Ernest H. Sanders—*Medieval English Polyphony and its Significance for the Continent*

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The amount of scholarly work on medieval English music published since the 1920's is quite daunting in its sheer bulk, and production in post-war years has increased to such an extent that one can hardly call the subject neglected. Articles and monographs have, however, tended to concentrate on isolated questions; they leave one conscious of the need for a study which would treat the subject as a whole. Dr. Harrison's invaluable *Music in Medieval Britain* did not entirely meet this need, for while it deals extensively with other matters, the account it gives of English polyphony from 1100 to 1400 is a mere 50 pages long—less than an eighth of the whole book. This dissertation does not supersede *Music in Medieval Britain* (it will be a while before anything does), but complements it, offering a detailed account of English polyphony in the 13th and 14th centuries. The work is all the more acceptable because it has been done with exemplary care, thoroughness, and responsibility. Sometimes Dr. Sanders permits himself an aside:

> It is extraordinary how the field of English medieval music particularly is littered with the fractured bones of theorists, who, for the sake of a theory, are presumed to be of limited competence (p. 314, f.n.).

We may not be too happy about the metaphor, but it is reassuring to know that one is dealing with an authority who will give due weight to primary sources. A willingness to do so is perhaps the great strength of the dissertation, for although Dr. Sanders has obviously profited from the work of Apfel, Bukofzer, Dittmer, and other investigators, he is not afraid to brush their conclusions aside when they have too tenuous a relevance to the facts as presented by the music. One very pleasing result is the excellent editing of Latin texts, on which Dr. Sanders rightly prides himself.

I shrink from summarizing the complex argument of Chapter I, which discusses the rhythmic ambiguity of certain early English pieces in mensural notation; Dr. Sanders' conclusion, based on a close study of theoretical and paleographic evidence, is that these doubtful pieces indicate an English variant of the third rhythmic mode, which he terms "alternate third mode": ♩. ♩ ♪ rather than ♩. ♪ ♩ or ♩ 𝅘𝅥𝅯𝅘𝅥𝅯. His reconstruction of the "irregular modes" of Anonymous IV seems, on balance, more convincing than Dittmer's, but (as Dr. Sanders acknowledges) there can be no absolute solution to a problem of this kind. His argument—inevitably—leaves one without a criterion for deciding which rhythm to apply in transcribing any of the

numerous English pieces which appear to go well in either duple time or the alternate third mode. I myself share his preference for the latter, although I cannot agree that his transcription of *Campanis cum cymbalis* (pp. 106–07) is better than Dittmer's binary transcription.

Be that as it may, Dr. Sanders deserves credit for having refuted the view that *Sumer is icumen in* was originally in duple meter. One might previously have had objections to the extremely awkward articulation of the text produced by Bukofzer's transcription in $\frac{4}{4}$, but there remained the undeniable fact that the original notation showed pairs of breves. Dr. Sanders has now made it clear that such pairs can be read ♩ ♪. It is gratifying to find one's instincts corroborated by someone else's scholarship.

The second, third, and fourth chapters consist largely of analysis of polyphonic techniques used by English composers of the period. All the analyses are impressively carried out, and it would be difficult to fault them as such; but they are not all equally relevant. Chapter II, for example, on the Worcester Fragments, is invaluable, demonstrating, *inter alia*, that the tenors, or *pedes*, of freely composed pieces in the Worcester repertory are remarkably similar, particularly the *ostinato* tenors in F, and that some of them may have been taken from popular song. It shows, too, that the English practice of altering a *cantus firmus*, which begins in the 13th century, was adopted in the interests of what Dr. Sanders calls "tonal unity." On the other hand, the following chapter (c. 1285–1325) strikes me, after repeated readings, as being merely a collection of technical analyses with little wider purpose; its findings would be obvious to all if a complete performing edition of the music were available. Similarly, Chapter IV opens with a useful reconsideration of the terminology used by scholars in discussing 14th-century English music (gymel, conductus, etc.,) but ends with a superfluous rejection of Bukofzer's "English discant" theory—superfluous because that theory has already been convincingly refuted by others. It is a pity, too, that fuller treatment was not given to the eleventh fascicle of MS Wolfenbüttel 677, especially in view of Dr. Sanders' interesting suggestion that the second Notre Dame generation was strongly influenced by English music; a closer examination would have further strengthened the first part of Chapter V, which makes a useful distinction between Continental works which show English influence and works of English origin in Continental sources.

In general, Dr. Sanders' dissertation is an admirable example of its genre. One's criticisms therefore center not on the question of his competence, but on the limitations of the genre itself. What one misses is any sense of the relation between the music and the culture which produced it and gave it purpose, as if music, in the Middle Ages of all periods, existed in a vacuum. Indeed, when he ventures outside the field of musical technique, Dr. Sanders runs into trouble; on p. 95 (f.n. 49) he suggests that the addition of the Latin text to *Sumer is icumen in* may have been due to Franciscan influence, a statement which betrays no more intimate knowledge of Franciscan literary activities (which consisted chiefly of converting secular material into religious

poems *in the vernacular*) than can be picked up at second or third hand from *Music in Medieval Britain* and the *New Oxford History of Music*. Besides, the other contents of MS Harley 978 show that the song belonged to quite another area of medieval culture.

The limitations of the approach associated with the term "history of music" are most evident in the concluding half-chapter. Its main contention is plausible enough: because of the preference for major tonality, for tonal unity, and for the consonance of the third characteristic of English medieval music, England may be regarded as the home of Western tonal music. But Dr. Sanders insists on taking an evolutionary view which he has to bolster with a long and somewhat specious argument, a large part of which appears between quotation marks. One highly questionable feature is the emphasis he gives to the role of so-called "instrumental elements" in the evolution of tonal harmony; it ignores what is perhaps the most important single aspect of medieval music, namely, the fact that it is essentially rooted in the possibilities and limitations of the human voice. If, moreover, one were looking for exceptions to the general rule (no chromaticism, avoidance of unvocal intervals, etc.), one would turn not to English music, but to the work of Machaut and his French contemporaries.

The fact is, of course, that the idea of evolution in music is at best a metaphor from biological science, and at worst a gross oversimplification of the ways in which human sensibility manifests itself; it has long been recognized as unhelpful in the other arts. In Dr. Sanders' evolutionary scheme, 18th-century tonality represents a kind of zenith in European music; he may well be right, but the two inferences most naturally drawn from his concluding argument are (a) that a composition is only interesting as a rung in the ladder of evolution and (b) that any tonal composition is better than any modal one. I refuse to believe that so capable a scholar as Dr. Sanders accepts either of these absurdities; but there they are, inescapable side-effects of a muddled argument which is patently lacking in the qualities shown by the rest of the dissertation.

Some minor blemishes, remarkable in a work which maintains so high a standard of competence, must be pointed out. They are all avoidable characteristics of a lot of musicological work on medieval subjects. There is, for example, an overconfident misuse of such words as "certainly," "doubtless," and "unquestionably" when probabilities, possibilities, or even hypotheses are being discussed. One has qualms about blaming Dr. Sanders for a trick which seems endemic among medievalists in all disciplines, but it *is* a trick, even if the writer is unaware of it—a trick to blackmail the reader into feeling as convinced by the writer's theory as he is himself.

I do not expect to find a sympathetic audience for my view that sigla, over-abbreviated titles, and the like are pedantic paraphernalia, but I am convinced that these devices (outside a bibliography or catalogue) hardly justify themselves. They are supposed to save space —though there are plenty of pages in this dissertation which have large blank spaces between text and

footnotes—but they decidedly do not save the reader's time. Even more annoying is Dr. Sanders' reluctance to refer to a composition by its title or incipit; instead, he gives the number of the piece in its manuscript or appropriate edition, so that unless the reader has at hand a fair number of editions of Continental and English music, he has often no means of knowing what composition is being cited. After searching through an edition only to find that the piece cited has been in one's performing repertory for years, one may be forgiven for regarding this practice as an attack on sanity.

Finally, I must warn intending readers (who will, I hope, be numerous) of two nuisances for which Dr. Sanders is not responsible. On the film, several musical examples are partly illegible; and pp. 321 and 332 are missing. The latter annoyance seems quite unnecessary; if frames informing us that a page is missing can be inserted into the film, at the point where the page should be, why cannot those frames show the pages themselves?

PROFESSOR SANDERS REPLIES:

Perhaps the most complimentary comment in Mr. Maddrell's thorough and largely favorable review is that my dissertation complements as singularly important a work as Frank Ll. Harrison's *Music in Medieval Britain*. I am somewhat taken aback, therefore, by his criticism that it fails to convey "any sense of the relation between music and the culture which produced it and gave it purpose." For any doctoral candidate in 1961 or so to have set himself such a task would have involved a great deal of presumptuous duplication of the subject matter of Dr. Harrison's book. Moreover, its execution would have had to be slipshod, since the stated aim of this dissertation was to provide a first comprehensive as well as analytically detailed investigation of all extant English polyphony of the 13th and 14th centuries and to assess and define its varying influence on Continental composers. In its conception, orientation, and execution a dissertation often differs markedly from a book. The author of this dissertation found it desirable to limit himself to the examination in depth of a large, relatively homogeneous, and insufficiently explored topic.

These considerations explain the plenitude of what Mr. Maddrell calls "technical analyses with little wider purpose." If one proposes to bring a body of music into focus, one has to analyze it. That the resultant findings were not truisms to be taken for granted is easily proved by a critical reading of much of the relevant musicological literature published prior to c.1960. Moreover, the very fact that most of the music was not available in reliable performing editions indicated the desirability of providing ample and detailed substantiation for the conclusions, which were neither commonly known nor obvious, e.g., the striking diversity of motet types in medieval England, such as the variation motet and the rondo motet; the circumstances that helped bring about the placement of the tenor as cantus-firmus carrier in the middle of the polyphonic fabric; the clarity with which the English repertoire demonstrates isorhythm to have originated as a means of underscoring the strophic phrase organization of motet superstructures, etc. Similarly, the rejection of Bukofzer's "English discant" theory did not strike me as superfluous. While Mr. Maddrell claims that it had already been refuted by others, the only substantial contribution to a very beclouded

field was an article ("'English Discant' and Discant in England," *MQ* (1959) 45:26–48), in which the author, Sylvia W. Kenney, uncovered Bukofzer's misreading of the theoretical sources, which in any case postdate the repertoire in question. It was the detailed examination of this repertoire that showed Bukofzer to have subjected the musical evidence to a twofold misinterpretation. A further important consideration is that painstaking examination of the sources necessarily confers a specialized expertise,[1] which, in turn, gives rise to evaluation and conclusions. It seems to me that a reviewer cannot very well accept some of these, while rejecting others without presentation of commensurate evidence to the contrary. Thus, Mr. Maddrell is gratified that my discussion of the rhythmic interpretation of 13th-century English notational devices corroborates his instinctive musical attitudes toward the Summer Canon, but sees no need, presumably for similar reasons, to reject a method of "binary transcription" of *Campanis/Honoremus*. That it must, in fact, be discarded is overwhelmingly indicated by the pre-Franconian notational evidence.

There is, however, a larger issue that is implicit in the twin complaint about what might be called lack of ethnomusicologically relevant treatment on the one hand and the pervasiveness of "technical analyses" on the other. The globalization of Western culture has bestowed on the discipline of ethnomusicology an entirely novel and embracing importance, which gives the demand for "anthropomusicological" investigation of Western music greater relevance than ever before. Yet, I should like to take this opportunity to emphasize as strongly as I can the specific tendency of the Western tradition towards divorcing music from its intrinsic ancillary functions as an ingredient of processes, and, concomitantly, toward the view of music as an autonomous art, whose purpose is the conception and facture of finite products, of structured objects, by means of notation. This seems to me to be the unique (and ethnologically significant) glory of Western music; it follows that the careful, loving, and expert analytical exploration of any part of this huge repertoire is inevitably a primary musicological obligation, as it reveals the concepts that gave rise to any given graphic fixation.

The medieval view of *ars musica* is elucidated by the writings of the contemporary teacher-reporters ("theorists") and by the works of the composers. A dissertation dealing with aspects of this tradition necessarily has a predetermined focus, and Mr. Maddrell rightly stumbles over the irrelevance of my suggestion that the addition of the Latin text to the Summer Canon may have been due to Franciscan influence, even though it is appropriately buried in a footnote. The fact that as an *obiter dictum* it is not substantiated or developed need not, however, be taken to indicate a lighthearted disregard of our knowledge of Franciscan activities in 13th-century England. The Franciscans strove to spiritualize and "re-Christianize" all social groups with which they made contact, including the monastic and secular clergy. While it is true that a large part of their efforts was directed to the laity and therefore produced religious poetry mainly in the vernacular, there is sufficient direct and indirect evidence that in the proper environment the influence of the Franciscan spirit may well have caused a secular song to be equipped with a secondary sacred text in Latin. I should think that the contents of the Harleian manuscript would support the notion of the reformation of a secular piece under such circumstances.

One major section of the dissertation (its final half-chapter) does go beyond the description and evaluation of the immediate evidence, since it attempts to address itself to the larger implications of the significance of medieval English polyphony for the Continent.[2] I am distressed that Mr.

Maddrell misunderstood two salient thoughts expressed in that chapter. (1) It was (and is) my contention that the genesis of counterpoint required a rational ordering of the tonal material, i.e., a process that loosened the primal bonds tying pitch to language by considering pitch as a separate entity to be defined precisely (numerical ratios). This is impossible without the intercession of essentially instrumental factors; the voice can be "fretted" only after the model of a string, e.g., the monochord. This notion of an instrumental seedbed as the basis for the proper conception of polyphony obviously has nothing to do with the manner (usually vocal) in which any given medieval composition was performed. Significantly, music, no matter how it was realized in performance, was in the Middle Ages generically referred to as "*musica instrumentalis*." (2) It seems to me that a teleological view of the history of Western music is defensible. The advantage of historical hindsight is that, without falling into etiological traps, it can and must at least suggest, if not plausibly demonstrate, curves of evolution; indeed, Mr. Maddrell grants that I "may well be right." However, that meliorism has nothing to do with this position seemed to me so manifest that I quite spontaneously refrained from belaboring the point in the confident expectation that an evolutionary view would not become alloyed with valuational connotations. The inferences (a) and (b) that Mr. Maddrell nevertheless draws from the concluding chapter of the dissertation are patently unjustified.

NOTES

[1] To say so is no proof of pride; unless expertise is to be taken for granted, there is no point to academic publishing.

[2] It has appeared in *AfMW* (1967) 24:24–53 ("Die Rolle der englischen Mehrstimmigkeit des Mittelalters in der Entwicklung von Cantus-firmus-Satz und Tonalitätsstruktur").

Editorial note: Our readers may examine several of Dr. Sander's central theses in the following articles derived from his dissertation: "Duple Rhythm and Alternate Third Mode in the 13th Century," *JAMS* (1962) 15:249–91; "Peripheral Polyphony of the 13th Century," *JAMS* (1964) 17:261–87; "Tonal Aspects of 13th-Century English Polyphony," *Acta Musicologica* (1965) 37:19–34; "Cantilena and Discant in 14th-Century England," *Musica Disciplina* (1965) 19:7–52; "Die Rolle der englischen Mehrstimmigkeit des Mittelalters in der Entwicklung von Cantus-firmus-Satz und Tonalitätsstruktur," *Archiv für Musikwissenschaft* (1967) 24:24–53.