint of the possibly thin line that separated stylised reading from simple song in the

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8 In his explanation of Psalm 40 Augustine alludes to the psalm refrain as ‘what we have sung in response to the reader (legenti)’. See J. McKinnon, Music in Early Christian Literature (Cambridge, 1987), no. 357; cf. also nos. 364, 366 (‘we had prepared for ourselves a short psalm, which we had ordered to be sung by the reader’), but also 371 (‘we heard the Apostle, we heard the Psalm, we heard the Gospel; all the divine readings (divinae lectiones) sound together . . .’). In commenting on another Augustinian passage (no. 373) McKinnon himself points to the ‘interchangeability of dicere and cantare’, a commonplace in medieval Latin. Inversely, one is reminded of the phrase ‘cantantibus organis’ in the responsory that once contributed to St Cecilia’s fame as patroness of music.
ancient world.\textsuperscript{9} Simplicity of execution helps to explain how the lector, very often a child, assigned to present the first reading could also take up a responsorial psalm.

McKinnon has used the term ‘lector chant’ to describe this type of singing.\textsuperscript{10} The office of lector was generally not a permanent one, but almost invariably a preliminary step to a clerical career. The \textit{Breviarium Hipponense} (393), a canonical collection compiled only a few years before Augustine became bishop of Hippo, assumed that those who intended to remain lectors would either marry and live chastely with their wives or dedicate themselves to celibacy.\textsuperscript{11} No permanent institutions existed to train children as lectors, nor did they enjoy any specialised musical instruction. Neither at Rome or elsewhere was there an established ‘guild’ of lectors comparable to the organisation we know as the Roman schola cantorum. At Rome a young aspirant had merely to demonstrate his ability in the presence of the pope, who would promptly appoint him a lector. While \textit{The Advent Project} acknowledges the youth of lectors, an assessment of this office in the Western Church might have revealed the small role the lectors played in the evolution of chant. The young lectors were eventually replaced by skilled cantors, thus opening the way for a professional body of singers like the Roman schola cantorum to elaborate a genuinely musical repertory.

An incipient stage in the process of developing a yearly cycle of


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chants may be seen in a late fifth-century report that a priest of Marseilles, Musaeus, was commissioned by his bishop to draw up a cycle of Mass readings with chants that would serve to complement them. According to Gennadius' *De viris illustribus*, Musaeus ‘excerpsit ex sacris scripturis lectionum [lectiones?] totius anni, festivis apta diebus responsoria, etiam psalmorum capitula tempore et lectionibus congrua’. McKinnon interprets *capitula* as entire psalms. This may be so, but Benedict of Nursia (d. 530) did not understand *capitulum* in this way: he applied the term not to full psalms but to the individual segments of Psalm 118 assigned to the day hours. The books which list the beginning and end of the pericopes read at Mass are known as capitularia. They identify the extracts by citing the identifying number from the canon tables of Eusebius of Caesarea (c. 265–c. 339/40) preceded by the abbreviation *cap*. Musaeus’ responsoria may have been refrains designed to answer the verses chosen as ‘psalmorum capitula’. His commission to produce such a collection may indicate that in Gaul a process of shortening the interlesson psalmody had begun before the fifth century came to a close.

The singing of a psalm between readings must have been introduced to the Roman liturgy before the time of Pope Leo I (440–61). Unlike Augustine, Leo referred only rarely to this psalm, but it was obviously a permanent feature of the Mass of his day. It has recently been argued that a passage in the *Liber pontificalis* biography of Pope Celestine I (422–32) refers to the introduction of the responsorial psalm between the readings. Celestine is credited by his biographer with introducing the singing of psalms ‘before the sacrifice’. Amalar of Metz (c. 775–c. 850) had interpreted the phrase ‘ante sacrificium’ as the equivalent of ‘before the Mass’ and concluded that the early sixth-century author of the biographical sketch was referring to the creation of the introit as a musical enhancement of the pope’s ceremonial entrance for the liturgy.

12 Cap. 79, *Hieronymus et Gennadius De viris illustribus*, ed. C. A. Bernoulli (Sammlung ausgewählter kirchen- und dogmengeschichtlicher Quellenschriften; Freiburg, 1895), p. 87; *Scholastik*, 3 (1928), 8 (1933).
15 *Liber officialis* 3.5.2; *Amalarii episcopi opera liturgica omnia*, ed. J. M. Hanssens, 3 vols. (Studia e Testi, 138–40; Vatican City, 1948–50), ii, p. 272. The *Liber officialis* went through three or four editions, the last about the year 835; on the dating of Amalar’s treatise see Hanssens, i, p. 75.
This interpretation was followed by most authors from the Middle Ages to the present, but Peter Jeffery challenged this attribution of the introit to Celestine and proposed instead that Celestine had introduced responsorial psalmody between the Epistle and Gospel readings.¹⁶ This view is accepted in The Advent Project.

The crux interpretum is sacrificium, a word that embraced multiple meanings in Christian Latin. Jeffrey construed its meaning in Celestine’s biography narrowly as referring explicitly to the eucharistic portion of the Mass, as distinct from what modern writers would call the ‘liturgy of the word’ (known also as ‘Mass of the catechumens’, since those who had not been baptised were dismissed at its conclusion). Unfortunately, the Liber pontificalis proves of little contextual assistance, since sacrificium occurs only two other times in the first edition (c. 530). The biography of Pope Telesphorus (c. 130) claims that he decreed that ‘the angelic hymn Gloria in excelsis be sung at the entrance of the sacrifice (in ingressu sacrificii)’¹⁷ As historical evidence, this statement is worthless: the author of Telesphorus’ ‘biography’ could have known nothing of this pope, nor did he realise that the Gloria did not even exist in the early second century. The Liber pontificalis also asserts that Pope Sylvester (314–35) required that the ‘sacrificium altaris’ be celebrated with altar coverings of pure linen, not silk or dyed cloth.¹⁸ While one might accept this regulation as authentic, it seems dubious that the passage intends to make a distinction between the ‘liturgy of the word’ (if that even existed in Sylvester’s day) and the portion of the Mass that centres on the eucharistic prayer.

The fundamental question, of course, is not what Pope Celestine actually did, but what the early sixth-century author of his Liber pontificalis biography thought he had contributed to the liturgy with which he was familiar. An admittedly incomplete survey of sacrifici-


¹⁷ ‘Hic fecit ut natalem domini nostri Jesu Christi noctu missae celebrarentur et in ingressu sacrificii hymnus diceretur angelicus Gloria in excelsis deo, tantum noctu natale domini’; Liber pontificalis, i, p. 57. Emanuel Bourque observed the tendency of the Liber pontificalis to place liturgical practices whose origins were obscure or unknown ‘presque au rang de traditions apostoliques’; Étude sur les sacramentaires romains, i: Les textes primitifs (Studi di Antichità Cristiana, 20; Vatican City, 1949), p. 19.

¹⁸ ‘Hic constituit ut sacrificium altaris non in sircicum neque in pannum tinctum celebraretur, nisi tantum in lineum terrenum procreatsum, sicut corpus domini nostri Iesu Christi in sindonem lineam mundam sepultus est’; Liber pontificalis, i, p. 171.
cium in early Christian Latin strongly suggests that the wider meaning of the word, i.e. the equivalent of ‘Mass’, should be assumed unless arguments to the contrary can be adduced. There is, moreover, fifth-century Roman evidence for the broader meaning. Pope Leo I, who became pope within a decade of Celestine’s death, was asked by Bishop Dioscuros of Alexandria (d. 454) what to do whenever the traditional single Sunday Mass could not accommodate the throngs wishing to attend. Leo wrote that the ‘sacrificii oblatio’ should be repeated as many times as necessary, ‘since it is quite pious and reasonable that, as many times as the basilica is filled with a new congregation, this many times should a subsequent Mass (sacrificium) be offered’. In the following sentence Leo employs the term missa as the equivalent of sacrificium. One cannot escape the conclusion that fifth-century Roman usage did not reserve sacrificium for a specific, restricted portion of the Mass liturgy.

The anonymous Liber pontificalis author who composed the biography of Celestine must have been familiar with the meaning attached to sacrificium in Lateran circles. If he claimed that something occurred ‘ante sacrificium’, he probably meant ‘before the Mass’. This can only be the introit. The responsorial psalm may very well have been present in the Roman Mass of Celestine’s day – this would not at all be unlikely, since it certainly existed at the time of Leo I. The Liber pontificalis report can still reasonably be read as a reference to the existence of the introit in the Roman


21 A late sixth-century reviser of the Liber pontificalis added the phrase ‘antependantim ex omnibus’. Peter Jeffrey has shown that ‘psalmis’ should be supplied, thus implying that portions of psalms only (and appropriate refrains?) were understood by the reviser. Could the existence of refrains have been emphasised? See Arnobius (‘floruit in Italia tempore Leonis papae I’; Clavis patrum Latinorum, p. 94) and his use of the term in the Commentarii in psalmos, in Arnobii Iunioris Opera omnia (Corpus Christianorum: Series Latina, 25; Turnhout, 1990), ed. K. Daur and indexed in Instrumenta lexicologica Latina, A, 73.
Mass by the end of the fifth century, whether or not Pope Celestine was the one who introduced it.

Amalar of Metz was familiar with both the narrower and the broader meaning of the *sacrificium*. In addressing the question of how long catechumens should be allowed to remain at Mass, he maintained that they should stay for the Gospel, but ‘they cannot by any means be present for the sacrifice, but only the reborn [baptised]’ (*sacrificio omnimo [sic] interesse non possunt nisi renati*). Since they are not yet numbered among the faithful, they must depart ‘when the sacrifice is celebrated’ (*eo tempore recedunt quo sacrificium celebratur*). Amalar recognised the ambivalence of the term and interpreted it according to the context.

More puzzling still is a reference to the singing of psalms at Mass in an annotated bibliography, the *Liber retractationum*, prepared by Augustine of Hippo about the year 426. A distinguished citizen of Carthage, Hilary by name, had objected to ‘singing at the altar hymns from the book of psalms both before the oblation and while what had been offered was distributed to the people’ (so Augustine’s refutation of Hilary has not been preserved, one can only surmise what the objections of the aristocratic Carthaginian (a ‘vir catholicus’ and not a hateful Donatist, as Augustine dutifully mentions!) might have been. McKinnon proposes as ‘the more obvious meaning’ of this passage a reference to the responsorial psalm between the readings, though it is not clear why Hilary might have objected to this apparently well-established African practice.

It may be of significance that Augustine places the objectionable singing ‘at the altar’ (*ad altare*). Excavations of North African churches have revealed that the altar was located forward of the apse in the middle of the nave, hence far removed from the elevated podium at the eastern end of the basilica where the readings (and presumably the psalm chanted between them) would

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22 *Liber officialis* 3.36.8–9, ed. J. M. Hanssens, ii, p. 371. This passage occurs only a few lines before the chapter ‘De adventu Domini’ (*Liber officialis* 3.40).

23 ‘Ut hymni ad altare dicerentur de psalmortum libro, sive ante oblationem sive cum distribueretur populo quod fuisset oblatum’; *Retractationum libri II*, ed. A. Mutzenbecher (Corpus Christianorum: Series Latina, 57; Turnhout, 1984). The translation is from *Music in Early Christian Literature*, no. 385. Note that Augustine uses the verb *dicere* in a context that can only mean genuine singing.
have been delivered. Is it any more likely that an introit chant would be sung from this position? Thus we are left to consider what McKinnon calls the ‘more complex’ explanation: that Hilary had objections to singing psalms while the bread and wine for the Eucharist were being brought forward. It seems unlikely that Augustine would have bothered to respond to Hilary if he simply disdained liturgical innovations of any sort. Hilary’s objections must have been as theological as they were liturgical. Possibly he objected to honouring the unconsecrated bread and wine with excessive ceremony. The unfortunate loss of this Augustinian treatise deprives us of a valuable document on the early history of liturgical music.

Commentators on the liturgy have long admired the choice and cohesiveness of the readings and chants for the four Sundays of Advent and the three Ember Days (Wednesday, Friday and Saturday) that fall in the third week of Advent. Obviously, the singers who devised this programme of chants did not work alone, but implemented liturgical plans devised by higher authorities in the city of Rome. Just as obviously, there could be no ‘Advent Project’ before the institution of Advent itself. This observance of a period of preparation before the feast of the Nativity had a history of several centuries preceding its Roman musical codification and probably evolved still further before the chants made their earliest appearance in the Frankish graduals. A comprehensive chronology of the development of Advent might have well served as a prelude to the study of the schola’s musical activities. Only about a half-dozen pages address this question directly, and the results are expressed in terms more tentative than they need be.

In the hope that it might be useful to readers of The Advent Project, I will offer a rapid summary of the early history of Advent, its


introduction to Rome, and the transformation it underwent on the banks of the Tiber.

The season of Advent was unknown to the early Roman liturgy. It is never mentioned in the sermons of Pope Leo I (440–61), nor are there any prayer formularies for this season in the collection of Mass *libelli* known as the Verona Sacramentary, compiled from much earlier materials probably in the final third of the sixth century.26 A period of preparation antecedent to the celebration of the Nativity developed for the first time in Gaul and Spain. Gallican lectionaries indicate that churches in Gaul and Francia began Advent six weeks before Christmas.27 The lectionary of Sélestat (c. 700) provides a series of six pericopes from Isaiah for the first reading (of three) in the Gallican Masses of Advent. Its testimony is confirmed by a fragmentary epistolary, also preserved in the Bibliothèque de la Ville at Sélestat.28 Since the beginning of this six-week period of fasting followed shortly upon the feast of St Martin (11 November), it was sometimes known as the ‘carême de Saint Martin’. The remarkable series of prayers in the ninth-century *Liber Mozarabicus sacramentorum* manifests eloquently the profound theology that had begun to surround liturgical preparation for the observance of Christmas. This ancient sacramentary and the tenth-century Mozarabic antiphoner of the Mass have five Sundays of Advent. In both cases the Temporale begins with the first Sunday of Advent.29

Sometime about the middle of the sixth century the six-week Gallican model of Advent was introduced at Rome. The Würzburg

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26 Antoine Chavasse dates it to the pontificate of John III (561–74) in ‘Le sacramentaire dit Léonien, conservé par le Veronensis LXXX (80)’, *Sacris Erudiri*, 27 (1984), pp. 151–90. The article was reprinted in the author’s *La liturgie de la ville de Rome du Vᵉ au VIIᵉ siècle* (Studia Anselmiana, 112; Rome, 1993), pp. 69–107 (hereafter *La liturgie*).


Epistle list, considered to represent an urban lectionary of the early seventh century, contains five Sundays ‘de adventu domini’. The nominal sixth Sunday, which followed the Saturday Ember Day, was a *dominica vacans* in Rome and thus had no liturgical formulary of its own. The readings for the December Ember days (*ieiunium mensis decimi*) in the Würzburg manuscript do not feature the traditional harvest references, having already been adapted to the theme of Advent. The early seventh-century (c. 626/7) Roman model of the *comes* of Alcuin must also have accommodated an Advent of six Sundays. In this source the first three Sundays *ante natale domini* are followed by prophetic readings for the Ember Days and the *ebdomada I ante natale domini*. Immediately thereafter, however, are two additional Advent readings (Heb. 10: 35–9 ‘qui venturus est veniet’, and Jer. 23: 5–8 ‘ecce dies veniunt’) marked ‘item ut supra’, thus bringing the total number of Sundays to six.\(^{30}\)

The homiliary of St Peter’s, represented by several manuscripts dating from the eighth and ninth centuries, still implies an Advent of six weeks (five readings for the Sundays and one set for Ember Saturday preceding the *dominica vacans*) characteristic of the later sixth-century Roman liturgy.\(^{31}\) The lectionary of St Peter’s (Archivio di San Pietro C 105) begins with seven lessons for Advent followed by three sermons of Leo I for the *ieiunium mensis decimi*. The Roman homiliary of Agimond (Vat. lat. 3835/3836) from the beginning of the eighth century offers a large selection of Advent pieces. Three sermons *de adventu* (Caesarius, Maximus) precede the four Advent homilies of Gregory the Great, which are followed in turn by three further homilies *de incarnatione domini*, closing with Leo I’s sermons for the Ember Days.\(^{32}\)


\(^{32}\) Grégoire, *Homéliaires*, pp. 380–83. The collection of Gregory’s homilies contains only four on the Gospel readings of Advent (1, 6, 7, 20), the last designated for the Saturday vigil of Ember week. [Chavasse argues that there are six Advent homilies in the collection...}
Sacramentary preserves the very same format: five sets of ‘orationes de adventum [sic] Domini’ are followed by prayers and a preface for the Ember Days. The sole surviving manuscript of this sacramentary was copied about 750 in the vicinity of Paris, but most of its contents represent a stage of the Roman liturgy at least a half-century earlier, and thus not too far removed in time from the Advent Project itself.

The six Sundays were eventually reduced to four, and the dominica vacans following Ember Saturday was provided with its own liturgical formulary, borrowed generally from the preceding Wednesday. The Advent Project singles out a form of the Roman evangeliary (PI = Π in Theodor Klauser’s comprehensive analysis of such books) as an early representative of a four-week Advent. The eventual dominance of the four-Sunday Advent model at Rome is confirmed by the Gregorian sacramentaries, whose three Advent Sundays are complemented by a (fourth) dominica vacans. It has been suggested that four vs. six Sundays might represent a distinction between the papal liturgy and that of the Roman tituli respectively. Since the schola was dedicated exclusively to the service of the popes, a four-week Advent model would be natural. This question merits further study, and it could have shed light on the events that led up to and surrounded the Advent Project.


It has been suggested that the Advent of five Sundays found in some eighth-century Frankish documents was occasioned by the Franks’ misunderstanding of the dominica vacans and their confusion about the older and newer Roman observances. Amalar resolved the problem of the rubric ‘ebdomada quinta ante natale Domini’ and the four-Sunday Advent known to him with his accustomed recourse to creative allegorising; Liber officialis 3.40.1–3 (De adventu domini), ed. Hanssens, ii, pp. 374–5.
The early Roman sources, reacting no doubt to the novelty of Advent, placed it at the end of the liturgical year. Not knowing what to make of this foreign innovation, the Gelasian Sacramentary appended the (five) Advent Sundays and the December Ember Days to the end of Book 2, the sanctoral portion of the manuscript. A striking feature of the earliest Frankish Mass antiphoners is their placement of Advent at the beginning of the annual liturgical cycle. The foundations of this arrangement can almost certainly be traced back to Gallican practice. The earliest liturgical book that commences with Advent seems to be the Bobbio Missal from the early eighth century. The same arrangement is found in the homiliary of St Peter’s (C 105) and the homiletic collection of Paul the Deacon compiled between 786 and 799 at the request of Charlemagne.

In several documents (the Paduensis and Hadrianum Sacramentaries, Ordo XIV) the phrase ‘mense decembri’ occurs as an apparent title to the prayers or readings of Advent. I do not believe, as McKinnon does (p. 149), that the season of Advent was ever compressed into three Sundays. If such were the case, four Advent Sundays could be accommodated within the month of December only four times out of seven. The phrase may be simply a convenient title or, as with the Office lectionaries, a normal way of assigning Scripture readings to particular months of the year.

By the time the ‘Advent Project’ got under way Rome (or the papal liturgy) had embraced a four-week Advent and had begun to follow the Gallican practice of considering Advent the beginning of the ‘liturgical year’. What chants were sung at Mass

35 The index of an Advent chant libellus might have resembled the two leaves in MS 490 of the Biblioteca Capitolare of Lucca, a nearly complete list of chants for Mass and Office headed ‘INC[ipiunt]. AN[tiphone]. ANNI CIRCUĻI DE ADVEN[tu]’. See J. Froger, ‘Le fragment de Lucques (fin du VIIIe siècle)’, Études Grégoriennes, 18 (1979), pp. 145–55, who dates the fragment c. 796. See also Hesbert, Antiphonale missarum sextuplex, pp. xxv–xxvi.

36 An abridgement of a Roman lectionary in a Verona manuscript also begins with Advent; R. Etaix, ‘Un homiliaire ancien dans le ms. LII de la Bibliothèque Capitulaire de Vérone’, Revue Bénédictine, 73 (1963), pp. 289–306.

37 The statement that ‘The four gospels of the 645 evangelium then, and the four (in fact five) prayer sets of the Gregorian sacramentaries could provide for the possibility of a four-Sunday Advent, although in practice the number appears to have been variable’ (p. 149) leaves the reader in greater confusion than is necessary.

38 The theory that Gallican chants were imported along with the Advent observance cannot be demonstrated. If the Romans were able to create new prayer formularies and select appropriate scriptural readings, they were capable of providing their own music, too.
during the previous hundred years, when six Advent Sundays were the norm? Intriguing in its implications is Dom Jean Claire’s observation that there are really six Mass formularies for Advent in the Old Roman and Gregorian traditions: three for the Sundays and three for the Ember Days.39 The Advent Project does not explore the possibility that the already extant chants of a six-Sunday Advent were simply redistributed by the schola to cover four Sundays and the ‘properised’ Ember Days. If such were the case, the activity of the schola cantorum – at least with respect to the chants of Advent – might have been not an act of creativity but merely one of skilful reorganisation.

More than one-third of the book (chs. 8–13) is devoted to an examination of how each chant genre (introit, gradual, etc.) fits into the overall scheme of the Advent Project and its presumed extension to the rest of the liturgical year. Each chapter discusses the repertory according to the four liturgical seasons: Advent–Christmas, Lent, Paschaltime and post-Pentecost, taking up special cases as the need arises. Several common criteria are applied to each of the repertories in turn: (1) the distinction between psalmic and non-psalmic chants, (2) the phenomenon of ‘textual adjustment’ – editing of the biblical text to create unique texts for musical setting – and (3) musical analysis. While all of these indicators are called into play to assign dates to individual portions of each repertory, musical considerations are rarely decisive in the author’s assignation of portions of the repertories to specific temporal layers. The stylistic homogeneity of the introit melodies, for example, renders distinctions based on musical analysis and comparison difficult.

**Introit**

The earliest unequivocal references to the Proper chants of the Roman Mass liturgy occur in *Ordo Romanus I*, a ceremonial guide that describes in great detail the solemn papal mass as it was celebrated on Easter Sunday at the beginning of the eighth century. By this time an elaborate ritual had been developed for the procession of the pope with his assisting clergy from the *secretarium*

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(sacristy) to the presbyterium (altar precinct). A member or members of the schola cantorum sang psalm verses to which the entire schola responded with a refrain, a musical form found in the earliest graduals. This purely Roman development has no clear parallel in the Byzantine liturgy.

The introit chants are, by and large, uniquely assigned to specific days: few are sung on more than one occasion, except for the chants of the commune sanctorum. The Gregorian introits do not depend on model melodies or on the application of formulae, and the same is true of most of the Roman introits. Some of them, however, particularly those of Paschaltine (Table 14) and to a lesser extent the Lenten introits, manifest a modest level of ‘formulaicism’ in which the opening phrase recurs at the beginning of the second half of the introit. The surface uniformity so characteristic of the Roman introits does not place this feature in markedly high relief. Although formulaicism militates against uniqueness, the high value attached to properisation merits admiration for the ‘flawless annual cycle’ (p. 207) of the introits.

Regarded in this light, the formulaicism of the Roman introits for Paschaltine makes an ostensibly ‘bad fit’ with the rest of the chants of the Temporale. For the same reason introits of the Sanctorale are regarded as ‘foreign to the original [Advent Project] Roman introit style’. McKinnon is convinced that Frankish cantors would never have ignored devices like repetitive formulae that would have facilitated their task of absorbing the Roman repertory. He concludes that the elaboration of such formulae must have occurred at Rome between the transmission of Roman chant to the North in the eighth century and its notation in the earliest Roman gradual (1071). Recitations on one or two adjacent pitches and the phrase repetitions characteristic of the Roman introits are almost invariably diversified in the Gregorian versions of the melodies.

Further evidence of organisational planning is furnished by the introits for the first sixteen Sundays after Pentecost, whose texts are taken from the Psalter in a gapped numerical order. Textual

The relationship between the Roman and Gregorian introits may be studied in the comparative transcriptions published in A. Turco, Les antennes d’introït du chant romain comparées à celles du grégorien et de l’ambroisien (Subsidia Gregoriana, 3; Solesmes, 1993). This valuable resource is not referenced in The Advent Project.
adjustment in the introits (and other chants in the Roman repertory) attests not only to the care that the schola brought to its responsibilities but also to a later layer of composition, representative of an era in which the schola ‘produced carefully crafted chant texts that they then set to music’ (p. 219). The natural corollary is that these melodies were designed to be reproduced exactly and not subjected to annual ‘reimprovisation’.

Gradual

*Ordo Romanus I* provides little information about the singing of the gradual, but the presence of the entire respond in the oldest cantatoria presupposes that the choir repeated the respond after the soloist’s initial statement. At the end of the solo verse, the complete respond was repeated.41 The Roman schola cantorum or its predecessor institution apparently replaced the old *psalmus respondorius*, once sung by a young lector, with an elaborate formulaic chant reduced to only two psalm verses. According to McKinnon, this took place in the earlier or mid-seventh century, shortly after the founding of the schola. This musical discontinuity seems to be matched by a liturgical one: there is little correspondence between early Christian literary references to psalms sung on specific feasts and the texts of the graduals.

The graduals of both the Roman and the Gregorian traditions share an essentially formulaic structure, which finds expression in two dominant melodic types (A-2 and F-5); these account for more than half the repertory. (While the two traditions have comparable formulae, the match is not complete.) Apparently the Franks found this particular application of formulae congenial, while they seem to have spurned similar procedures among the introits and offertories, if such were present in Roman chant of the late eighth century. In McKinnon’s view, of course, they were not. The Lenten graduals rate highly from the perspective of *The Advent Project*, because they are uniquely assigned, apart from the nine borrowed to fill in the Sundays after Pentecost.

McKinnon provides a lucid explanation of the paired series of post-Pentecostal graduals found in the earliest Frankish chant books. One series is based on a gapped ascending numerical order-

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41 In the first line of p. 223 ‘verse’ should obviously be changed to ‘respond’.
ing of psalms from 6 to 142. This appears to have been a Frankish creation, since it includes two graduals, *Unam petii* and *Domine exaudi*, absent from the Roman tradition. The other, an authentic Roman series, definitively adopted by the Gregorian tradition, receives low marks from the properisation standpoint, however, because of the large number of borrowed chants and the repetition of two chants on different Sundays.\(^4\) This imperfect post-Pentecostal disposition elicits the judgement that the ordering of the gradual in the Roman and Gregorian traditions ‘was never completed perfectly in accord with the ideals of the Advent Project’ (p. 248).

### Alleluia

Chant scholars have long been convinced of the late origins of the alleluia repertory, not least because of the lack of fixity in liturgical assignment and the frequent recourse to a small core of model melodies. McKinnon’s investigations confirm this view and add a number of important points towards refining the implications of ‘late’. He relates the first stage of the alleluia’s development to the singing of alleluia-psalms (the twenty psalms of the Hebrew Psalter prefixed by this acclamation) during Paschaltime as the responsorial psalm of the Mass. The identification of a second stage, the one that produced two consecutive chants for choir and soloist, rests on the hypothesis that Byzantine psaltai influenced the Romans to emulate a Jerusalem practice of singing two chants before the reading of the Gospel.\(^3\) Non-psalmic alleluias belong to the third, and last, phase of the alleluia’s history.

Once the concept of singing alleluia at Mass (outside of Lent) had been adopted, Roman singers had quickly to develop a repertory sufficient to cover the needs of the Temporale and Sanctorale. They resorted to the expediency of basing the repertory on a restricted number of model melodies. The concentration of such melodies is evident even at the heart of the Advent Project. At Rome the three Advent Sundays borrow model melodies (*Excita*...)

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\(^4\) ‘[This] creates the impression of something better described as cobbled together than something carefully crafted’, *The Advent Project*, p. 243.

\(^3\) The notion that the Roman liturgy at one time had three readings and that the gradual and alleluia were thrust together when one of the readings was discarded has no basis in fact. See A.-G. Martimort, ‘À propos du nombre des lectures à la messe’, *Revue des Sciences Religieuses*, 58 (1984), pp. 42–51.
and Ostende) from Paschaltime, while the melody of Dies sanctificatus serves not only for the third Mass of Christmas, but also for Epiphany, three of the sanctoral feasts celebrated during the week following Christmas, and for many of the non-psalmic alleluias.

McKinnon’s ‘exhibit A’ for Byzantine influence is the appearance of five transliterated Greek-texted alleluias in the Roman liturgy of Easter week. The five alleluias occupy a prominent place in the splendid paschal vespers celebrated at the Lateran basilica throughout the week.44 Two of the alleluias (O kyrios and Epi si) were also used for Mass during the same week. McKinnon argues that the introduction of the Mass alleluia ‘inspired the Roman clergy to transform Easter Sunday vespers, hitherto a vesper service of the standard format, into one of special splendour’ (p. 257). Whether or not the development proceeded in this direction, the hypothesis of influence from the East, particularly as regards the role of the alleluia alongside the gradual, merits consideration. The question might arise, nevertheless, why Byzantine musical influence exerted itself so narrowly. Table 21, which lists textual concordances between Byzantine alleluiaria and Roman Mass alleluias, does not by itself demonstrate a very profound influence. All the concordances are psalmic; hence overlaps are to be expected.45 If the Greek-texted alleluias incorporated directly into Old Roman chant are subtracted from the list, only five of the remaining chants suggest possible parallels, and these links are purely textual, not liturgical.

Considerable discontinuity of alleluia assignment exists between Rome and Francia, but the pattern is erratic. While the (later) Roman manuscripts agree among themselves on a standard series, the constant ‘quale volueris’ (whichever you wish) instruction applied to the Paschaltime and post-Pentecost alleluias in the Frankish graduals seems to imply a repertory transmitted in an

44 The Greek alleluias and their Roman parallels were analysed in Christian Thodberg, Das byzantinische Alleluiazyklus (Monumenta musicae Byzantinae: Subsidia, 8; Copenhagen, 1966), pp. 168–95. Obviously, the transmission would have been entirely oral, and strong evidence for this may be seen in the transliterated texts: they were not transcribed from the Septuagint and then set to music. The texts of the Greek alleluias in the Roman gradual Vat. lat. 5319 presume the iotazation of vowels, something that can be explained only by a viva voce communication.

45 Table 21 should also be consulted for its analysis of the Old Roman chant repertory by melody type. It also lists the nineteen alleluias, three of them Greek, that do not conform to a melody type.
unstable, ‘unproperised’ form. McKinnon has identified only fifteen Temporale assignments on which there is complete agreement between the two chant traditions (p. 266), implying that the Franks received only a list of unassigned alleluias, which they employed according to their best devices. This helps to explain the internal Roman consistency and the Frankish lack thereof.

Surprisingly, Advent fares no better with respect to Roman–Frankish agreement. Greater continuity exists between Rome and the North for the three Masses of Christmas, the two following saints’ days (Stephen, John the Evangelist) and Sunday, and Epiphany (Table 23). McKinnon concludes from this evidence that even the Advent alleluia assignments remained flexible when the cantus romanus was exported to the North, suggesting that they were devised only after the Paschaltime series had been completed. Why the schola would have omitted these essential chants from the Advent Project is not explained.

Tract

Of all the Mass chants the tract has the smallest repertory – only fifteen or sixteen chants, all based on two model melodies – one with a D final, the other with a G final, a peculiarity consistent across the two traditions. Almost all the tracts are assigned to the penitential season of Lent. Although McKinnon is justly sceptical of arguments about the presence of ‘latent psalm tones’ as structural devices, he admits the possibility that the C-cadence in the D-2 tracts (F in G-8 tracts) might be the remnant of an ancient in directum psalmody. Such a view fits well with his speculation that the psalm between the scriptural readings was at one time considered a reading itself, though (it must again be observed) the evidence indicates that the psalm was invariably sung with a congregational response. He must therefore assume ‘that the psalm was sometimes, probably on penitential occasions, sung without refrains. There is no positive evidence for this because of the nature of the phraseology involved’ (p. 283).

There is little numerical ordering among the tracts: the only suggestion of such organisation is found in Sundays III–V of Lent, whose tracts are taken from the first few verses of Psalms 122, 124 and 128 respectively. They are all set to the G-8 tract melody, as are the tracts of the Easter Vigil. The latter group of four chants
Cantemus domino, Vinea facta est, Attende caelum and Sicut cervus has generated more than a little interest, inasmuch as the Gregorian melodies displaced the traditional Roman ones by the late eleventh century.46 Chant scholars have understandably tended to focus on the music, identified alternatively as either tracts or cantica, and have not taken into account the full range of Paschal vigil readings in which they are embedded. The replacement of the (vanished) Roman chants might not have been a piecemeal effort focused exclusively on music, but rather part of a wholesale reorganisation of this portion of the Easter vigil modelled after the Romano-Frankish liturgy from the North.47

Ultimately, the tract remains the most puzzling of the Mass chants, both with respect to its form and its exclusive liturgical assignment to penitential occasions, the Paschal vigil excepted.48 Given the brevity of the period when the tract formed part of the liturgy (the six weeks of Lent with a backward extension to Septuagesima Sunday) the lack of a more coherent organisation raises questions about the thoroughness of the Advent Project. On the other hand, it might be regarded as evidence that confirms an ancient lineage for the tracts.49 McKinnon leans towards the latter perspective, but he must judge the ‘tract’s meagre and scattered repertory as the mark of an unfinished product’ (p. 281).

Offertory

A thorough treatment of the offertories and their verses would be too much to expect from a single chapter, but McKinnon makes several observations that will be enlightening even to researchers experienced on this vast terrain. Already at the time of Ordo Romanus I the offertories were probably the richly neumatic chants known from the Roman and Gregorian manuscript tradition.

46 McKinnon observes that the Roman sacramentaries and ordines never single them out for special mention: an argument for the existence of ‘lector chant’. On this topic see also P. Bernard, Du chant romain au chant grégorien (IVe–XIIIe siècle) (Paris, 1996), pp. 101–9 and 163–70.

47 This will be discussed in the broader Roman context in my ‘The Late Eleventh-Century Epistolary from Santa Cecilia in Trastevere’, to be published in the proceedings of a conference on The Liturgy of Rome in the Eleventh Century, ed. Richard Gyug (forthcoming).

48 At the Easter vigil the tract Laudate dominum that follows the Paschal alleluia with its verse Confitemini dominum may conceivably be ‘the earliest of all tracts’ (p. 295).

49 The most promising candidates for tracts with ancient roots are the very long Qui habitat (First Sunday of Lent) and Deus deus meus (Palm Sunday).
Indeed, McKinnon argues from both textual and musical evidence that the schola cantorum may have created pieces that approached the status of original works. He therefore questions whether they were merely the end result of an extended period of gestation, or ‘a carefully crafted genre, the creation of quasi-professional liturgical musicians’ (p. 304).

McKinnon emphasises that the textual basis of the offertories is very frequently not a text extracted verbatim from the Scriptures, but a skilfully crafted fabric of phrases strung together to create passages specifically intended for musical setting. Nearly one-quarter of the Advent–Christmas offertories have such substantial textual adjustment. This reinforces his belief that the genre was a late creation in the scheme of the Mass Proper, and it argues decisively against continuity with a hypothetical early psalmodic phase of the offertory chant. McKinnon also calls into question whether the terms ‘respond’ and ‘verse’ may be appropriately applied to the offertory, for he observes that the portion of the respond repeated between verses is much shorter than is customary in authentic responsorial chants. The musical style of the Roman choral responds, does not, moreover, invariably differ from that found in the ‘soloistic’ verses.

Here and in other chapters McKinnon measures ‘musical continuity’ between Rome and Francia in two different ways: identical finals and similarity of musical details. Relationships of the latter kind are not strong among the Lenten offertories, a phenomenon attributed to the failure of Frankish cantors to master the long and complex Roman music for these chants: ‘the sheer quantity of offertory music caused them to stray from what they heard the Roman cantors sing’ (p. 315). I came to a rather different conclusion in an article that was published too late to be considered in The Advent Project. It seemed to me that the Franks, while accepting the Roman textual tradition, took a rather critical view of Roman formulaicism: in this case the offertories’ habit of depending on two recitation formulae that pervade large parts

50 McKinnon claims that ‘the intention of the schola cantorum members (both during the Advent Project and immediately before it) was to create stable melodies’ (The Advent Project, p. 397): the existence of choral chants would seem to demand as much.

51 Still unexplained, however, is the puzzling repetition of text segments in both responds and verses with the same or slightly varied music.
of the repertory and on the repetition of phrases of varying length.\textsuperscript{52} One could argue that the Franks supplanted all these passages (and there are scores of them) with diversified music, while the Romans, depending on these strategies, were able to maintain the traditional melodies with some degree of integrity until they were notated in the late eleventh century. The Franks also added twelve offertories to the received Roman repertory, further evidence of an independent stance vis-à-vis the Roman tradition.

The large number of offertory chants borrowed from elsewhere to create the Paschaltime cycle represents a low level of properisation. In McKinnon’s view this situation marks ‘the point of breakdown in adherence to the aims of the Advent Project’ (p. 317). Divergence from the (presumed) ideal is not infrequently explained as a failure on the part of the Roman singers, either because they were incapable of carrying out the grand design of the project or because they lacked the stamina to persevere in its realisation. While I cannot judge this argument impartially, it seems unfair to impute such insufficiencies to singers who are elsewhere credited with impressive musical achievements. Alternately, the Franks are portrayed as overwhelmed with the enormity of the task confronting them. Then for some reason the Romans gathered enough strength to finish the post-Pentecostal offertory series with a ‘burst of creativity’. In this instance similar praise is heaped on the Frankish cantors, who adhered closely to the last few chants in this series: ‘recognising the extraordinary character of these chants, [they] made special effort to master them’ (p. 321).

\textbf{Communion}

The nearly thirty pages devoted to the communion chants are central to the overriding concept of the present volume: the schola’s ‘penchant for providing thematically appropriate communions for important festivals [Lent, post-Pentecost, sanctoral feasts] gave way to the ambitious plan of the Advent Project, with its aim of providing a unique and fitting chant for every date of the temporale’ (p. 328). The brilliantly conceived Advent–Christmas

communions (and other genres from the same season) should have furnished the model for the remainder of the liturgical year, but as soon as one visits the Lenten weekday communions, additional questions about the consistency of the Advent Project begin to present themselves.

The original cycle of Lenten communions from Ash Wednesday to the Friday before Palm Sunday was an unbroken numerical series taken from Psalms 1–26. This perfect ordering was twice disturbed, first by the necessary insertion of chants for the Thursdays of Lent, which did not become liturgical until about 720. These communions were supplied by 'a seemingly arbitrary selection of six post-Pentecostal communions' (p. 334), an indication that the schola had ceased creative activity. The perfect symmetry of the Lenten communions was further disrupted by the insertion of five communions on texts from the Gospels, displacing Psalms 12, 16, 17, 20 and 21 of the numerical series. The first three of these communions found a place of refuge early in the post-Pentecostal series (Table 36).

Scholars have assumed that three of the five Lenten Gospel communions were originally assigned to the Sundays of Lent on which 'scrutinies' of the candidates for baptism were conducted. When the Gospels for these Sundays were moved to weekdays (certainly before the Π-type evangeliary that documents the Roman system of readings c. 645), their communions supposedly followed them. McKinnon favours a different scenario – that the Lenten Gospel communions were composed by the schola later in the seventh century along with other Gospel communions. This seems to place the schola in the role of disrupting an inherited symmetry, and it leaves unexplained the introduction of two new Gospel communions (Oportet te and Nemo te) unrelated to the scrutinies that displaced the psalmic communions.

The ad hoc nature of the rest of the annual communion cycle seems to be reinforced by the discovery of Bradford Maiani that fully one-fourth of the Roman communions are actually responsories called upon to do double duty. In addition, McKinnon notes

33 McKinnon proposes an explanation that is aesthetic and theological; see The Advent Project, p. 336.
that some of the syllabic communions ‘do not just resemble antiphons, they are Office antiphons, borrowed, apparently, to fill out the communion repertory as expeditiously as possible’ (p. 338). Lack of the kind of meticulous liturgical planning presupposed by The Advent Project continues to manifest itself in the Paschaltime communions based on texts from the New Testament. The texts of these communions are not invariably echoes of the assigned pericopes. Of the twenty-eight Gospel communions of the sanctoral cycle, for example, only eighteen are derived from the Gospel of the day (Table 35). While McKinnon admits that this practice amounts to a defect, he nevertheless concludes that the strategies of the schola cantorum led to a ‘polished and elegant communion cycle’, but that ‘only the introit can lay claim to having achieved the ideals of the Advent Project without compromise’ (p. 341).

The Advent Project soon came to a halt. Since there is no independent documentary evidence of its existence, the reasons for its abandonment cannot be verified. Musical work did not cease entirely, however, since the sanctoral cycle had yet to be organised.\(^5\) McKinnon opines that the schola broke off its work on the Mass and turned its attention to the music of the Office – an attractive idea, but one that will need further examination, if and when the liturgical relationships between monastics and the secular clergy at Rome can be clarified. Perhaps the double employment of Office antiphons and responsories as communions represents a suggestive link that will lead somewhere.

The long ‘epilogue’ that concludes The Advent Project ponders what Willi Apel called so many years ago ‘the central question’ – the historical relationship between the Roman and the Gregorian chant traditions. In brief, McKinnon regards ‘the Frankish or Gregorian version as closer to the Roman [eighth-century?] original than the extant [eleventh-century] Roman version’ (p. 377). And further:

The overall greater individuality that one observes in Gregorian melodies might very well reflect a corresponding individuality in Roman melodies at the time of the transmission, and that the general stylistic homogeneity of the extant Roman melodies might reflect a blurring of distinctions from melody to melody in the course of centuries of oral transmission. (p. 379)

\(^5\) I should note that one of the book’s most satisfying chapters, not given its due in the present review, is ‘Dating the Mass Proper II: The Sanctorale’ (7).
This is not a new view, but McKinnon argues it with far greater thoroughness than heretofore attempted. The conclusion rests on several presumptions, all of them adumbrated in the earlier chapters but here arrayed in support of the hypothesis.

The Franks presumably intended to copy the Roman melodies as accurately as possible. Indeed, they were under royal order to do so, and there are familiar (and conflicting!) anecdotes about the measure of their success. As we have seen, the author’s estimation of northern cantors’ abilities varies. On one hand, they are overwhelmed by the challenge of absorbing certain parts of the Roman repertory like the offertories, yet elsewhere they are regarded as faithful purveyors of the Roman tradition as they found it.

It is argued that, if the eighth-century Roman melodies were as ‘formulaic’ as they are in the extant Roman graduals, it would be inconceivable that the Franks would not have taken advantage of that fact to lighten their burden of assimilation. Could one not just as well assume that the Frankish cantors, comfortable with the aesthetic canons of Gallican chant, confronted the Roman melodies critically? Might they have had little sympathy for generalised formulaic principles of melodic design, making an exception only for those genres that manifested a logical implementation of formulae like the graduals and tracts?

One of the puzzles about the relationship between the Roman and Gregorian traditions is the virtual absence of shared, identical chants, except for those that were borrowed by the Romans from the mature Gregorian tradition later in the Middle Ages. McKinnon compares a Roman Magnificat antiphon and a Gregorian communion on the one hand with a Roman communion in the typical ‘ornate oscillating style’ on the other. All three have the same text: Pater si non potest. The Roman antiphon and the

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56 Charlemagne did not hesitate to authorise the revision of Roman liturgical books (sacramentary, lectionary, homiliary). It is inconceivable that he would have ignored musical experts, particularly if they had Alcuin’s ear, had they had complaints about the problems of reduplicating Roman chant in the Carolingian realm. In the closing lines of The Advent Project the author alludes to the central role of Metz in the transmission. Sadly, the valuable pages he could have given us on this subject will remain unwritten.

57 This principle is not implemented without exception, however. McKinnon notes that three post-Pentecostal Roman graduals (Ego dixi domine, Liberasti nos, Benedicam dominum) with stereotypical melodies in the Roman tradition are replaced with ‘free’ melodies in their Frankish versions.
Gregorian communion have very similar musical features: they are mainly syllabic with a sprinkling of two- and three-note neumes, but with different finals (F vs. G). The Roman communion, however, has a predominantly neumatic melody. Supposedly, the Roman antiphon–Gregorian communion pair exemplify the musical style of the ‘original Roman Mass Proper’, while the Roman communion exemplifies the end result of several centuries of further evolution.

Curiously, one of the problems with the thesis that the style of Roman chant in the eleventh- and twelfth-century manuscripts represents a transformation of its eighth-century shape is not addressed in The Advent Project. Both Thomas Kelly and Kenneth Levy have shown that the composition of Beneventan chant ceased by the first third of the ninth century.\(^5\) Both Roman chant and Beneventan chant share the ‘ornate oscillating’ style typical of native Italian chant. How can it be then that Roman chant evolved for three centuries, only to resemble a chant repertory that ceased to evolve centuries earlier?

To what extent are the surviving three graduals and the two antiphoners of Roman chant representatives of the ‘same’ chant? Part of the answer must be sought in the obscure history of Roman ecclesiastical institutions, particularly in the as yet undetermined amount of communication and collaboration that might have existed between the monasteries attached to the principal basilicas and the secular clergy assigned to these same churches. During the early Middle Ages monks were not generally ordained to the diaconate or the priesthood, and they would not have performed the pastoral duties incumbent on the secular clergy. (Doubts were even raised in Rome about the compatibility of the monastic state with ordination to sacred orders.) Did the Roman monks keep to themselves in the performance of their duties or were they joined by the secular clergy at some of the hours of the Office?

I would be less inclined than McKinnon to assume that musical influences passed between the music of the monastic Office and the developing repertory of Mass chants. He suggests (p. 196) that the schola cantorum might have modelled the introit on the psalmody of the monastic Office. The lack of textual or repertor-

ial overlap and the dissimilarity of musical style between the Office antiphons and the introit (p. 120) are only two of the difficulties inherent in establishing such a connection. The lack of affinity between the static antiphonal psalmody of the monastic Office and the processional psalmody of the Mass further weaken the likelihood of such a crossover.

During the ‘centuries of silence’ at Rome, how did the urban secular clergy supply music for liturgical and pastoral needs? Did young clerics in the pre-schola days absorb the urban musical tradition as part of their apprenticeship, assisted at a later period by graduates of the papal schola? Except at the Lateran, St Peter’s and a few other Roman basilicas, was there even need for an annalis cantus, an immense repertory of music to be mastered, even taking into account formulaicism, model melodies and multiple uses of the same chant? Did the singer – there might have been only one – assigned to a small church possess only a basic, passepartout repertory, suitable for ordinary occasions of the Temporale and a small repertory for whatever saints’ feasts were celebrated at his church? This situation resembles to some hypothetical extent the ‘pre-schola’ state of the repertory: lists of undifferentiated chants grouped by genre without assignment to particular dates in the calendar. For the greatest festivals, of course, a special, proper repertory would have been required.

Around the middle of the seventh century the newly formed schola cantorum stepped into this situation. Their commission was to reorganise the musical patrimony of the Roman church, primarily for papal services, thus creating the kind of ‘disruption’ that McKinnon posits as the primary factor than can explain striking musical change.

In The Advent Project James McKinnon never shirks from confronting the thorniest questions: he always assumes that they will occur to the reader, and he does his best to offer plausible explanations. Often these can be framed as no more than assumptions or suggestions. The author’s objective in this book – to elucidate the process by which the Proper chants of the Mass were created – is a brave endeavour, a task worthy of the endeavour it seeks to describe, the Advent Project. The latter was the combined effort of a cadre of professional church singers, the former is the effort of a single scholar, completed under the most difficult of circum-
stances. Although the author has unfailingly provided his own explanations, supported by a richness of detail that cannot be reflected in this review, he leaves readers to continue pondering the questions and seeking their own solutions.

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Although this book deals at length with five sixteenth-century theorists (Gaffurio, Aron, Heyden, Glarean and Zarlino), it is not primarily concerned with ‘reading’ their treatises in the conventional sense of explication of text or comparative analysis of content. We should take note of the book’s subtitle; ‘hearing with the eyes’ makes me wince a bit, but it does suggest that special attention is to be given the treatises’ musical examples, and to how they are read now and were read when the books were new. ‘Reading’ examples, and indeed the whole of these treatises, will here refer not to absorption of content but to process, to application of various cultural filters, and above all to consideration of books as objects, as representatives – tactile as well as intellectual – of ‘print culture’.

Part I, ‘Beginnings’, opens with a Prologue including a brief discussion of ‘the nature of musical exemplarity’ (pp. 5, 8–9) in which Judd broaches a theme that is to run through the book: she sees examples as grounded in beliefs, ideologies, ethics and strategies of rhetoric that give them meaning beyond their usually workaday, and generally quite clear, purpose. On the other hand, examples on a page may not really ‘be’ music, since they are soundless. For Judd examples are thus both more and less than what they seem. Perhaps; but they are also what the authors using them intended them as, bits of music that help to define and illustrate points made in the text. In the context of this review I do not apologise for such a simplistic observation; Judd is not at all reluctant to state – sometimes rather ponderously – the self-evident.

Are, or were, musical examples meant to be read aloud, thus to
‘become’ music? Sometimes, surely, though not necessarily; and this would not have been possible for a single reader when faced with polyphony. Judd therefore concludes her prologue by discussing ‘silent reading, silent listening’ (pp. 11–16). Again I think she makes rather more of a problem out of this than is necessary. If we can assume that in the age of the printed book, and indeed for some time earlier, verbal texts could be and often were read silently, what about music? The meaning of words is not dependent on their sound; is the meaning of music – but I must remember that I am not writing a book but reviewing one. Judd does not face the question I just stopped myself from posing. Indeed she says ‘this book implicitly addresses the general question of what it is to “read” music while simultaneously considering explicitly the specific nature of musical exemplarity’ (p. 16; italics mine). At this point I can respond only by making another obvious point: the authors of music treatises clearly assumed that the audience they wrote for could ‘read’ music as well as verbal text. Whether the reading was sounded or silent was immaterial.

‘Beginnings’ continues with a second chapter, ‘Music Theory Incunabula: Printed Books, Printed Music’. A very quick mention of the treatises of Ramis de Pareja (1482) and Nicolo Burzio (1487) is followed by several pages devoted to Franchino Gaffurio, especially his Practica musicae, printed in 1496. Gaffurio’s work is acknowledged to have been highly influential on later theorists; but it is considered (p. 22) as ‘essentially scholastic’ for the odd reason that it quotes ancient authorities. His numerous musical examples make use of woodcuts, to admirable effect; but this, somehow, is seen as marking its origins and character as ‘very much in the world of manuscript culture’ (p. 28). I do not grasp the logic of this; that apart, what is wrong with a connection with manuscript culture? There was in fact a long manuscript tradition of using musical examples in treatises, even of including treatises in manuscript collections of music. Judd’s interest in the effect of printing on music and writings about it is of course an entirely legitimate one, but I do not see why it should lead her to neglect the obvious links between manuscript and printed musical sources; both were after all publications.

The appearance of Petrucci’s prints of polyphonic music is given due recognition (pp. 30–1) in an account marred by a small inac-
accuracy (these occur from time to time throughout the book). The general importance of printed music for Judd’s argument is that it was used as the source of citations and examples in treatises beginning with the work of Pietro Aron. About this Judd, not given to modest understatement, has this to say:

Indeed, the invocation of printed musical sources by theorists betokened an irreversible change in the interplay of music theory, practice, printed repertories, and communities of readers. Re-examining that interplay has major ramifications not only for the interpretation of sixteenth-century musical and printed culture, but also for our understanding of our own relationship to that culture and the ways in which it has shaped us as scholars and musicians (p. 31).

The importance of musical examples to Judd’s thesis is signalled by their looming presence in this second chapter. Of its fifteen pages, ten are given wholly or mostly to examples. Judd likes to quote Glarean (see pp. 117, 121, 169) describing his own Dodecachordon as ‘certainly of monstrous size if one considers the examples, but of no great size at all if one looks at the text’. She took this to heart; of her book’s 320 pages (excluding bibliography and index), 138 are wholly devoted to examples and tables, another twenty-two partly so – amounting to half the book.

Part II, covering the period 1520–40, is concerned with the work of two theorists, Pietro Aron and Sebald Heyden. Chapter 3, ‘Pietro Aron and Petrucci’s Prints’, is essentially the same as an article by Judd published in this yearbook a few years ago. Aron’s Trattato attempts something not previously done – ‘non da altrui più scritti’, as the title-page states – by any theorist: he aims to categorise polyphonic music (‘canto figurato’) by mode, using citations, by title and author, of real pieces. Most of the music cited is drawn from Petrucci prints, chiefly of chansons and motets. Judd did not discover this, and her work is, as she acknowledges, indebted to a study by Harold Powers. But her identification of Aron’s citations is more complete and systematic than that of other scholars, and for this she deserves full credit.

1 Judd says that Petrucci began with single-volume choirbook format but switched to part-books with Motetti C (1504). The first partbook publication was actually Josquin’s first book of masses (1502); and Petrucci continued to use single-volume format for publications of secular music.


Reviews

What is harder to praise is the use to which Judd puts this material. As is the case with the examples in the other theorists she examines, she gives what she calls (following Roger Chartier) an ‘external’ reading to Aron’s citations, stressing ‘the means by which a musical repertory, as defined by its appearance in printed form, not only conditioned, but effectively shaped’ the theorist’s work (pp. 37–8). Could it not be that Aron simply found Petrucci’s prints a repertory conveniently at hand, and hoped that his readers would have access to at least some of this material? No, not for Judd, who sees poor Aron as substituting ‘the authority of a contemporary printed polyphonic repertory for that of the Church as represented by chant, a potentially heretical gesture when thus described’ (p. 48). She goes on to picture Aron as drawing his own stature as a theorist from the supposedly exalted rank of music in print (p. 71 et passim). Less extreme, and more convincing, is her view that Aron’s reading of Petrucci volumes (he seems to have looked mainly at tenor parts) is ‘personal, silent, and visual, in marked contrast at times to the aural reality represented by the notation’, and that for him the music in print ‘became an authoritative, visual text that need not necessarily be the object of performance’ (p. 61). How does she know this, one might ask. Without splitting hairs, one might reasonably answer that she could not ‘know’ it; she is giving an opinion stated, as one finds very often in the course of the book, in such a way that it appears, or at any rate means, to carry the force of fact.

Chapter 4, bearing the ungainly title ‘Music Anthologies, Theory Treatises, and the Reformation’, is devoted to Nuremberg in the period 1530–50 and to the theorist Sebald Heyden. The latter is known to modern scholars chiefly for his theories of mensural simplification. To these Judd devotes a single footnote (p. 94); as with the other theorists she considers, she is intent on an ‘exterior’ reading of the musical examples in the three versions (1532, 1537, 1540; each differently titled) of Heyden’s De arte canendi. Heyden, a devout Protestant in the religiously divided city of Nuremberg, used textless polyphonic examples in the second and third editions of his work, material for students to use in mastering the art of mensural music as part of their general education, rather than for use in the liturgy.

Saying that Heyden, unlike Aron, could not count on his read-
ers having access to a polyphonic repertory and thus could not simply cite musical titles (p. 95), Judd refers to the preface of the 1537 version of his treatise, where Heyden acknowledges a Nuremberg patrician named Ulrich Starck as the source of his examples. She provides a table showing that most of these are contained in Petrucci prints, but the circle linking Heyden to Aron cannot be completed. Judd’s confession that ‘it is difficult to assert with complete confidence that Heyden was working from Petrucci examples’ (p. 101) might be rephrased as admission that printed music may have played no role here. By 1540, however, Heyden could draw on printed mass and motet anthologies published by the Nuremberg printers Ott and Petreius, the presumed source of many of the examples added in the final version of his treatise. In the context of this book the most interesting thing about Heyden’s examples is that they were borrowed for use by subsequent theorists, including Heyden’s contemporary Heinrich Glarean.

The Dodecachordon (1547) of Glarean is the subject of chapters 5 and 6, forming Part III. This and Part IV, dealing with Zarlino, are the book’s gravitational centre; here Judd expands upon her notions of ‘exemplarity’, and she tries to give a rounded picture of these two major figures. She begins by describing the Dodecachordon’s imposing physical appearance, saying (p. 120) that it resembles humanist books such as the Adagia of Erasmus more than it does a typical music treatise of the period. This is supported, for Judd, by the book’s rhetorical style and use of examples, which she aims to study in a way that ‘radically revises the view of a treatise that has been studied . . . primarily as a self-contained theoretical examination of mode and a repository of repertory’ (ibid.). Whether she succeeds in such a revision is, I think, very much open to question; what is not in doubt is the self-importance of her claim, a note sounded with irritating frequency in these chapters.

Other scholars, beginning with Arnold Schering and continuing with Clement Miller and Frans Wiering, have seen Glarean’s examples ‘in the sense that they most obviously function in the text: as illustrations of the mensural and modal precepts Glarean

\[\text{\small 4 In a footnote on the same page Judd states that Ulrich Starck married (in 1513) Katherine Imhoff, a member of the Nuremberg family to which the celebrated scribe Petrus Alamire presumably belonged.}\]
describes’ (p. 122). This ‘overt’ reading is not exactly denied truth, but it is steadily pooh-poohed; esoteric data are not to be compared with the esoteric meanings uncovered by Judd. What are these meanings? In large part they consist of aligning Glarean’s choice and arrangement of examples with the humanist aims of Erasmus, in particular those set down in the latter’s De duplici copia verborum et rerum commentarii duo (1512). Erasmus recommends the use of notebooks or commonplace books for recording passages with an eye to future use in strengthening both the technique of one’s argument and the moral fibre of one’s writing. While admitting (p. 129) that many music notebooks might not rise to Erasmian standards, Judd thinks that a claim can be made that Glarean gathered and ordered his exempla with methodological and moral purpose that makes them aspire to such a level, to become ‘the stuff of rhetoric itself’ (p. 130). His procedures in giving examples to illustrate his twelve modes are explicitly compared with passages in De duplici copia; an example is Glarean’s concentration on four-voice polyphony, accompanied by the commonplace analogy to the four elements. Erasmus remarks that ‘the four elements, so very unlike and even hostile one to another, are nevertheless the essence of primal matter’ (quoted on p. 136). The reader may decide whether this remark transfers easily to a consideration of four-voice harmony.

What about the ‘moral element’ in Erasmus’ discussion of exempla? Judd, who sees Glarean as a strong Catholic apologist, cites (pp. 147–8) the theorist’s prefatory letter in a set of manuscript partbooks of motets copied by his student Martin Besard (these partbooks are one of the ‘notebook’ sources of the examples in the Dodecachordon). This letter defends the moral worth of singing ‘ecclesiasticas cantilenas’ and attacks those (presumably Calvinists) who oppose the practice. Following the excellent argument about Glarean’s religious ideology presented by Sarah Fuller, but as elsewhere in the book extending it – here, I think, stretching it to unconvincing lengths – Judd claims that the polyphonic examples of the Dodecachordon have moral weight, are exhor-

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5 Munich, Universitätsbibliothek MS 8° 322–25. Another Glarean source is St. Gall, Stiftsbibliothek MS 463, compiled by Aegidius Tschudi, another of his pupils.
tations to the reader to use them as a means to increase religious fervour. Speaking of the contents of another of Glarean’s sources, the Tschudi manuscript, arranged in part in modal order, Judd sums up her view with confidence, if not perfect clarity:

He [Tschudi] chooses which works to include in his Liederbuch precisely on the basis of their modal identity and the structured order planned for the Liederbuch. This is actually not a surprising turn in an Erasmian understanding of the Dodecachordon, because with its implicitly moral basis, Glarean’s modal theory confers moral status upon exemplars which are read by means of his model (p. 174).

There is much useful information in the chapters on Glarean. Judd gives a clear picture of the sources for the Dodecachordon’s examples, including Petrucci prints and manuscript copies made from them; examples from Gaffurio’s Practica and Heyden’s De arte canendi; and a few short pieces commissioned by Glarean. Most of this work had, she acknowledges, already been done by other scholars; still, this is a good and informative summary. As with the other theorists treated in this book, Judd does not engage with Glarean’s subject matter. Her main concern is with her theories of exemplarity; if the reader accepts these the pages spent on them will seem well worth their length.

The career of Gioseffo Zarlino, centred on Le istitutioni harmoniche (1558), is the subject of Part IV, chapters 7 and 8. Judd makes two points about Zarlino that seem to me of considerable interest. One is that both the nature and the chronological spacing of Zarlino’s publications of music and of theory treatises, over a period of forty years (1549–88/9), were to an unusual degree calculated, serving to advance his career and to establish his reputation as both theorist and practical musician. Here is one statement of this theme, an oft-repeated one in these chapters:

There is a regular alternation between the publications of compositions and theoretical works that ultimately favors the theory treatises: motets in 1549, Le istitutioni harmoniche in 1558–62; a book of motets in 1566 and several madrigals in anthologies in 1562–67; the Demostrazioni and a revision of the Istitutioni in 1571–73; and the Sopplimenti and complete works in 1588–89, a year before Zarlino’s death. . . . By following his printed traces, I will illustrate how Zarlino used publication as a means of not only enhancing, but shaping, his public image; the evidence suggests that he was a masterful manipulator of his printed persona (pp. 185, 188; cf. pp. 180, 184, 188, 192, 208, 250).

7 On these commissioned pieces see H. S. Powers, ‘Music as Text and Text as Music’, in H. Danuser and T. Plebuch (eds), Musik als Text, 2 vols. (Kassel, 1998), i, pp. 20–6.
This is not a self-evident proposition. Zarlino was not, at least in print, a prolific composer; there are only two books of motets and a few pieces scattered in anthologies. Among theorists, both Gaffurio and Tinctoris probably wrote more music (but did not of course publish it). Still, its existence was useful in establishing his claims as a practising contrapuntist, claims he bolstered through frequent citations of his own music in the *Istitutioni*. That he was a genuinely important theorist is beyond question (I do not think Judd emphasises this strongly enough), but equally undeniable is the boost that publication and republication gave his reputation. Zarlino took full advantage of ‘print culture’, and it served him well.

The second point of real interest that I find in these chapters is Judd’s observation that both the 1549 motet volume and even *Le istitutioni* show Zarlino’s adoption of Glarean’s twelve-mode system to be so recent as to betray signs of being imposed on a traditional eight-mode concept. The chief evidence for this is his patched-together treatment of Glarean’s Lydian (F–f, no flat) and Ionian (C–c, or F–f with a flat) modes (see pp. 217, 222–5).9 This suggests that Zarlino did not arrive at his twelve-mode system independent of Glarean’s work (we know in any event that he read the *Dodecachordon*) and also that *Le istitutioni harmoniche* must have been written over a long period, ten years or more, before its publication (Judd does not say anything about this).

As with her treatment of Glarean, Judd shows little interest in Zarlino’s thought, not even bothering to mention his central concept of the *senario* and its effects on subjects as diverse as tuning systems and modal ordering. Her concern is once again with ‘exemplarity’; but here Zarlino is less obliging than Glarean. The musical examples in Books III and IV of the *Istitutioni*, illustrating contrapuntal precepts and modal propriety, are of Zarlino’s own invention; otherwise he limits himself to citations of works by ‘ancients’ (mostly Josquin) and ‘moderns’ (above all Willaert,

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8 Judd refers to the 1549 print as *Musici quinque vocum*, a very confusing abbreviated title; the next word in the title is *moduli* (motets), the noun to which ‘musici’ stands as adjectival modifier. In this print Zarlino identifies pieces by mode, using Glarean’s Greek names (dropped in *Le istitutioni*).

9 There is also Judd’s discovery that a group of motets based on the biblical Song of Songs, scattered in the 1549 volume and in two anthologies, shows adherence to an eight-mode system. The results of her investigation of this music are soon to be published.
chiefly his *Musica nova*, but including many references to Zarlino himself). Judd talks about the musical examples in some detail. It is a pity that she ignores the beautifully executed diagrams – surely as important as examples – gracing Books I and II of the treatise. She even makes a few descriptive remarks about what she refers to (quotes included; see p. 216) as the ‘music itself’. And she lists the known and putative sources for the citations, spending what is to me a rather wearisome number of pages in the process.

One or two details in these chapters might be singled out. Judd strongly emphasises Zarlino’s reliance on his teacher Adrian Willaert, as ideal composer and musical thinker and as the source of his own authority as theorist (pp. 179–80 *et passim*). In doing so she asserts (p. 189), citing the work of Martha Feldman, that Zarlino’s treatise was written inside a ‘Bembist framework’, with Willaert as single model taking the place of Pietro Bembo’s literary model Petrarch. I think this is an exaggerated view. The first two books of *Le istitutioni harmoniche* are surely not indebted to Willaert; nor, for that matter, is the twelve-mode system of Book IV. Zarlino’s central concept, the *senario* and the system of just intonation derived from it, comes not out of the teachings of Willaert but from the work of the Modenese theorist Lodovico Fogliano, whose *Musica theorica* was published in Venice in 1529. Zarlino knew Fogliano’s work and said approvingly of it that ‘he [Fogliano], having perhaps considered what Ptolemy left written on the syntonic diatonic, took the pains to write a Latin book on the subject to demonstrate the true proportions of the intervals involved’. Among the music titles contributed by Zarlino to the list of books the short-lived Accademia Veneziana (Accademia della Fama) intended to publish is a translation of Fogliano’s work: ‘Ludovici Folliani musices theorice a latino in italicum sermonem verba cum quibusdam annotationibus ad confrirmandum auctoris

10 Ex. 8.1, pp. 230–1, is a transcription of the duo illustrating mode 3 in the 1558 edition of *Le istitutioni*. The example is covered with letters, numbers, arrows and diagonal lines in distracting boldface type, serving to mark points of imitation and cadences in a perfunctory and not altogether accurate way.


sententiam’ (p. 197, n. 42). Presumably the annotations were to be the work of Zarlino himself.

On Zarlino’s compositional career Judd has predictably little to say, preferring to consider his published music as part of his scheme of self-advancement through the medium of print. A couple of motets from the 1549 volume are briefly looked at in reference to their modal designation (indicated in the print itself). Some less than complimentary general judgements by twentieth-century scholars are passed along (p. 184), and the work of several musicologists on the 1549 motets is referred to (pp. 206, 216). Her conclusions about Zarlino’s music are bleak: ‘From the interpretation of Zarlino’s prints that I offer below, one senses that despite his self-promotion, he spent his early career at San Marco responding to precisely this perception: that while he was a man of great learning, he was but a second-rate composer’ (p. 185). It might be of interest here to note what Zarlino purportedly had to say about his own music. In his *Prattica di musica, seconda parte* (Venice, 1622), Lodovico Zacconi, writing of an earlier period (which he designates as 1584), discusses seven aspects that distinguish *musica armoniale*. Expertise in at least some of these (having to do with melodic gift, contrapuntal skill and good sound) will set a composer apart, indeed make his name. He then continues:

I remember that in the year 1584 there was a conversation on music one day held in the presence of many musicians gathered before Don Gioseffo Zarlino; and as the talk turned to the style of this and that composer, he [Zarlino] made the above distinctions and then came down to particulars, saying, ‘What would you have me say? He who has one of these lacks another, and even he who is distinguished in two or three cannot have them all. My own genius (he said, speaking of himself) is given over to regular *tessitura* and *arte* . . .

What Zarlino meant by *tessitura* and *arte* seems to be the practical skill and theoretical knowledge of counterpoint. He does not claim for himself the more beguiling qualities of *diletto* and *buona disposizione*, but neither does he condemn himself to mediocrity. There is no reason to think that his contemporaries did so behind his back.

I think the book might well have ended at this point, or with a few concluding added words. Judd has, however, appended a Part

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V, ‘Readings Past and Present’, consisting of a single chapter, ‘Exempli gratia: Reception History of *Magnus es tu domine / Tu pauperum refugium*’. The latter is a motet, attributed – not very definitively – to Josquin, and taken by Glarean from Petrucci’s *Motetti C*, where it is anonymous, for inclusion in the *Dodecachordon* (by way of the manuscript copies mentioned earlier), where it is firmly ascribed to Josquin. Judd finds its inclusion in the treatise appropriate on the grounds, mystifying to me, that its text contains themes which are ‘exactly those Glarean invoked in the letter which heads the [Munich partbooks] collection’ (p. 273). After discussing the mode of the piece and its odd reception by musicologists of the late nineteenth and early twentieth centuries (they ignored the motet’s comparatively severe first half or even questioned its belonging with the universally praised *Tu pauperum refugium*, chordal and symmetrical, showing the ‘Italian’ side of Josquin), Judd proceeds to give a post-*Dodecachordon* publication history of the motet, or rather of its second part. *Tu pauperum refugium* was published in Friedrich Rochlitz’s *Sammlung vorzuglicher Gesangstücke* of 1835, was transcribed though not published by Ambros, and appeared in two more nineteenth-century practical editions. It was reunited with *Magnus es tu domine* in 1924 in Albert Smijers’s Josquin edition. Two more German editions, one by Heinrich Besseler, followed; then, in 1946, *Tu pauperum refugium* appeared in Davison and Apel’s *Historical Anthology of Music*.

This is interesting enough, though its connection to the rest of the book seems to me slender – despite the author’s repeated assertions of its relevance. Next Judd turns back to theorists, but to twentieth-century ones, whom she sees as drawing on *HAM* as Aron and Glarean had drawn on Petrucci. Between 1969 and 1978 three analyses of *Tu pauperum refugium* were published.14 Judd considers all of them and finds them all – whether Schenkerian (Salzer and Schachter), ‘neo-modal’ (Berry) or motivic (Joseph) – wanting. This is not surprising given her generally dismissive attitude throughout the book towards most earlier scholarship on subjects it touches.

The story of *Magnus es tu domine / Tu pauperum refugium* is not over, however; and it ends happily. Judd presents an analysis of

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her own, which she calls, for some reason, ‘a dialogic reading’ (p. 302). She offers a kind of apology – I think – for intruding her own analytical voice: ‘the impossibility of standing outside my historical moment would make it presumptuous, if not impossible, to place this reading in a simple narrative continuum, as though I were not as susceptible to the contingencies of the materiality of the text and inherited conventions of exemplarity as my theoretical predecessors’ (pp. 302–3). Nevertheless she means her analysis to supersede those she has just discussed; and in at least one aspect it does, since she considers the whole motet and shows tangible connections of style and musical material between the two halves. I will leave the details of Judd’s analysis to the judgement of the reader. In general I think she does it well, and am only surprised that she seems not to notice that she is here departing from the non-analytical stance she proclaims throughout all the rest of the book.

In a very brief epilogue, ‘Reading Theorists Reading Music’, Judd summarises the book’s content and tries, not to my mind very successfully, to show what she means by ‘dialogic’, which she is, predictably, using in a Bakhtinian sense (p. 320). How should I summarise this review? The material covered in the book is of considerable interest, but for me its central thesis, the multiple resonance of ‘exemplarity’, as concept and indeed as historical force, remains unclear. I think Judd is straining too hard to give ‘meaning’ to what is basically a quite positivist historical investigation. Part of the problem is in her choice of language. Certain terms, such as the long-suffering ‘mediation’, simply appear too often and with too many demands made on them. Almost every buzzword in the postmodern thesaurus may be found in these pages, and sometimes they are crowded together uncomfortably, as in the following sentence (pp. 257, 261): ‘Zarlino’s work provides a specific instance of the significance of citations and notated music examples embedded in discursive writing about music that is emblematic of a reflexive intertextuality of theory and repertory.’ Judd finds her own work significant and charged with meaning, and frequently says so. Apart from whether this judgement might better be left to the reader, these constant reminders of the importance of her treatment can lead to an unhappy impression of over-interpretation, even of self-importance. I do not for a moment
think that Cristle Collins Judd is guilty of self-importance but I do think that she over-interprets her material. For me this lessens the impact of what in many ways is an admirable book.

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