

treating the octave plus fourth. The consonance or dissonance of an interval, however, is a relative matter, dependent upon both sensory perception and reason or system. The consonant character of the octave plus fourth—essential for Barker's argument—is in no way certain, and the issue was hotly debated in musical treatises throughout antiquity and the Middle Ages. Certainly the Aristoxenians and Ptolemy deemed the interval to be consonant, but Ptolemy's criticism of the Pythagoreans for their rejection of this interval from the category of consonance indicates that ancient theorists were less than unanimous on the matter (Düring 13). Unlike the *Sectio*, both Plutarch and Boethius explicitly reject the octave plus fourth because it is dissonant.²¹ For some Pythagoreans, the interval sounded dissonant because its numerical characterization not only included a number, 8, not found in the tetractys, but also fell into the multiple superpartient variety of ratio, i.e. the variety furthest removed from the beauty of unity and equality. The orthodox Pythagorean position on the matter is exactly opposite Barker's: the octave plus fourth *sounds* dissonant because all consonances are either multiple or superparticular.²²

In conclusion, we see how important it is to keep the Pythagorean tradition in mind when reading *Sectio Canonis*. That the introduction provides a footing, albeit shaky, for the construction of Pythagorean musical theory goes without question. But as I have shown, this foundation needs to be supported with additional Pythagorean dogma regarding the tetractys. Furthermore, the entire treatise must be read with the two-octave system in mind.

The style and language of the *Sectio* are like those of Euclid's *Elements*, and there can be hardly any objection to calling the musical treatise 'Euclidean'. There is a great danger, however, in expecting from the *Sectio* a pure and general theory of acoustics similar to Euclid's treatment of geometry. In Euclid's *Elements* we find an abstract theory of geometric and arithmetic truth that can be applied impartially to the physical world. With the *Sectio*, the distinction between corporeal and incorporeal, be it between sound and number or sound and line, is not clear nor, I think, was it intended to be. The relationship between number and sound was both a miracle and a mystery that wowed the Pythagorean mind and ear. An appropriate response to this relationship was to demonstrate the mysterious rather than to deduce the obvious.

By modern standards for theory, even by the standards set by Euclid's *Elements*, the *Sectio* falls flat on its face. I believe, however, that one must read the *Sectio* from a Pythagorean point of view. The Euclidean style of the treatise notwithstanding, one does better to approach *Sectio Canonis* with Nicomachus or Theon of Smyrna in mind rather than Euclid.

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²¹ Boethius, *De Musica* ii 27, and Plutarch, *On the E(psi)lon at Delphi*, *Mor.* 389d–e.

²² See my 'The consonant eleventh and the expansion of the musical tetractys: a study in ancient Pythagoreanism', *J. Music Theory* (forthcoming).

Signa tabulae priscae artis

The article 'Signa priscae artis: Eretria and Siphnos' in *JHS* ciii (1983) 49–67, by David Francis and Michael Vickers (hereafter 'FV'), is part of a programme of investigation of 'fixed points' in Archaic archaeological chronology, the tendency of which is to demonstrate that the conventional chronology is some half-century wrong. This broaches various problems of wider significance, not made explicit in the article and not considered here. The present Note considers the article alone, since some features of the content and manner of the arguments give ground for concern. It is written mainly as a guide to students who may have been puzzled or impressed that such radical new views could be published so confidently. Briefly, FV argue that the Temple of Apollo at Eretria should be dated to the 470s, and that, although Herodotus places the Siphnian Treasury at Delphi c. 525, we ought to be happy with a date in the 470s for this building also.

1. The Eretria Temple

The argument is simple. Herodotus says that the Persians burnt Eretria's temples in 490. The latest temple of Apollo Daphnephoros on the site is the marble one with the sculptures surviving from one pediment. Since inscriptions show the continuation of cult there after 490 the temple must have been constructed after 490 (in fact after the Persians left Greece finally in 479) and was destroyed only in the Roman sack of 198 when the attackers found little wealth but 'signa tabulae priscae artis ornamentaque eius generis' which they carried away. One of the temple pediment figures (an Amazon) has been found in Rome. I observe:

(i) 'Many scholars now accept a date c. 510' (FV 49). Their n. 5 shows that some would go later, as does the fullest recent publication of the pediment by E. Touloupa,¹ though no later than 490. So there is not that much in it and the question of construction, destruction and survival becomes of more moment than stylistic dating.

(ii) No inscription mentions a temple, and in the one so restored *ναο|υ* (quoted in FV 50 n. 11) is not the only solution suggested,² and, even if a *naos* were named, it gives no indication of its condition. All other instances in inscriptions cited (FV 50 n. 11) mention only a *hieron*, and as a location, not in a context of cult, although we might assume that the word implies cult. David Lewis has pointed out to me *IG* xii.9 191 lines 10 f., 43, which seem to imply that the *hieron* was spacious enough to accommodate the citizens of Eretria. For cult, of course, a temple is unnecessary: a *temenos* and altar are all required and often all available. Continuation of cult on the site is probable but there is no proof in inscriptions

¹ *Τά ἐναετία γλυπτὰ τοῦ ναοῦ τοῦ Ἀπόλλωνος Δαφνηφόρου στὴν Ἐρέτρια* (Ioannina 1983). And cf. Boardman in *The Eye of Greece, Studies . . .* Martin Robertson (Cambridge 1982) 9, where n. 29 should read 'later than 499', not '490'. FV cite (50 n. 10) Coulton's study of Doric capital proportions, placing the Eretria Temple with the Temple of Zeus at Olympia (and many others) in one group, without quoting his conclusion 'proportions must be used as evidence of date only with great caution', having reviewed evidence from Archaic to Hellenistic.

² Cf. A. Wilhelm, *ArchEph* 1892, 134.

for the survival of the temple in 'full working order'. The Athenian Acropolis, John Barron reminds me, attracted cult and dedications with no temple from the Persian Wars to the completion of the Parthenon.

(iii) FV (52) summon up a picture of the destruction of the temple in terms of the flames engulfing the thatched roofs of Sardis (where the whole matter started) and allude to the 'relative absence of damage or traces of fire on the surviving sculpture'. But a fire could leave a marble temple relatively unscathed though roofless, and need hardly do more than warm its exterior sculptures. (The temple of Hera at Phaleron, destroyed by the Persians, lacked its doors and roof: Paus. i 1.5.) A wrecked temple of Apollo could have stood at Eretria, even with its sculpture in position, after the Persian visit, and the very disparate weathering of surviving parts is irrelevant to the question of whether the temple stood complete (or mainly complete) or a dignified ruin for about 265 or 295 years. Some weathering, Jim Coulton points out to me, could be post-antique. Why too is there only one (back) pediment with sculpture if there was plenty of time to provide two? Perhaps the construction was interrupted,³ and this could only have been in 490, not 198.

(iv) The long survival of a partly ruined temple above ground might seem surprising. The Oath of Plataea, in which the Greeks pledged to leave un-restored those temples destroyed by the Persians (FV 53 f. refer), seems to have been real enough, however it may have been interpreted and observed, and the Athenians only revoked it once their League had driven Persians from all Greek soil. Eretria had suffered most and earliest from the Persians and probably had more cause to remember. In the circumstances, it is as well not to overlook the fact that Eretria is one of the few places in Greece for which there is clear later record that ruins ('foundations') were still shown of walls wrecked by the Persians—that 'Old Eretria' which has taxed historians, topographers and archaeologists for so long (Strabo 403, 448 C).⁴ Wherever we place these ruins the record considerably reinforces the plausibility of a long-ruined temple of Apollo at Eretria; it would have been the obvious memorial to Eretrian suffering and courage. Even in Athens and on the Acropolis the foundations of the Athena temple destroyed by the Persians were left visible, it seems, as they are today.

(v) It may seem churlish on top of this to complain that Livy's *priscae artis* perhaps does not refer to the *signa* (it does refer to the *tabulae*, painted panels, which are most unlikely to be Archaic); that we cannot know whether the *signa* included architectural sculpture. Or to surmise, if they did, why the Romans only took one piece—if they did: all we know of the Amazon in Rome is that she was dug up in 1888.

Further question about the date of the Eretria temple will be worth asking only when further excavation offers new evidence of a positive nature.

³ See Touloupa (n. 1) 91–2.

⁴ There have been various discussions, e.g. Boardman, *BSA* lii (1957) 22–4, P. G. Themelis, *ArchEph* 1969, 157–61, and M. R. Popham, *Lefkandi* i (London 1980) 323–4 with nn. 1, 4. I incline now to Themelis' view that Old and New Eretria are on the same main site. New Eretria was a village in Strabo's day, refounded on and beside the ruins of a city that had suffered a Persian and two Roman sacks. (I shall return to this problem elsewhere.)

2. The Siphnian Treasury

The second part of the article is devoted to the Siphnian Treasury at Delphi. Here the authors are tackling a problem that seems not to exist. Throughout, their argument is aimed at resolving what they see as a discrepancy in the dating of the Eretria Temple and the Treasury. They feel that the buildings should be near-contemporary, and in this they must be applying a chronology based on stylistic analysis, which is a method they seem ready to decry for chronological problems. Conventional chronology already has them 30 years apart, satisfying current views on stylistic development as well as other criteria. The FV date for Eretria widens the gap to 60 years. This should not unduly worry them, yet they wish them together and we must infer that the desire to downdate the Treasury so far is regarded as a worthy end in itself.

At least they cannot override Herodotus, and after five pages of discussion they agree that his date for the Treasury is c. 525. How Herodotus could have been so wrong about the date of a building which, they believe, was constructed during his lifetime and at a site he visited, is not explained. *En route* there is speculation, pointless in the circumstances, about whether Siphnos after the Persian Wars would have been able to finance the Treasury (about which there must be doubt), and finally, three further arguments are adduced for anchoring the Treasury after the Persian Wars.

(i) The first is Vitruvius' explanation of the term Karyatides as applied to statues of women serving as columns (a feature of the Treasury). Karyai (in Laconia) medised. After the Persian Wars it was punished and destroyed and the name of Karyai's women given to those weight-bearing statues to recall the city's ignominy for all time. The story is a good, if rather silly, *aition* for the woman-columns but can hardly be taken seriously (nor has it been, by others) whether or not Karyai was destroyed after the Persian Wars (the recorded destruction was in 369/8). There was a long tradition of woman-supports in Laconia, starting with the seventh-century *perirrhantaria* and continuing with fine early Archaic mirror-handles: the motif is an old orientalisising one. The women of Karyai danced for Artemis. The architectural member over some stone Karyatides might have recalled the women's head-dresses, suggesting the *aition*. This has been well explored by H. Drerup in *Marburger Winckelmannsprogramm* 1975/6, 11–14. However old the *aition* and whatever happened to Karyai, the history of the type makes clear that the story has nothing to offer by way of a *terminus post quem* for the Karyatides on the Treasury or elsewhere. The idea that the Karyatides of the Erechtheion, holding *phialai*, instruments of cult, might be images of oppressed medisers is absurd.

(ii) The second argument is elusive. There were other Archaic Karyatides at Delphi. These too require downdating and so the feasibility of the Cnidians being able to construct them after the Persian Wars has to be considered; and the Throne of Apollo at Amyklai, since there is danger of it too having sheltered Karyatides and it must therefore not be left in the sixth century. Recall here that the necessity for this downdating depends on disquiet at not having the Treasury and the Eretria Temple contemporaries. That the sculpture programme of the Treasury could be read as reflecting the Persian

Wars is undoubted. So could almost any series of stories, given a flexible approach to the subject, but all the stories had appeared in art long before, for other purposes, and they do not include some most closely associated with the Persian War symbolism elsewhere (Amazonomachy, Centauromachy).

(iii) The differing style of the two artists of the Treasury, one old-fashioned, one avant-garde, is observed, as it has often been. One dates, obviously, by the latest features, but no one has suggested that these could be anywhere near as late as FV wish and the point of this argument is obscure and can only make sense to those who have already decided that 'advanced Archaic' can be put post-Persian War. And, to add one scholar's tentative reading of Aristion as artist on the Treasury, to another scholar's stylistic comparison between Aristion's work in Attica and the Eretria Temple, and another's observation of stylistic affinities between the Treasury and the Temple (the last two scholars, Stewart and Lullies, making clear that their comparisons do not suggest contemporaneity) is neither a reassuring exercise⁵ nor, at any rate, any indication of a late date except for those who believe that there is already some positive evidence for a date in the 470s for all these monuments.

The last page summarises. FV admit that they cannot prove that the Treasury is of the 470s but have assembled 'the political, documentary and art-historical context in which such a view could be possible. If we are to reject that view or one like it and accept Herodotus' implication regarding the Treasury at face value then we question the confidence most scholars now place in dates derived from stylistic comparisons.' This is ingenious.⁶ Stylistic criteria, which they have selectively used themselves throughout the article, are only placed at risk if the Treasury is *c.* 525 and the Temple, with the other monuments they discuss, in the 470s. Since they have failed to prove the date for the Temple, while that of the other monuments will depend either on this downdating or on the assumption that the treasury is of the 470s too (a spiral rather than circular argument), it seems that security can lie only in ignoring the entire case.

A final paragraph introduces coinage. In recent years numismatists have, with good reason though not total unanimity, been downdating the inception of coinage in general and of the Attic owl series in particular. The cautious numismatist will realise that this downdating, within the generally agreed chronology of Greek art history and archaeology, is quite a different matter from the downdating of FV, which might have further implications for coinage that they would find less acceptable. At any rate, the argument sketched by FV deserves a closer look. They point to Ashmole's

⁵ Arguments based on a highly selective assembly of suggestions from different sources are bound to be insecure. Stewart has already retracted (*J. Paul Getty Mus. J.* x [1982] 95 n. 8) while, e.g., Frel attributes the Eretria sculpture to the artist of Attic *stelai* usually dated *c.* 500 (*ibid.* x [1982] 98–104).

⁶ In *Burl. Mag.* cxxiv (1982) 41–2, FV took a different line, claiming that '... confidence in the Siphnian Treasury as an absolute landmark in the development of Archaic Greek art is based on insufficient evidence and flawed reasoning'. Now, Herodotus is blamed, yet (*ibid.*) '... as long as the visual arts of ancient Greece are considered culturally autonomous and the evidence of Herodotus, for example, can be dismissed as having no archaeological validity, then we shall deprive ourselves of important information not only about "social conditions" but about Greek art itself. Quite so.

comparison of the Siphnian Treasury with an early owl, and to Kraay's dating for the bulk of the early owls to 483–480, to confirm a 470s date for the Treasury. I observe:

(i) In the article cited Ashmole nowhere mentions the Treasury. He places his owl centrally in a group of monuments (vase painting, sculpture in the round and in relief) which he dated (in 1936) from about 566 to about 550.

(ii) Kraay's date for the owl in question was still in the sixth century, but Price–Waggoner would have it 500–480.⁷ This is very probably correct, but it and its few kin stand out as desperately old-fashioned beside owls of the same group, and by these the period's standards must be judged. Its quaint, early appearance puzzles numismatists too.

(iii) Ashmole sounds a warning about comparisons of coins with other media, echoed by Price–Waggoner, and the method, where medium, technique and scale are so different, must by now be discredited, especially since it is used with equal vigour to thrust the dating of coinage earlier as well as later. Comparisons with gem engraving, where the technique and medium are similar, and the scale if anything smaller, are more rewarding.⁸

The conventional dating of styles and objects in the Archaic period depends on 'fixed points' of varying quality and some fluctuation of preferred dates is endemic in the discipline. Thus, the elastic downdating proposed by Tölle-Kastenbein in *AA* 1983, 573–84, moving nothing more than fifteen years, a half-generation, seems at first sight to present no serious art-historical or iconographical problems (which, of course, does not mean that it is correct). In broad terms, and allowing such adjustments, the old scheme is coherent and makes good historical and art-historical sense. To disturb it radically will require arguments mounted with more attention to relevance and consistency than those in the article under discussion. And any novel hypothesis which, whatever its superficial attraction, can only be sustained by a succession of other novel hypotheses, must be suspect.

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⁷ *Archaic Greek Silver Coinage: the 'Asyut' Hoard* (London 1975) 66–8, Group IVc. The impression of an owl on a tablet at Persepolis shows that the series starts earlier at least than 494: C. G. Starr, *NC* 1976, 219–22.

⁸ See Boardman in the Cincinnati symposium 'Archaic to Classical', publication forthcoming.

The Frogs in the Frogs

W. S. Gilbert's very model of a modern major-general knew among other things the croaking chorus of the *Frogs* of Aristophanes, and their refrain is perhaps the most widely-quoted line in Greek literature.¹ But the interpretation of the *Frogs*' scene gives rise to debate, and there is no agreement on even basic

¹ An early version of this article was read to the Classical Association of Canada in May 1980 and to the Philological Association of the Pacific Coast in November 1980. I should like to thank Rosemary Harriott for her helpful comments.