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JOHN WALSH

The Authenticity and the Dates of the Peace of Callias and the Congress Decree*

For over a century scholars interested in the history of the Pentecontaetia have been unable to agree on the question of the authenticity of the Peace of Callias. I re-open the case of the Peace with the modest hope that some of the following arguments may help to close the gap between those who accept the Peace's authenticity and those who reject it. It is my thesis that the Peace is authentic, but that it was made in the 460's shortly after the Greek victory at the Eurymedon, and not ca. 449 as those who support the Peace's authenticity hold. In the course of this study it is necessary to review at length the important, but generally overlooked, evidence from the Periclean ψήφισμα known as the congress decree (Plut. Per. 17). This study of the decree suggests that it too is authentic and that it, as is the case with the Peace, belongs to the aftermath of the Greek victory at the Eurymedon.

*The Date of the Peace of Callias*¹

Among those scholars who hold that the Peace of Callias is authentic, there is consensus that the Peace was made ca. 449, that is shortly after Cimon's campaign to Cyprus. W.B. DINSMOOR's response to the possibility that the Peace was made in the 460's, after

* This study is the result of a suggestion made by Prof. CHARLES FORNARA of Brown University. Most of the work on this subject was done in Göttingen. I would like to thank the Deutscher Akademischer Austauschdienst for a stipendium granted me (1976–78) and Profs. C. J. CLASSEN, A. HEUSS, and K. NICKAU for making my stay in Göttingen pleasant and rewarding. This article is a substantially revised version of my M. A. thesis submitted to the University of Texas in 1979. For helpful discussion I thank N. PLUMMER, my brother, JOSEPH, and especially the readers of my thesis Profs. E. D. FRANCIS, P. GREEN (Director), and M. GWYN MORGAN. Suggestions made by the editorial board of this journal, in particular by Drs. A. STYLOW and M. WÖRRLE, have improved this paper. What errors remain are, of course, my own.

¹ In the course of this paper standard works on fifth-century history are referred to by the following abbreviations: ATL = B. D. MERITT, H. T. WADE-GERY, and M. F. MCGREGOR, *The Athenian Tribute Lists* vol. iii, (Princeton 1950); AE = R. MEIGGS, *The Athenian Empire*, Oxford 1972; HCT = A. W. GOMME, *Historical Commentary on Thucydides* vol. i, Oxford 1945; GG = G. BUSOLT, *Griechische Geschichte* iii¹, Gotha 1897. When possible inscriptions are referred to by their number in R. MEIGGS and D. M. LEWIS, *A Selection of Greek Historical Inscriptions to the End of the Fifth Century B. C.*, Oxford 1969, hereafter abbreviated ML. Otherwise reference is made to *Inscriptiones Graecae* (IG). For bibliography, see MEIGGS' survey, AE 487 and 598, and that of C. SCHRADER, *La Paz de Calias*, Barcelona 1976, 205. For a more complete survey of earlier work, see GG 346 and n. 2 and L. HOLZAPFEL, *Berliner Studien für Class. Philol. u. Arch.* 7, 1888, 19.

the battle at the Eurymedon, is representative. DINSMOOR claimed that it was 'clearly' preposterous to believe that the Peace could belong in the 460's.² Yet, there remain a body of evidence and a number of historical considerations – whose implications have never been fully appreciated – which suggest that the Peace between the Greeks and the Great King Artaxerxes (known as the Peace of Callias) was made in the 460's.

The best place to begin a review of the testimonia in support of a date in the 460's is with the evidence from Herodotus. At 7,151 he records a visit made to Susa by Callias. Herodotus does not give the precise reason for which Callias made this journey in the 460's, but no reason for Callias' presence in Susa other than for the purpose of concluding Peace has been suggested.³ We are, of course, disappointed that Herodotus did not tell us more about Callias' mission, but the historian's reticence on this point provides no reason not to treat the coincidence seriously. An examination of the way Herodotus has passed on this information may help us to appreciate his brevity in this matter. Herodotus tells us very little of the events following the first stages of the Pentecontaetia and this only incidentally. A good example of the way Herodotus passes on this information is, in fact, his note on Callias' visit to Susa. At 7,150 Herodotus reports that Xerxes, before his invasion of Greece, sent an embassy to the Argives requesting their support. Herodotus then interrupts his narrative of the early stages of the Persian War to make note of an embassy from Argos to Susa at some time around Artaxerxes'

² *Hesperia* Suppl. 5, 1945, 156 and n. 324.

³ Herodotus makes it clear that the Argive mission was in Susa sometime soon after Artaxerxes' accession to the Persian throne. E. M. WALKER, *Camb. Anc. History* 5, 470, chose (citing no good reason) to set the embassy as late as 462/1, but it is unlikely that the Argives would have waited 3–4 years after Artaxerxes became king to ask him εἴ σοι ἔτι ἐμμένει τὴν πρὸς Ξέρξην φιλίην συνεκράσονται. HOLZAPFEL's arguments, *op. cit.*, 31, that both the embassies of the Argives and Callias were sent earlier are more convincing. Callias and the Argives were in Susa by 464 and perhaps even in 465. C. L. MURISON, *Phoenix* 25, 1971, 27, has recognized that Callias was in Susa to make Peace in the 460's. He believes, however, that Callias' mission 'clearly was rebuffed'. MURISON's conclusion is not at all clear. Demosthenes (19, 273) tells us clearly that Callias was fined fifty talents for his role in making the Peace. If Peace was not made, for what reason, then, was Callias fined? Nowhere in our sources do we get even the slightest hint that Peace was not made *because* Callias' mission failed. Even those who argue forgery must admit that, according to the 'invented' tradition, Callias' mission must have been successful. Our knowledge that Callias was in Susa in the 460's should be combined, not with a gratuitous conclusion, but with the other evidence which tells us clearly that the Peace Callias sought was made. J. H. SCHREINER, *SO* 52, 1977, 36, has suggested that the Peace was made 'after the Athenian failure in Egypt'. SCHREINER's thesis is not likely to win much acceptance. There is no evidence which suggests that the treaty we know as the Peace of Callias is identical with the truce which permitted the Athenian retreat from Egypt. It is also not credible that the terms of the Peace of Callias were agreed upon after an Athenian defeat. Surely the terms reflect an Athenian victory since they are so unfavorable to the Great King. Furthermore, SCHREINER has not presented an adequate analysis of the sources. At one point, p. 29, he writes: «No Athenian known to us, or non-Athenian, for that matter, shared Plutarch's eccentric chronology.» This is false. Lycurgus, in *Leocr.* 73, and *Menex.* 242a present the same chronology. Finally, SCHREINER seems not to have noticed that the chronology he calls eccentric is not only Plutarch's, but it is shared also by Callisthenes and Craterus in *Cim.* 13.

accession to the throne. Herodotus then digresses further with the remark that Callias was in Susa at the same time as the Argive mission, but there on another matter (ἐτέρου πρὸς ἡγεμονίας εἵνεκα). Argive relations with Persia shortly before Xerxes' invasion are Herodotus' primary concern in this part of his history. We have the note on Callias' mission in a digression on a digression and, in this light, Herodotus' brevity is expected. Another reason for Herodotus' not pursuing the matter of the Peace at greater length is the nature of the Peace itself. On any account the Peace was not a great success – it would have been broken shortly after it was made whether one dates it to the 460's or the 440's.⁴ It is possible that Herodotus intended to refer only indirectly to the Peace precisely because the great promise which the Peace held was not realized and, so, in his eyes, it was a Peace which deserved no more than a brief and oblique reference.⁵ Still, no matter how one views Herodotus' lack of interest in the Peace, the connection between the visit of Callias (the peacemaker) in the 460's and the Peace of Callias is so obvious that this reference in Herodotus, however brief, is sufficient for the modern reader of his (Histories), no less than his contemporary audience, to draw a subtle, yet clear, connection between this (other matter) and the Peace of Callias.

There is also evidence from the fourth century which is clear in its support of a Peace made shortly after the Greek victory at the Eurymedon. In his speech against Leocrates, Lycurgus at paragraph 72 mentions the Greek victory at the Eurymedon. To make the extent of this victory and resulting Peace clear Lycurgus notes an important difference between the Eurymedon campaign and the Cyprian expedition. As a result of the Cyprian expedition the Greeks were content (ἀγαπήσαντες) merely with the erection of a trophy at Salamis.⁶ On the other hand, after the victory at the Eurymedon, the allies implanted (πήξαντες) boundaries and not just a trophy. Lycurgus goes on to note that the chief result of the victory (τὸ κεφάλαιον τῆς νίκης) was that the Greeks συνθήκας ἐποίησαντο. There follows then Lycurgus' version of the conditions associated with the Peace of Callias. The clear testimony which this text offers has never been acknowledged. Most recently, its value has been doubted by C.L. MURISON who claims that: «It is not clear from this when Lycurgus thought the Peace was made: if anything, perhaps after the Eurymedon campaign, but again the matter is decidedly

⁴ S.K. EDDY, CPh 65, 1970, 14, has noted the possibility that the Peace, if made in the 440's, was violated soon after. For a more thorough development of this thesis, see HOLZAPFEL, op. cit., 19 ff.

⁵ HENRY R. IMMERWAHR, *Form and Thought in Herodotus*, Cleveland 1966, 9, in a discussion not concerned with the authenticity of the Peace, offers a convincing explanation of Herodotus' not relating Greek affairs with Persia down to the Peace of Callias: «such a continuation would conflict with the idea of balance by its stress on aggression in the formation of the Athenian empire ... such a sequel would also devalue the Greek victories of 480 and 479 B.C.» The aptness of IMMERWAHR's observation receives some confirmation by the fact that Herodotus does not mention the battle at the Eurymedon.

⁶ Lycurgus is surely referring to Salamis in Cyprus, not to Salamis in the Saronic gulf, since he is not likely to have compared the great victory of 479 unfavorably even with the Greek success at the Eurymedon.

vague.»⁷ Once again this evidence is not 'decidedly vague'. Lycurgus surely meant that the victory referred to in τὸ κεφάλαιον τῆς νίκης was the victory at the Eurymedon mentioned four lines above. The subsequent mention of agreements must refer to the terms of a Peace made as a result of this victory.

Lycurgus is not the only fourth century source which argues for an early dating of the Peace. The testimony of the 'Menexenus' (242a) which, by providing a terminus ante quem for the Peace, namely the battle of Tanagra, corroborates the early date for which I am arguing. As is the case with the evidence from Lycurgus' 'In Leocratem', the testimony of the 'Menexenus' is widely misunderstood and, as a result, rejected. The basic objection is that the author of the 'Menexenus' is guilty of the chronological impossibility of placing the Peace subsequent to 454 and, at the same time, before the First Peloponnesian War in 460.⁸ However, a close reading of the text and its composition show that the 'Menexenus' is not guilty of the chronological inconsistency with which it is charged. Simply because reference to the Peace or, for that matter, to the battle of Tanagra was made in this text after the Cyprian expedition is mentioned, we are not permitted to conclude that the author mistakenly thought that this Peace and the battle of Tanagra were events which followed the Cyprian expedition. It was not owing to an ignorance of correct chronology that the Peace is referred to in the section on Athenian relations with Sparta rather than in the preceding section on Athens' relations with Persia. The Peace is mentioned at this point in the narrative because our author wished to contrast, as closely as possible, the existence of a Peace with Persia (εἰρήνης δὲ γενομένης, 242a2) with the war which arose, shortly thereafter, among the Hellenes (γενομένου πολέμου, 242a6). The phrase μετὰ δὲ τοῦτο in 242a6 provides the reader with the intended chronological connection. Peace was made. After this war among the Hellenes began. The battle of Tanagra, then, is the first major engagement in the war which the author records. The Peace is explicitly and correctly dated by its relationship to the First Peloponnesian War and the battle of Tanagra.⁹

Plut. Cim. 13, provides us with further evidence which suggests that the Peace of Callias was made in the 460's:¹⁰ τοῦτο τὸ ἔργον οὕτως ἐταπείνωσε τὴν γνώμην τοῦ βασιλέως, ὥστε συνθέσθαι τὴν περιβόητον εἰρήνην ἐκείνην, ἵππου μὲν δρόμον αἰεὶ τῆς

⁷ MURISON, op. cit., p. 19. MEIGGS, AE 129, mentions the evidence from Lycurgus, but does not consider it or, for that matter, note that it does not support a Peace made ca. 449. The absence of any reference to this text on this point from R. SEALEY's influential article, *Historia* 3, 1954-55, 331, suggests that he did not consider it worth discussing.

⁸ For expressions of this argument, see G. BUSOLT, *Rh. M.* 38, 1883, 150; MURISON, op. cit., 14; and SEALEY, op. cit., 329.

⁹ Unfortunately the author of the 'Menexenus' does not tell us how long before the battle of Tanagra the Peace was made. Still, Menex. 242a does place the Peace before the beginning of jealousy and envy developed between Athens and Sparta. The period of ill-will described by the author of the 'Menexenus' is easily identified with the 'open split' of Thucydides' account (1, 102, 3) that is the Peace referred to by the author of the 'Menexenus' was made sometime before the Athenian dismissal from Ithome. This suggests a Peace after the Eurymedon.

¹⁰ The text has been taken from K. ZIEGLER's edition, *Plutarchi Vitae Parallelae* vol. 1 fasc.1, Leipzig 1960.

Ἑλληνικῆς ἀπέχειν θαλάσσης, ἔνδον δὲ Κυανέων καὶ Χελιδονίων μακροῦ νηὶ καὶ χαλκεμβόλῳ μὴ πλέειν. καίτοι Καλλισθένης οὐ φησι ταῦτα συνθέσθαι τὸν βάρβαρον, ἔργῳ δὲ ποιεῖν διὰ φόβον τῆς ἡττῆς ἐκείνης, καὶ μακρὰν οὕτως ἀποσπῆναι τῆς Ἑλλάδος, ὥστε πεντήκοντα ναυσὶ Περικλέα καὶ τριάκοντα μόναϊς Ἐφιάλτην ἐπέκεινα πλεῦσαι Χελιδονίων καὶ μηδὲν αὐτοῖς ναυτικὸν ἀπαντῆσαι παρὰ τῶν βαρβάρων. ἐν δὲ τοῖς ψηφίσμασιν ἃ συνήγαγε Κρατερός ἀντίγραφα συνθηκῶν ὡς γενομένων κατατέτακται. φασὶ δὲ καὶ βωμὸν Εἰρήνης διὰ ταῦτα τοὺς Ἀθηναίους ἰδρῦσασθαι, καὶ Καλλίαν τὸν προεβύσαντα τιμῆσαι διαφερόντως.

There can be no doubt that the Peace which Callisthenes found somehow objectionable was presented to him as having been made shortly after the Eurymedon. This is insured, first, by the fact that Plutarch gives no indication that it was any Peace other than the one just mentioned, namely a Peace shortly after the Eurymedon, which Callisthenes questioned and, secondly, by Callisthenes' observation that as a result of the Peace a squadron of thirty ships under Ephialtes' command sailed beyond the Chelidonian Islands without meeting Persian resistance. The date of Ephialtes' death (462/61) confirms the early date of the Peace under discussion. I realize that Callisthenes' report has been taken as evidence against the Peace's authenticity. These arguments will be examined in detail in the section of this paper which deals with the problem of authenticity. For the present, however, I wish to emphasize only that the tradition which Callisthenes knew placed the Peace clearly in the 460's and not in the 440's. In addition to the testimony of Callisthenes, Plutarch has transmitted evidence from Craterus' collection of inscriptions, including a copy of the agreements, which was considered by Plutarch a proof that they existed (συνθηκῶν ὡς γενομένων). Plutarch's word on this matter cannot be dismissed lightly. His interest in the famous Peace caused him to research it. This is reflected by the fact that he consulted more than one source and included a number of supplementary details, for example the notes on the Altar of Peace and Callias' fame. In fact, when Plutarch was interested in pursuing a subject he was capable of rather thorough research. The *᾿Λίβες* are full of examples where he checked a number of sources in an effort to reach the truth. I need cite only one such example. At Arist. 26 Plutarch has consulted at least three sources on Aristides' death.¹¹ It is worth noting that in this passage Plutarch is rather critical of Craterus' account because he did not include the usual supplementary material. In Cim. 13, however, there is no such complaint made about Craterus and this may be because

¹¹ The simplistic argument that Plutarch did not, as a rule, consult first-hand the sources he cites, but instead copied the material found in his *᾿Λίβες* from something resembling an ancient edition of the *᾿Real-Encyclopädie* is fortunately no longer available to those who chose, on the basis of no evidence, not to believe what Plutarch has reported. This method of discrediting evidence, popular among the *᾿Quellenforscher* of the last century and still lingering in this century, has been effectively countered by C. THEANDER's careful study, *Plutarch und die Geschichte*, Bull. Soc. des Lettres de Lund, 1950–1951, 1–86. THEANDER has shown that where Plutarch mentions a source for one of his reports we can assume that he read this information in the source he mentions and that he did not copy it, blindly and uncritically, from some ancient handbook.

Craterus' account of the Peace included sufficient testimonia and was convincing. The most important result of Plutarch's research is the fact that neither he nor Callisthenes nor Craterus gives so much as the slightest hint that another date for the Peace was known. In sum, the body of evidence from Lycurgus, the Menexenus, and Plutarch is clear and consistent. It must be respected.¹²

On the other hand, in support of a Peace at the beginning of the 440's we must rely on Diodorus (12, 4, 4). The first difficulty in following Diodorus on this point is that, in general, his account of the Pentecontaetia is notoriously confused. In particular Diodorus' account of the Cyprian expedition is, as A. W. GOMME put it, «nothing but a confused repetition of his own account of the Eurymedon».¹³ Diodorus' indiscriminate blending of the details of the Eurymedon campaign with those of the Cyprian expedition is certainly the most well-known instance in which he has conflated the details of two expeditions, but it is not the only example of confusion between two separate expeditions which were similar in purpose, but different in time. At 12, 10–11 Diodorus has again entangled two separate expeditions, on this occasion, two Greek expeditions to Thurii.¹⁴ The problem with using Diodorus' universalizing history as a source for the Pentecontaetia does not end with the author's failure to distinguish among the details of two separate events. In a number of other instances R. MEIGGS has correctly cast doubt on Diodorus' chronology on various grounds. Yet, when he considered Diodorus' date for the Peace, he was, by and large, satisfied with it.¹⁵ MEIGGS' trust on this point is all the more surprising because one of the lessons to be learned from his perceptive survey is that Diodorus' chronology should be followed without question only when there is independent evidence which corroborates it; for example, Diodorus can be said to give us the correct date for the Thirty Years' Peace because he agrees with the evidence from Thucydides (1, 115, 1). There is, however, no good evidential parallel for Diodorus' date for the Peace.

Diodorus' trustworthiness on this point cannot be bolstered by an appeal to the authority of Ephorus. Even if we grant, for the sake of argument, that Ephorus was

¹² There are other sources which, in varying degrees, suggest that the Peace was made after the Eurymedon, e.g. Amm. Marc. 17, 11, 3 ed. V. GARDTHAUSEN, Leipzig 1874; Himer. or. 6, 323 (Himerii Declamationes et Orationes ed. ARISTIDE COLONNA, Rome 1951; Euseb. Chron. vol. ii p. 104 ed. A. SCHOENE, Weidmann 1866; Ael. Arist., Panath. 153 ed. W. DINDORF, Leipzig 1829; Suda s.v. Kimon. I do not consider these sources separately because they do not contain any information not found in the other sources, but it is significant that the tradition which dated the Peace to the 460's is the one which these writers followed.

¹³ For details of the confusion, see BUSOLT, GG 343; E. MEYER, Forschungen zur alten Geschichte 2, Halle 1899, 1 ff.; GOMME, HCT 286 and 330; W. PEEK, HSPH Suppl. 1, 1940, 97; H. T. WADE-GERY, JHS 53, 1933, 71. Aristodemus also records a Peace in the early 440's (FGH Hist 104 F 13, 2). This evidence for a Peace in the 440's does not substantially influence the debate.

¹⁴ This confusion has been discussed by WADE-GERY, JHS 52, 1932, 217 and n. 49. The subject of the expeditions to Thurii has been dealt with recently by N. K. RUTTER, Historia 22, 1973, 155 and A. ANDREWES, JHS 98, 1978, 5.

¹⁵ MEIGGS, AE 452.

Diodorus' source for the date of the Peace, it does not necessarily follow that Ephorus recorded the correct date for the Peace or that, even if Ephorus did date the Peace correctly, Diodorus transmitted this date accurately. In view of Diodorus' difficulty in distinguishing the events surrounding the Eurymedon campaign of the 460's and the Cyprian expedition of the 440's there is reason to believe that he incorrectly assigned to the Cyprian expedition a Peace which belongs to the aftermath of the victory at the Eurymedon. Rather than begin by assuming that a reliable source and reliable transmission underlie Diodorus' account, we should consider the possibility that no ancient tradition recorded a Peace in the early 440's, but that Diodorus has simply made another mistake. In sum, then, the conclusion that Diodorus is guilty of misplacing the Peace is preferable to rejecting the testimony which we have seen points to the conclusion that the Peace was made in the 460's.

Since the total rejection of the possibility that the Peace was made in the 460's cannot reasonably be based on an evaluation of the evidence, one may legitimately ask why this possibility has been ignored for almost a century. It seems that objections of a general historical nature have been thought to be so compelling that any further debate on the matter has been precluded. The fullest expression of these objections has been made by F. KOEPP.¹⁶ His arguments against a Peace in the 460's were subsequently accepted by BUSOLT. BUSOLT's authoritative statement on the matter put an end to any further debate.¹⁷ It is surprising that BUSOLT adopted KOEPP's position so uncritically because the arguments on which this position is based are weak. They are not based on evidence, but only on general historical considerations which supposedly lead to the conclusion that the political climate within the Greek alliance and the military situation between the Greeks and Persians preclude peace in the 460's. The following review will show that these considerations do not force us to reject the solid evidence in favor of a Peace in the 460's.

KOEPP first assumed that the Peace could not have been made in the 460's because the subsequent Egyptian and Cyprian campaigns would have constituted a violation of the Peace. There is an error of logic in his argument. Peace after the Eurymedon and two subsequent Greek campaigns are not, as KOEPP believed, mutually exclusive. It can be objected to KOEPP's line of reasoning that the obvious happened – the Peace was broken.¹⁸ And there exists evidence that the Peace could not last; that is, that it was a Peace of convenience which satisfied neither party. Demosthenes (19, 273) reports that Callias was nearly sentenced to death on a charge of bribery in connection with his role in making Peace.¹⁹ Instead he was fined 50 talents. Some in Athens (among them

¹⁶ Rh. M. 48, 1893, 485.

¹⁷ BUSOLT, GG 345 ff.

¹⁸ See my note 4.

¹⁹ This information has been rejected by H. T. WADE-GERY, *Essays in Greek History*, Oxford 1958, 229 and n. 3, and J. K. DAVIES, *Athenian Propertied Families 600–300 B.C.*, Oxford 1971, 261, on the grounds that it conflicts with the evidence of Plut. Cim. 13 (which states that Callias was honored greatly for his role in making the Peace) and Pausan. 1, 8, 2 (who reports that a statue

Callias' prosecutors – was Pericles one of them?) doubtless thought that the Greeks should have exacted a higher price for Peace or, perhaps, that they should have followed their great victory at the Eurymedon with further military action. Whatever the precise nature of the objection, one thing remains clear: in Athens some were not satisfied with the Peace. Too little attention has been paid to the situation at the Persian court at the time I am suggesting the Peace was made. Sometime in 465 Xerxes was murdered. Not surprisingly, his successor, Artaxerxes, was at the beginning of his reign beset with considerable difficulties. He was greeted with intrigue and the revolt of a major province (Bactria).²⁰ At the time of this unrest the new King needed no further trouble from a victorious Greek force. Furthermore, a respite from continued Greek interference would permit him to concentrate on putting his own house in order and on rebuilding the military forces destroyed at the Eurymedon. To buy this time, Artaxerxes made Peace by ceding territory which had long been the royal family's. Such a concession must have been particularly humiliating for the new Great King and he probably did not regard it as permanent. So, there is good reason to believe that the Peace, as a long term solution, was unsatisfactory to both sides and could easily have been broken. KOEPP did not seriously consider the evidence from Demosthenes or the conditions of Artaxerxes' accession. Instead, he based his claim on the general hypothesis that violation of a Peace treaty somehow implies that the Peace violated was never made. The evidence, however, suggests that this Peace was particularly vulnerable.

KOEPP's second objection reflects what he believed to be the situation within the Hellenic League in the 460's.²¹ He claimed that Athens could not possibly have made peace with the Great King because such a Peace would have put an end to the alliance which formed the basis of Athenian strength. It is true that the Athenians occupied a special position among League members, but the Greek victory at the Eurymedon was a League operation and, although we do not know with certainty by what procedure the League reached its decisions, there is no reason not to follow GOMME's conclusion that

was erected to the Callias who made the Peace with Artaxerxes). Scepticism about the trustworthiness of Demosthenes' statement is unnecessary because the evidence from Plutarch and Pausanias in no way forces us to reject it. None of this later panegyric precludes the possibility that Callias was, as Demosthenes suggests, held in low repute at the time the Peace was made. Every effort should be made to accommodate all this evidence which fits together most reasonably if we hold that Callias was, at first, blamed for the Peace and later glorified because of it. The Athenian Demos is well known for its propensity, at one time, to condemn one of its great men and, at another time, to praise this same man. Compare, for instance, the case of Socrates. I believe that Demosthenes has given us an important piece of evidence on the reception of the Peace at Athens and it should not be dismissed arbitrarily as fiction.

²⁰ On the date of Xerxes' death and Artaxerxes' accession, see HOLZAPFEL, *op. cit.*, 31. The revolt in Bactria was not the only difficulty faced by the new Great King. There was court intrigue and trouble in Egypt. For details, see A. T. OLMSTEAD, *History of the Persian Empire*, Chicago 1948, 289. It should be noted that Artaxerxes' situation was particularly delicate because he was only 18 years of age at the time of his accession.

²¹ KOEPP, *op. cit.*, 487.

there were «equal votes for each member».²² So, even if Athens as *primus inter pares* had opposed the Peace for the self-serving reason KOEPP suggested, it may not have been within her power to prevent this Peace. In other words, KOEPP's assumption that, in this matter, Athens' will must have been law, is gratuitous. Nor is it all certain, as KOEPP believes, that it would have been to Athenian advantage to oppose the Peace. Even with peace Athens could argue that the continuance of the League's fleet was essential to the security of its members. There were two good reasons for the League's continued existence. First, the fact of peace with Persia does not permit the conclusion that, correspondingly, a complete renunciation of the League's defensive responsibilities would be welcome to League members. The continued existence and effectiveness of the League, in particular the fleet, would have been very important for those who, as a result of the Peace, were guaranteed their freedom and, at the same time, saw no safeguard in the Great King's good-will. An active League provided another common benefit. It does not need to be demonstrated that the Greek cities found freedom from piratical activity to their advantage. Now that hostilities against Persia were at an end, the most natural instrument for the insurance of this security was the League's fleet. Although, as a result of the Peace there may have been a reduction in League activities and expenditures, we have no reason to assume, as KOEPP did, that the demise of the League (and with it Athens' base of support) was a foregone conclusion once peace was made. KOEPP has not based his case on explicit evidence which tells us that there was no peace made after the Eurymedon. He did not even provide credible reasons for disbelieving the evidence we have which supports a Peace after the Eurymedon. His case rests entirely on an interpretation of the 460's which led him to believe that peace at that time was impossible. I have not brought forth these responses to KOEPP's position as proof of a Peace in the 460's. I have merely attempted to show that KOEPP's reconstruction of the Greek world after the Eurymedon does not, in any way, prove that peace was, at that time, impossible.

Before I proceed to a consideration of the hypothesis that the Peace of Callias is a fourth-century forgery, I would like to note some historical considerations which support the evidence I have adduced in favor of a Peace in the 460's. It has been recognized that the Cyprian expedition was not a Greek success. They abandoned the expedition with their goals largely unaccomplished.²³ On the other hand, the Great King successfully defended his position. There is nothing in the outcome of this expedition which would have induced him to make a Peace so favorable to the Greeks and so unfavorable to himself. Furthermore, by the early 440's the Great King's affairs in Persia were running smoothly and there was not the domestic disorder which faced Artaxerxes on his accession in 465 and was, I believe, a major factor in his willingness

²² GOMME's conclusion, HCT 373, has recently been strengthened by PHYLLIS CULHAM, *AJAH* 3, 1978, 27, who on the basis of an examination of Thucyd. 1, 97, 1 and 3, 11, 1–3 has shown that the evidence on this point suggests that the League was unicameral with each member having equal voting power.

²³ This has been emphasized most recently by S. THOMAS PARKER, *AJPh* 97, 1976, 30.

to come to terms with the Greeks in the 460's. In other words, the terms of the Peace, if agreed upon after the Cyprian expedition, lack plausibility. The Eurymedon campaign, however, was a major defeat for Persia.²⁴ After this Greek victory the Peace's terms (so favorable to the Greeks) are consistent with the outcome of the hostilities and the new Great King's weak position at home.²⁵

Secondly, although the Peace with Persia has become known as the Peace of Callias, the sources suggest that another man was responsible for the military victory which made the Peace possible. In Plut. Cim. 13 we read that it was Cimon who «humbled the King's pride». Entries in the «Suda», s.vv. Καλλίας and Κίμων suggest that the boundaries imposed on the Persians were the direct result of Cimon's military victories. And it was for this reason that the Peace of Callias was, at one time, known as «Cimon's Peace». ²⁶ If the Peace were made in the early 440's as a result of Cimon's participation in the Cyprian expedition, a problem with this Peace surfaces when we turn to Thucydides' account. According to Thucydides (1, 112, 4), Cimon died at Citium *before* the two battles at Salamis. Cimon, then, did not lead the Greeks in the battles which led to the Peace. Some of those who hold that the Peace was made in the 440's have been aware of this difficulty. Diodorus apparently chose to retain Cimon's association with these battles. To do so he must have rejected Thucydides' chronology because he placed the two battles at Salamis before the battle at Citium and Cimon's subsequent death (this is, in itself, a good illustration of the limits of Diodorus' accuracy in respect to the chronology of the Pentecontaetia). GOMME approached this evidence in a different way. ²⁷ He accepted correctly Thucydides' chronology and claimed that the inconsistency was «due to a later glorification of Cimon's deeds». But we should be careful not to equate glorification with fabrication. The possibility that there might have been some glorification of Cimon's deeds does not permit the conclusion that there is absolutely no basis in fact for the association of a living Cimon with the Greek victories which forced the Peace. However, this difficulty exists only for a Peace set in the early 440's. With a Peace after the Eurymedon in the 460's there is no problem in reconciling the sources. We are not obliged either to reject (with Diodorus) Thucydides' chronology or to reject (with GOMME) the reasonable association of Cimon with the victories which led to the Peace.

²⁴ For the pride which the Greeks felt from their great victory at the Eurymedon, see K. SCHAUENBURG, *MDAI(A)* 90, 1975, 97 and E.D. FRANCIS, *Greeks and Persians: The Art of Hazard and Triumph*, in: D. SCHMAND-BESSERAT (ed), *Ancient Persia*, Los Angeles 1979, 71–72. It is hard to see how this exuberance could be paralleled by the results of the Cyprian expedition.

²⁵ There is no reason to delay over the differences in the varying accounts of the land and sea limits established by the Peace (see MEIGGS, *AE* 487, and C. SCHRADER, *op. cit.*, 106, for the details of the debate). My point is simply that any account of the boundaries restricts the movement of the Persian in territory which was previously open to him. Similarly, the effect of the autonomy clause (Lycurg. in *Leocr.* 73 and *Diod.* 12, 4, 5 and 12, 26, 2) in the Peace was that the King formally recognized that he had lost Ionia.

²⁶ For examples, see HOLZAPFEL, *op. cit.*, 20 and n. 5.

²⁷ HCT, 330.

Finally, there is another consideration which, although it is certainly not proof of a Peace in the 460's, nevertheless does raise an interesting doubt about the possibility of a Peace in the 440's. By the early 440's Pericles was prominent and it is remarkable that he is nowhere in the ancient testimonia associated with the Peace. Had the Peace been made in the 440's, it would not be unreasonable to expect that some source would have connected the Peace with the first man of the state. One misses a reference to Pericles particularly in Plutarch's life of Pericles. Had there been any account of a Peace made in the early 440's available to Plutarch, it is surprising that he makes no mention of this well-known event to illustrate this or that quality of his hero. Yet, Plutarch is silent. It is not in the 'Pericles' (a vita which deals in detail with the events of the 440's) where we find a direct reference to the Peace, but in the 'Cimon' (a vita which deals in detail with the events of the 460's) where we read clearly that the Peace was made as a result of the victory at the Eurymedon.

The Hypothesis that the Peace is not Authentic

The question of the authenticity of the Peace is one of the most vexing and complicated problems in fifth-century history. I agree with C. L. MURISON's judgement that perhaps the only way to solve the problem satisfactorily would be the finding of secure fifth-century inscriptional evidence for the Peace.²⁸ I have not found such evidence, but, since the arguments made against the Peace have been directed specifically against a Peace thought to have been made in the 440's and since these arguments do not necessarily affect the authenticity of a Peace made in the 460's, the present re-assessment of the evidence is warranted.

In the preceding discussion of the Peace's date, all the evidence brought forth (including Diodorus) supports the authenticity of the Peace. This evidence has been explicated and there is no point in reviewing it. There is, as is well known, other evidence, mostly from the fourth-century orators, which, while it makes no statement on the date of the Peace, also supports authenticity.²⁹ That these texts give clear testimony to the Peace's authenticity has been recognized by those scholars who are sceptical on this matter. What causes them to dismiss this evidence is their puzzlement over the lack of any reference to the Peace before its appearance in the orators of the fourth century. Surely, these scholars maintain, a Peace as important as the Peace of Callias must have been at least referred to by someone somewhere sometime before the fourth century. In response to this argument I would like to repeat the conclusion of my examination of Herodotus 7, 151. It is difficult to see for what purpose Callias was in Susa in the 460's if it was not to make the Peace which has come to bear his name. This connection is not new.³⁰ Scholars have been aware of this possibility, but have been unwilling to pursue

²⁸ MURISON in the Introduction to SCHRADER's book, cf. my note 1.

²⁹ Isocr. 4, 118 and 120; 6, 80; 12, 59; Dem. 19, 273.

³⁰ See my note 3.

it because of their reluctance to consider an alternative to the orthodox date. Our fifth-century sources may not be as silent as has been thought. The objection that Thucydides does not mention the Peace is an *argumentum ex silentio* whose weakness has long been appreciated.³¹ It was not Thucydides' purpose in his excursus on the *Pentecontaetia* to outline – and it must be remembered that his account of these years is, for whatever reason, just an outline – Athens' relations with Persia, but rather Athens' relations with other Greek states. The Peace should not be expected to have a place in Thucydides' brief account of the *Pentecontaetia*. The modern historian may be disappointed that Thucydides made no mention of this Peace, but his frustration provides no evidence that the Peace is fictitious.

As an alternative to the conclusion that the evidence from the fourth century has no value and, so, that the Peace is not authentic, I would like to offer an explanation of the reason why silence on the topic of the Peace is not broken until the fourth century and why in the fourth century it is broken so dramatically. I have suggested that the Peace was, whether it was made in the 440's or the 460's, short-lived. Perhaps this Peace, viewed by scholars as so very integral to our reconstruction of the *Pentecontaetia*, interested those contemporary with the Peace and those writing shortly after very little.³² It is often the case that what seemed important to those who experienced particular events is considered unimportant by following generations of historiographers. Similarly, often what is considered unimportant to those close in time to a particular event becomes, with the added perspective provided by the passage of time, important. This is, I believe, what has happened in the case of the Peace of Callias. How then can the sudden interest in the Peace by fourth century orators be explained? For those who wrote deliberative oratory in the fourth century, the Peace of Callias was not important as an item of history. They were interested in the use to which it could be put as a rhetorical *topos* in an effort to convince or to add emphasis. A consideration of the use of the *topos* of the Peace in the orators will make this clear. Isocrates (*Panegy.* 120) mentions the Peace in a context in which he denounces the Peace of Antalcidas. It is not at all surprising that one of the ways he argues against the Peace of Antalcidas is to compare it unfavorably with the better, more glorious fifth-century Peace between the Greeks and the Persians. Similarly, Demosthenes' point (*de falsa legat.* 273) in referring to the Peace of Callias was not to pass on historical information to future generations, but to persuade a jury. He notes Callias' being fined fifty talents in connection with making the Peace in order to point to a precedent for punishing those guilty of abusing ambassadorial trust. His reason for doing this is to secure punishment for those he feels are guilty of the same offense. In passing, he takes the opportunity to compare the glorious fifth-century Peace with the Peace initiated by Philocrates which he felt left

³¹ GOMME, HCT 370. MEIGGS, AE 141, considers Thucydides' silence a more serious objection, but concludes that its importance should not be overestimated.

³² Of course we cannot say that reference to the Peace was first made in fourth-century oratory. It is certainly possible that historians lost to us, such as Hellanicus or the *Atthidographers*, mentioned the Peace.

Athens defenseless and the ambassadors responsible for the Peace rich. My aim in explicating these texts has been to show that we do not begin to have frequent references to the Peace only in the fourth century because the Peace was 'invented' sometime shortly before these references start being made, but rather because in the fourth-century treaties of Peace were made frequently. The Peace of Callias, then, began to be considered important not for what it was in the fifth century, but for the way it could be used as a rhetorical foil in the law courts of the fourth century. Whatever rhetorical conceit might be read (rightly or wrongly) into these references to the Peace should not blind us to the fact that they were made in court and it is difficult to see why these orators would chance a reference to a Peace with a dubious pedigree which would weaken the case the speaker wanted to make by straining the credulity of the very men he wanted to persuade. In other words, the rhetorical evaluation of the Peace of Callias as a marvelous act of Greek diplomacy resulting from a major victory of Greek arms at the Eurymedon is certainly exaggerated. Some of the details associated with the Peace, such as the actual boundaries sworn to, may have been amplified, but the central fact of the Peace's authenticity was not invented for use as propaganda in the courts of fourth-century Athens.³³

Similarly, the assumptions which underlie the methodology followed by most scholars who have questioned the authenticity of the Peace are not free from question. The following statement of purpose by R. SEALEY is representative of this school of thought: «The peace of Callias is a question of source criticism; the question is, what hypothesis best accounts for the statements of fourth-century writers and does this hypothesis imply that the Peace was genuine or not.»³⁴ This is, of course, one way of approaching the evidence. There is another way which stands in fundamental disagreement with the methodology urged by SEALEY. This is to present the testimony of the sources as they stand and then to reconcile, as best as possible, whatever differences there are among the ancient testimonia before we have recourse to *Quellenforschung* which leads the investigator through a maze of assumptions and indemonstrable conclusions about the relative chronology and sometimes even the very existence of fourth-century publications.³⁵ The sole purpose of such explanations, as SEALEY himself admits, is to set up a framework which provides a hypothetical basis for accepting or rejecting the authenticity of the Peace. This methodology is, to be sure, appropriate for the solution of some

³³ Discrepancies in the details of the boundaries set up by the Peace are often pointed to as a reason for rejecting its authenticity. Such differences in detail are not serious objections.

³⁴ SEALEY, *op. cit.*, 328. In this point he may have been influenced by a similar statement of purpose by E. SCHWARTZ, *Hermes* 35, 1900: «Der Vertrag mit Persien oder der sogenannte Kalliasfrieden ist kein Problem der politischen, sondern der litterarischen Geschichte (p. 111) ... Zunächst muss der Nachweis versucht werden, wie der in der zweiten Hälfte des 4. Jahrhunderts in Athen offenbar verbreitete Irrthum entstanden ist.» (p. 112).

³⁵ WADE-GERY's argument, *Essays in Greek History*, Oxford 1958, 203, to the effect that Callisthenes was misled into dating to the 460's a Peace in fact belonging to the 440's is an example of such analysis. These techniques do not always make matters clearer as is the case with SCHRADER's «*Graphicos*», *op. cit.*, 193.

historical problems. For example, judicious use of this approach has helped us make progress in untangling the splicing of divergent traditions which have caused the confusion between the Eurymedon campaign and the Cyprian expeditions. However, in the case of the evidence on the authenticity of the Peace, use of this complicated methodology is unnecessary because, with the exception of the supposed dissent of Callisthenes and Theopompus which will be considered presently, there are no substantial contradictions or irregularities which render the basic question of the Peace's authenticity suspect. There are, of course, difficulties in determining the exact boundaries set by the Peace, but this problem should not be permitted to obfuscate the fact that the sources which unequivocally attest to the authenticity are really quite numerous and uniform.³⁶ In fact, in respect to the question of authenticity, we have before us a body of evidence which, judged by comparison with the meagre testimonia on other problems in fifth-century history, such as the Oath of Plataea or the Themistocles Decree, is plentiful and surprisingly consistent. I am suggesting only that before we fall back upon a brand of source criticism which, in some cases, is guilty of putting the cart before the horse, we first try to solve the problem by more direct methods of analysis.

In order to complete this review of the evidence on the Peace's authenticity, three important texts which have been thought to provide evidence that the Peace is a fabrication must be examined. The fragment from Theopompus (preserved by Theon in his 'Progymnasmata') deserves careful consideration:³⁷

... παρὰ δὲ Θεοπόμπου ἐκ τῆς πέμπτης καὶ εἰκοστῆς τῶν
 Φιλίππικων, ὅτι «Ὁ Ἑλληνικὸς ὄρκος καταψεύδεται, ὃν
 Ἀθηναῖοι φασιν ὁμόσαι τοὺς Ἑλλήνας πρὸς τῆς μάχης τῆς ἐν
 Πλαταιαῖς πρὸς τοὺς βαρβάρους, καὶ αἱ πρὸς βασιλέα Δαρεῖον
 5 Ἀθηναίων καὶ Ἑλλήνων συνθῆκαι. ἔτι δὲ καὶ τὴν ἐν Μαραθῶνι
 μάχην οὐχ οἷαν ἅπαντες ὑμνοῦσι γεγεννημένην, καὶ ὅσα ἄλλα,
 φησὶν, ἢ Ἀθηναίων πόλιν ἀλαζονεύεται καὶ παρακραοῦται
 τοὺς Ἑλλήνας.

- 2 «Ὁ» K. W. KRÜGER, *Historisch-Philologische Studien*, Berlin 1836, I, 118.
 κατέψευσται E. SCHWARTZ, *Hermes* 35, 1900, 108, n. 4.
 ὃν «Ὁ» Ἀθηναῖοι SCHWARTZ
 4 Δαρεῖον seclisit SPENGEL
 5 πρὸς Ἑλλήνας συνθῆκαι MSS.
 «καὶ» Ἑλλήνων scripsi

³⁶ This kind of source analysis, pioneered by E. MEYER, op. cit. in my note 13, is, to be sure, an appropriate means of solving some historical problems, for example the sorting out of the details of two similar events such as the Eurymedon campaign and the Cyprian expedition. The success of source analysis in solving such problems, however, should not lead one to conclude that this is the best methodology for the solution of any historical problem. This methodology seems to me to be particularly ill-suited to the solution of problems such as questions of authenticity.

³⁷ The text and apparatus printed were published by W. R. CONNOR, *Theopompus and Fifth-Century Athens*, Washington 1968, 78.

- καὶ τῶν Ἑλλήνων WICHERS, *Theopompi Fragmenta* 218.
 πρὸς Ἑλληνας secluit SPENGEL
 περὶ Ἑλλήνων, JACOBY
 καλλίονες ἢ βασιλέως πρὸς Ἑλληνας SCHWARTZ
 6 οἷαν ἅπαντες, SCHWARTZ
 οὐχ ἅμα πάντες Estensis 116
 οὐχι ἅμα πάντες Paris 2918, Mediceus Laurentianus Plut. 55.10.

It has been recently emphasized by CONNOR that SPENGEL's deletion of πρὸς βασιλέα Δαγεῖον from the text is completely arbitrary.³⁸ As the text stands, Theopompus is objecting to a Peace made with Artaxerxes' successor, Darius, rather than to a Peace with Artaxerxes himself, a chronological impossibility if Theopompus were, in fact, objecting to the Peace which has become known as the Peace of Callias. And, even if Theon's text were corrupt and the Peace under Theopompus' consideration was the Peace of Callias, as is most likely the case, we would still not have proof that Theopompus was objecting to the Peace's authenticity since Theopompus' criticism was not directed against the authenticity of the Peace, but against the Athenians' attitude towards the Peace. Theopompus' complaint is that the Athenians are in the habit of magnifying the accomplishments of their past history. The connective ἐτι δὲ καὶ suggests that Theopompus' criticism of their victory at Marathon is on the same level as his criticism of the Oath of Plataea and the Peace; that is, the Athenians did not invent the Peace any more than they invented the battle of Marathon. The verb καταψεύδεται provides the key to interpreting this passage correctly. CONNOR's cautious understanding of this word deserves to be repeated: «Particularly troublesome is the word καταψεύδεται with which Theon expresses Theopompus' criticism of the Oath before Plataea and the Peace with Persia. The construction is not easily paralleled, but it is clear that while the word indicates some sort of attack on these «events», it does not justify the widespread notion that Theopompus made an outright denial of the oath and the Peace.»³⁹ CONNOR's assessment is, I believe, right on the mark. Καταψεύδεται does mean «falsify», but «falsify» in the sense of «misrepresent», not «falsify» in the sense «forge» or «invent». And in this the Athenian-baiter Theopompus was certainly correct. As I have argued earlier, the short-lived Peace after the great victory at the Eurymedon was by no means the magnificent diplomatic success which the later orators claimed. Theopompus recognized this and was not about to let these orators go unchecked on this point. This evidence, rather than being an argument against the authenticity of the Peace, actually supports my thesis that the Peace was, as viewed by near contemporaries, of very minor significance in the history of fifth-century Athens.⁴⁰

³⁸ CONNOR, loc. cit.

³⁹ CONNOR, op. cit., p. 85.

⁴⁰ The well-known fragment preserved by Harpocration s.v. Ἀττικοῖς γράμμασιν (F.Gr.Hist. 115 F 154) has often been adduced as further proof that Theopompus objected to the authenticity of the Peace. He claimed that the Peace was misrepresented (ἐσκευωρήσθαι) because the inscription he saw was cut in Ionic and not Attic letters. Since, for official use in Athens, the Ionic alphabet replaced the Attic in 403/2, Theopompus and those scholars who follow him have concluded that this stone was inscribed after 403/2 and, so, not an inscription contemporary with the Peace and

Callisthenes' testimony on the Peace (transmitted by Plut. Cim. 13 – this text is printed in the first section of this paper) is difficult to interpret and has led some, not without reason, to believe that Callisthenes claimed that the boundaries listed were de facto restraints put on the Persians as a result of their shattering defeat at the Eurymedon and not boundaries set up as a result of a formal Peace. However, a careful reading of this text suggests that this may not have been the force of Callisthenes' objection. Plutarch starts his account of the Peace by noting that the Greek effort at the Eurymedon so humbled the King's pride that he συνθέσθαι τὴν περιβόητον εἰρήνην ἐκείνην. He then states two of the treaty's articles relating to the boundaries by land and the boundaries by sea. The following sentence begins καίτοι Καλλισθένης οὐ φησι ταῦτα συνθέσθαι τὸν βάρβαρον. Those who hold that Callisthenes is objecting to the authenticity of a formal Peace obviously believe that the pronoun ταῦτα refers to the εἰρήνη mentioned at the beginning of Plutarch's account. Yet, if this were so, the construction would seem to require a feminine pronoun in the singular and not the neuter plural. It is really more likely that the referent of ταῦτα is the two articles of the Peace with which Plutarch concluded his first sentence, that is Plutarch is telling us that Callisthenes denied only that the Persians agreed to *these* terms. This reading will receive added confirmation after the subjects of the infinitives ἀπέχειν and πλέειν are determined. There is no problem with the subject of ἀπέχειν, it is surely 'the King' supplied from the preceeding line because there can be no doubt that it was the king who was to stay away from the Greek sea. There is, however, more difficulty in determining the subject of πλέειν. It is generally assumed that the subject of this infinitive is also 'the King'. For example, R. SEALEY offers the following paraphrase or translation (it is difficult to tell which) of this crucial clause: «Whereby ... he the King should not sail in a warship beyond the Chelidonian Islands.» SEALEY is, of course, correct in saying that the King was not to sail beyond, that is west of these islands, but this is not what the Greek says and it is important for an accurate understanding of Callisthenes' point that the Greek be rendered precisely: it was forbidden for one of the parties to the treaty (leaving, for the moment, the subject of the infinitive undetermined) to sail *within* (ἐνδον) the Cynaean and Chelidonian Islands.⁴¹ The Chelidonian Islands are located off the southern

perhaps even a forgery. This conclusion is not at all certain. There are many fifth-century inscriptions which were cut, at least in part, with Ionic letter forms. For examples of public texts in Ionic script earlier than 403/2, see L. THREATTE, *The Grammar of Attic Inscriptions*, Berlin – New York 1980, 31. This inscription was a League document and, if there were any basis on which to question the authenticity of the stone, it seems to me that such a basis would be the presence of a local Attic script rather than that of Ionic script which is certainly more likely to have been preferred for use in League documents. In fact, there is some slight evidence that this inscription was written in Ionic dialect. In Plutarch's account of the Peace (Cim. 13) we read ἐνδον δὲ Κυανέων καὶ Χελιδονίων ... μὴ πλέειν. πλέειν is an Ionic form. In paraphrasing Craterus' copy of the inscription, Plutarch has retained this phrase (μὴ πλέειν) from the original Ionic text. It may be that Theopompus, although he did not know it, criticized an authentic inscription of the mid-fifth century which recorded the terms of the Peace, was cut in Ionic script, and written in Ionic dialect.

⁴¹ SEALEY, *op. cit.*, 329. J. H. OLIVER's interpretation, *Historia* 6, 1957, 254, that the Peace 'forbade Persia to sail within the Chelidonian Islands' is mistaken.

tip of Lycia. Their geographical location makes it very unlikely that it was the Persian fleet which was prohibited from sailing within these islands. First, the preposition ἔνδον requires that the voyage made by those who were likely to violate the prohibition be from the west to the east. Second, such a prohibition would have denied (incredibly) the Persians the right to operate from their ports on the southern coast of Asia Minor, most obviously from ports located in the Bay of Pamphylia. It was certainly a Greek fleet which was not to sail within the Chelidonian Islands.⁴² This reading of the text enables us to come a little closer to the precise meaning of Callisthenes' objection. According to the terms of the treaty, the Greeks were not to sail within, that is from their perspective to the far side of the Chelidonian Islands. But Callisthenes had information (we do not know from what source and it is pointless to use speculation about the origin of this report in order to discredit it) that Pericles and Ephialtes, in fact, did sail beyond (ἐπέκεινα) these islands. Callisthenes tried to reconcile this contradictory evidence by questioning the accuracy of the limit defined by the Chelidonian Islands, that is he claimed that this boundary was not the one agreed to by the Persians. So far this reading of the text is possible, but a convincing interpretation of this text must take into account the meaning of ἔργω δέ and what follows. Admittedly, Plutarch's condensed report makes a clear understanding of Callisthenes' point difficult, but an examination of the *Gliederung* of this passage may help to clarify his meaning. The statement of the limits by land imposed by the Peace on the Persians (τῆς Ἑλληνικῆς ἀπέχεν θαλάσσης) is balanced by the observation that in reality these limits were observed (καὶ μακρὰν οὕτως ἀποστήναι τῆς Ἑλλάδος). On the other hand, the statement of the limits by Sea imposed by the Peace on the Greeks (ἔνδον Χελιδονίων μὴ πλέειν) is balanced by the observation that the Greeks did not observe these limits (Περικλέα καὶ Ἐφιάλτην ἐπέκεινα πλεῦσαι Χελιδονίων). The phrase ἔργω δέ is not an indication that Callisthenes believed in a 'de facto peace', but, rather, the expression chosen to link by contrast the post-war world as it existed in the Peace with the post-war world as it existed in reality (ἔργω δέ). In making this connection Callisthenes introduced a second explanation of the contradictory evidence he had before him. Callisthenes concluded

⁴² I translated Κυανέων as the Cyanaean Islands, located in the mouth of the Bosphorus, because I believe Aelius Aristides' understanding of this boundary is correct (Panath. 153): εἴσω μηκέτι πλευνεῖσθαι, πρὸς μεσημβρίαν μὲν Χελιδονέας, πρὸς δὲ ἄρκτον Κυανέας θέμενος. The result in the north was that a Greek fleet was permitted access to the Propontis, but forbidden entry into the Euxine. WADE-GERY, HSPH Suppl. 1, 1940, 136 with n. 1, considered this possibility and then rejected it in favor of the traditional identification of Κυανέαι with the city in Lycia which bears this name. More recently, SEALEY, op. cit., 330, rejected the identification I am suggesting on the grounds that 'there was no point in naming the Bosporan Cyanaeae as a sea limit since the Persian Empire had no Black Sea fleet'. This is irrelevant because the fleet forbidden to sail within these islands was the Greek fleet. There has been a good deal of confusion about the boundaries set by the Peace and this confusion has been considered by some, e.g. SEALEY, op. cit., 329, reason to question the authenticity of the Peace. The problems associated with the boundaries are too complex to be given the attention, in this paper, which they deserve, but I would like to note that I find SEALEY's arguments unconvincing and I do not see any problem with the boundaries which can be used as an argument against the authenticity of the Peace.

that the terms of the Peace he knew were correct, but that a simple solution to the dilemma was to accept that they were violated. The lack of Persian response to this violation bothered Callisthenes (καὶ μηδὲ αὐτοῖς ναυτικὸν ἀπαντῆναι), but he attributed this to Persian weakness as a result of their defeat at the Eurymedon (διὰ φόβον τῆς ἡττῆς ἐκείνης). In sum, my argument is that the evidence from Callisthenes in Plut. Cim. 13 does not force us to the conclusion that Callisthenes rejected the existence of a formal Peace in favour of a 'de facto peace'. It seems, rather, that Callisthenes was faced with evidence of voyages made by Pericles and Ephialtes which contradicted what he knew to be the boundaries set down by the terms of the Peace and he tried to reconcile the evidence in two ways. The first was to deny that the boundary which he knew was violated (the Chelidonian Islands) was the boundary set by the Peace. This reading is made possible first, by understanding that the referent of ταῦτα is the terms of the Peace and not the existence of a formal Peace and, second, by a correct identification of the Greeks as the subject of πλέειν. Callisthenes' second, more cynical, and, I believe, historically accurate attempt to explain this dilemma started from an acceptance of the correctness of the limits set by the Peace and concluded that the reality of the situation after the Peace was not what, according to the terms of the treaty, it should have been. On both readings, Callisthenes' testimony, rather than providing evidence to question the authenticity of the Peace, would seem to be strong evidence in support of the Peace's authenticity because his discussion of the Peace assumes the existence of a formal Peace.⁴³

The final text we have to consider is Plut. Per. 12, the so-called building debate. D. STOCKTON correctly noted that this debate speaks of on-going hostilities with the Persians at precisely the time (the early 440's) when the orthodox view holds that the Peace of Callias was made.⁴⁴ STOCKTON was, of course, not the first to appreciate the power of this argument. BUSOLT was so impressed by this evidence that he chose to place the Peace after both the building debate and construction on the Parthenon in 447/6.⁴⁵ MEIGGS believes that the difficulty raised by the building debate is not 'insuperable'. He claims that the passage is unreliable owing to rhetorical inflation or even invention. This arbitrary solution is unsatisfactory because it gives no real reason for disbelieving Plutarch on this point.⁴⁶ And in this case it is particularly difficult to credit the common argument that, because a text is thought to be rhetorical, it is somehow unreliable as evidence. Per. 12 recounts a speech or summarizes several speeches given in a mid-fifth century debate. The topic of Athens' relations with her allies was doubtless as vigorously debated in the fifth century as it is now. In view of the inflammatory subject matter, if there were any argument based on rhetoric upon which the historicity of what Per. 12 has to tell us about the state of war and peace could be questioned, one would think that it would be the absence from (and not the presence of) rhetoric in Per. 12 which would cause the passage's value as evidence to be questioned. Surely one need not believe that every piece of information in Per. 12 is free from distortion,

⁴³ On Callisthenes in general, see E. SCHWARTZ, *Hermes* 35, 1900, 106.

⁴⁴ *Historia* 8, 1959, 70.

⁴⁵ BUSOLT, *GG*, 351.

⁴⁶ MEIGGS, *AE*, 132.

whether on the part of the orator or in transmission, in order to accept one clear piece of information we can learn from this passage, namely that at the time of the building debate the Athenians considered themselves to be at war with Persia. The building debate is solid evidence against a Peace thought to have been made in the early 440's, but this argument has absolutely no effect on the authenticity of a Peace made after the Eurymedon. The acceptance of a Peace in the 460's does not force us into the methodologically undesirable position of gratuitously disposing of sound evidence for the existence of war between Athens and Persia in order to establish the historicity of a Peace which has no good evidential support.

The Date of the Congress Decree

For the past fifty years historians of the Pentecontaetia have presented the date of the congress decree (ca. 449) as certain.⁴⁷ Strangely, in these writings there has been no indication that the decree's date was, in the nineteenth century, the subject of a lively scholarly debate.⁴⁸ The current incurious attitude is all the more surprising, in an area

⁴⁷ In referring to ch. 17 of Plutarch's *Per.* as the congress decree, I follow the tradition started, as far as I have been able to tell, by WADE-GERY, *JHS* 52, 1932, 216 n. 47. But, since we do not have epigraphical evidence for the congress decree, I see no reason to print the first letters of 'congress decree' in upper case as is the tradition with names given to decrees for which there is epigraphical evidence, e.g. the Coinage Decree.

⁴⁸ On the one side of the debate were A. SCHMIDT and F. KOEPP who, following the lead of K. O. MÜLLER and A. HOLM, argued that the congress decree belonged to the 460's. On the other side was BUSOLT who, more than any scholar, was responsible for the wide-spread acceptance of a date in the early 440's. It has also been argued that the decree was passed after the Thirty Years' Peace (446). Of the scholars who have held that the decree was passed this late, only BUSOLT, *RhM* 38, 1883, 150, (before he came out in 1897 in favor of a date in the early 440's BUSOLT advocated a date ca. 438) and H. B. MATTINGLY, *Historia* 10, 1961, 166, have offered arguments in support of this position. BUSOLT's first argument starts from the proposition that the date which LIPSIUS assigned to the Eleusinian Decree is 'reasonably secure'. BUSOLT then pointed to what he thought was a panhellenic tone in both texts and concluded that the congress decree was passed at about the same time as the Eleusinian Decree. This argument is indecisive, however, because the Eleusinian Decree has no reasonably secure date and consequently is of no help in determining the congress decree's date (on the problem of the date of the Eleusinian Decree see ML 73). BUSOLT's second argument is that the correspondence between the four-fold order in which the envoys of the congress decree were dispatched (11–20) and the presence of four headings in the tribute list of 438 (Ionia, Hellespont, Thraceward regions, Islands) speaks for a date ca. 438. Later, noting discrepancies between the envoy-order and the tribute list divisions, BUSOLT rejected a date ca. 438 in favor of one ca. 448 (GG, 445 n. 2). MATTINGLY, urging a date ca. 438, has recently attempted to salvage BUSOLT's argument from the tribute list divisions. Subsequently, he too abandoned this position on the date of the congress decree (CQ 16, 1966, 187 n. 5). The strongest argument in favor of a date after the Thirty Years' Peace has not survived the criticism of its own proponents and there is no other reason to believe that the decree was passed at this time. For a more complete review of the tribute list divisions see B. D. MERITT and H. T. WADE-GERY, *JHS* 82, 1962, 72; B. D. MERITT, *GRBS* 8, 1967, 125–26; and ATL 11–12.

as well researched as the Pentecontaetia, because what evidence we have for the decree informs us that it was passed in the 460's.⁴⁹ But, before the evidence in favor of the decree's being passed in the 460's is considered, the influential arguments which have sent the possibility that the decree was passed in the 460's to a scholarly limbo should be completely reviewed. Since the text of Per. 17 is central to any argument on this issue, it is printed in full:⁵⁰

Ἀρχομένων δὲ Λακεδαιμονίων ἄρχεσθαι τῇ αὐξή-
σει τῶν Ἀθηναίων, ἐπαίρων ὁ Περικλῆς τὸν δῆμον ἔτι
μᾶλλον μέγα φρονεῖν καὶ μεγάλων αὐτὸν ἀξιοῦν πραγμά-
των γράφει ψήφισμα, πάντας Ἑλληνας τοὺς ὅποιοιτε κατ-
5 οἰκοῦντας Εὐρώπης ἢ [τῆς] Ἀσίας παρακαλεῖν, καὶ μικρὰν
πόλιν καὶ μεγάλην, εἰς σύλλογον πέμπειν Ἀθήναζε τοὺς
βουλευσομένους περὶ τῶν Ἑλληνικῶν ἱερῶν, ἃ κατέπερ-

⁴⁹ Examples of this unquestioning faith in a date in the early 440's can be found in all of the standard handbooks on this period and in most special studies, e.g. E. BAYER and J. HEIDEKING, *Die Chronologie des Perikleischen Zeitalters*, Darmstadt 1975, 141 and n. 8, 149 and n. 33, 154.

⁵⁰ The text is K. ZIEGLER, *Plutarchi Vitae Parallelae* vol. 1, fasc. 2, Leipzig 1964. C. COBET, *Mnemosyne* 1, 1873, 112–14, has convincingly demonstrated that the source for ch. 17 was Craterus' συναγωγή ψηφισμάτων. The most compelling reason for following COBET is not so much (as many have claimed, most recently S. PERLMAN, *Historia* 25, 1976, 8) that the *language* of ch. 17 points to an inscriptional source. We conclude that Plutarch used inscriptional evidence by the presence, in his account, of *details* which would not have been transmitted in any other way than through an inscription or a copy of an inscription e.g., the number of envoys and the age-limit found in lines 11–12 of ch. 17 (for exactly the same provision in a decree regulating Athenian relations with Methone and Macedon, see ML 65, 17). The description of the envoy-routes detailed in Per. 17 (11–21) supports the same conclusion. It is true that MEIGGS seeks to explain these facts differently (AE 512) by suggesting that Plutarch may have used a separate collection of Pericles' decrees. There is no evidence for such a collection (Plutarch, Per. 8 does not refer to a separate collection of Pericles' decrees) and I prefer not to invent *ex silentio* yet another encyclopaedic source for Plutarch (see my n. 11). While I agree with COBET that Plutarch's source for the congress decree was Craterus, I do not share his conviction that the actual wording of the decree can be reconstructed. COBET's attempt at reconstruction of the decree's *Wortlaut* has recently been presented by J. M. BALCER, *Historia* 23, 1974, 36: «The verbs in section B (present, active, infinitive, παρακαλεῖν «to summon» and πέμπειν «to send») differ from that of section C (aorist, passive, indicative, ἐτέμφθησαν «they were sent forth»). Section B, therefore, remains as Plutarch's edition of the official text as perhaps obtained by Krateros». Yet Plutarch's language speaks decisively against BALCER's claim. As H. A. HOLDEN noted in his commentary on the «Pericles» (London, 1894) κατέπερσαν (7) is an «unclassical» word. The compound καταπίπτειν is first attested much later than the fifth century (Polybius, Dio, Herodian) and appears frequently in Plutarch. Even in the section of ch. 17 which might be thought most likely to contain the original wording of the decree, it is impossible to tell with certainty what this original wording was. (However, it may be possible that the Ionic πλέωσι, line 10, does reproduce the *Wortlaut* of the inscription, see my n. 40). I do not doubt that something very much like κατέπερσαν was written in the decree. It is, however, essential to recognize that our evidence for the congress decree is literary and not epigraphical. Arguments based solely on the premise that words or phrases in ch. 17 must reflect epigraphical usage of the fifth century are, therefore, unconvincing.

σαν οἱ βάρβαροι, καὶ τῶν θυσιῶν, ἃς ὀφείλουσιν ὑπὲρ τῆς
 Ἑλλάδος εὐξάμενοι τοῖς θεοῖς, ὅτε πρὸς τοὺς βαρβάρους
 10 ἐμάχοντο, καὶ τῆς θαλάττης, ὅπως πλέωσι πάντες ἀδελῶς
 καὶ τὴν εἰρήνην ἄγωσιν. ἐπὶ ταῦτα δ' ἄνδρες εἴκοσι τῶν
 ὑπὲρ πεντήκοντα ἔτη γεγονότων ἐπέμφθησαν, ὧν πέντε
 μὲν Ἴωνας καὶ Δωριεῖς τοὺς ἐν Ἀσίᾳ καὶ νησιώτας ἄχρι
 Λέσβου καὶ Ῥόδου παρεκάλουν, πέντε δὲ τοὺς ἐν Ἑλλησ-
 15 πόντῳ καὶ Θράκῃ μέχρι Βυζαντίου τόπους ἐπήεσαν, καὶ
 πέντε ἐπὶ τούτοις εἰς Βοιωτίαν καὶ Φωκίδα καὶ Πελο-
 πόννησον, ἐκ δὲ ταύτης διὰ Λοκρῶν ἐπὶ τὴν πρόσοικον
 ἤπειρον ἕως Ἀκαρνανίας καὶ Ἀμβρακίας ἀπεστάλησαν.
 οἱ δὲ λοιποὶ δι' Εὐβοίας ἐπ' Οἰταίους καὶ τὸν Μαλιέα
 20 κόλπον καὶ Φθιώτας [καὶ] Ἀχαιοὺς καὶ Θεσσαλοὺς ἐπο-
 ρεῦντο, συμπεύθοντες ἰέναι καὶ μετέχειν τῶν βουλευμά-
 των ἐπ' εἰρήνῃ καὶ κοινοπραγίᾳ τῆς Ἑλλάδος. ἐπράχθη δ'
 οὐδὲν οὐδὲ συνῆλθον αἱ πόλεις, Λακεδαιμονίων ὑπεναντιω-
 θέντων, ὡς λέγεται, καὶ τὸ πρῶτον ἐν Πελοποννήσῳ τῆς
 25 πείρας ἐλέγχθεισης. τοῦτο μὲν οὖν παρεθέμην ἐνδεικνύ-
 μενος αὐτοῦ φρόνημα καὶ τὴν μεγαλοφροσύνην.

The case against the possibility that the congress decree was passed in the 460's rests primarily on two objections made by BUSOLT. First, he believed that the first two lines of Per. 17 provide an indication that passage of the decree in the 460's was impossible: «denn die Lakedaimonier begannen über den Aufschwung Athens bereits vor dem politischen Hervortreten des Perikles beunruhigt zu werden und Vorkehrungen zu einem Kriege zu treffen.»⁵¹ The most likely interpretation of BUSOLT's objection is the belief that Pericles was not an active politician in the middle to late 460's. Yet, the most recent and thorough study of Pericles' date of birth shows that he was born sometime between 498 and 494.⁵² He was, then, at the time I am suggesting he proposed the decree, at least thirty years of age – certainly old enough to have begun a political career. We know little about Pericles' early political career, but it is recorded that in

⁵¹ RhM 38, 1883, 150. At Thuc. 1, 95, 7 (the change-over from Spartan hegemony of the Hellenic League to Athenian hegemony) we are told that relations between Athens and Sparta were friendly: καὶ σφίσιν ἐν τῷ τότε παρόντι ἐπιτηδεύουσιν. In Thucydides' account we read nothing more about relations between these two states until 1, 101, 2 where we are told that Sparta promised (unknown to the Athenians) help to Thasos against Athens. Spartan displeasure with Athens became apparent (the well-known διαφορὰ φανερά) shortly thereafter when the Athenian expedition to Ithome was dismissed (1, 102, 3). Thucydides' introduction of a change in relations between Athens and Sparta only in the middle/late 460's suggests that (at least as far as he was concerned) between 478 and this time there was no appreciable change in their relations. The conclusion that Plutarch intended his note in line 1 of Per. 17 to refer to the time of the open split between these two powers is obvious. In this connection it should be noted that the author of the *Menexenus* also considered the late 460's the time at which Spartan envy of Athens began to surface.

⁵² J. K. DAVIES, *Athenian Propertied Families 600–300 B.C.*, Oxford 1971, 455.

473/2 he was a victorious choregus.⁵³ More important, his prosecution of Cimon for bribery in 463/2 shows that he was actively engaged in politics at precisely the time I am suggesting the congress decree was passed.⁵⁴

It is possible, though, that I am being critical of the letter of BUSOLT's objection, but not fair to its spirit. Perhaps BUSOLT meant to argue that, even if Pericles had made his entry into politics by the 460's he would have been too young and inexperienced a politician to have proposed a decree as ambitious as the congress decree. If this is what he meant, he was anticipated by E. CURTIUS who cast the objection in this way: «Aber der Antrag [the congress decree] setzt bei den Athenern wie bei Perikles ein hohes Selbstvertrauen voraus.»⁵⁵ In lines 25–26 we are told by Plutarch only that he included the decree in the *vita* to illustrate Pericles' μεγαλοφροσύνη. Nowhere in ch. 17 is it suggested that the decree was the work of a mature or self-confident Pericles. In fact, it may be that Plutarch chose to illustrate Pericles' greatness of mind with one of his early political acts precisely because the μεγαλοφροσύνη which characterized Pericles' political career is exemplified most clearly by the over-confident and ambitious effort of a young, bold man whose ability was not yet equal to his political ambition.

Somewhat later BUSOLT made another objection: «In Athen selbst war man ferner nach dem Sturze des Areopags vollauf mit der Durchführung der demokratischen Reform und erregten Parteikämpfen beschäftigt.»⁵⁶ In making this claim BUSOLT ignored the one piece of evidence which we have on the relationship between the congress decree and the political climate at Athens when the decree was passed. Lines 2–3 of Per. 17 suggest that the decree was passed during the kind of unrest which BUSOLT believed precluded its proposal. Unfortunately, BUSOLT did not attempt to square his hypothetical claim with the very different account our evidence presents. On the other hand, there is nothing which prevents us from concluding that the decree was passed during that well-known period of unrest which accompanied the reform of Areopagus. There has been no valid objection made against a congress decree set in the 460's and BUSOLT's claim that his objections invalidate the possibility that the decree was passed in the 460's is not justified. On the contrary, both of BUSOLT's arguments, carefully considered, point to aspects of our evidence which, rather than insuring that the decree could not have been passed in the 460's, actually suggest that Plutarch indicated that the decree was passed in the 460's early in Pericles' career.

The Case for a Date in the Early 440's

A date ca. 449 is generally accepted on the grounds that it is securely fixed by its relation to the Peace of Callias (*terminus post quem*) and construction on the Parthenon in 447/6 (*terminus ante quem*). There are two versions of this argument – one made by

⁵³ IG II² 2318. Pericles was the choregus for Aeschylus' «Persians».

⁵⁴ Ath. Pol. 27; Plut. Cim. 14; Per. 10.

⁵⁵ Die Stadtgeschichte von Athen, Berlin 1891, 139. G. GROTE, *History of Greece* vol. vi, London 1857, 25 and n. 1, made a similar objection.

⁵⁶ GG, 445 n. 2.

BUSOLT and one made by H. T. WADE-GERY.⁵⁷ Both began with the assumption that the congress decree pre-dated the construction on the Parthenon in 447/6. Both also concluded (though by different routes) that the congress decree was passed shortly before this construction was begun. In order to establish his conclusion, BUSOLT proposed two possible chronological contexts (461–59 and 449–47) and concluded that the decree was passed in the early 440's, not by arguments for that date, but by arguments against a date in the 460's. The arguments BUSOLT brought against a date in the 460's have been seen to be unconvincing and so provide no basis for eliminating the possibility that the decree was passed in the 460's. WADE-GERY reached his conclusion by means of a more positive approach and what might appear a more convincing argument. He claimed that the Parthenon was one of the temples referred to in line 7 of Per. 17 and concluded that, since we know that the Parthenon was begun in 447/6, the decree was passed shortly before construction had been begun.

Although the connection WADE-GERY made is attractive, it is not valid. There are problems both with his line of argumentation and his understanding of Per. 17. He has set out to prove that the congress decree preceded the building of the Parthenon on the grounds that the latter's construction followed the congress decree. If WADE-GERY's claim that the Parthenon was built as a direct result of the congress decree had some support in our evidence, there might be some validity to his assertion. However, there is no such independent evidence and the connection he makes between a datable Parthenon and a general reference to «temples» in the congress decree is not sufficient to prove his point. As it is, WADE-GERY's circular argument only begs the question. The problem, of course, is not whether the decree was passed before the building of the Parthenon (there can be no question of that), but *how long* before. WADE-GERY's most crucial conclusion, namely that the congress decree must have been passed *shortly* before the building of the Parthenon in 447/6 began should, for the following reason, be rejected.

Since the congress never met, proponents of a date for the decree in the early 440's must trust in the hypothesis that Athens, without the desired allied consultation, proceeded unilaterally with the *immediate* replacement of her temples.⁵⁸ Yet lines 22–

⁵⁷ BUSOLT. GG, 446; WADE-GERY, *Hesperia* 14, 1945, 222–23. F. KOEPP, *JDAI* 5, 1890, 268, also believed that the Parthenon was built shortly after the congress decree. He felt confident that a «Cimonian Parthenon» was referred to and consequently set the decree in the 460's.

⁵⁸ The position that the Oath of Plataea is authentic has recently received considerable support from P. SIEWERT in his special study, *Der Eid von Plataiai*, Munich 1972, and T. L. SHEAR Jr., in his dissertation: *Studies in the Early Projects of the Periklean Building Program*, Diss. Princeton 1966. Although SIEWERT does believe that the Oath is authentic, he argues that the clause found in the literary versions of the Oath (Lycurg. in Leocr. 80 and Diod. 11, 29) which claim as a part of the Oath a promise not to rebuild the temples destroyed by the Persians is not part of the Oath taken in the fifth century. The following are his reasons for rejecting this clause: 1) no reference to this clause is found in the Ephebic inscription from Acharnae 2) the style in which the clause is written does not suggest that it is part of the fifth-century Oath 3) some of the *Heiligtümer* which, under

23 speak decisively against the connection – completely inferential – which one must then make. Neither the Parthenon in particular, nor the building program in general, followed shortly after the passage of this decree: ἐπράχθη δ' οὐδὲν οὐδὲ συνῆλθον αἱ πόλεις. This sentence is often given in a paraphrase which creates the impression that Plutarch is telling us in two different ways the same thing, namely that the congress did

the conditions of the Oath, should not have been rebuilt were, in fact, rebuilt. SIEWERT's first objection is inconclusive because there is no reason to expect that the Ephebic inscription recount the Oath in full. Certainly those parts of the Oath which would most reasonably find a place in an inscription of this nature are the clauses which can be pointed out to the Ephebes of the fourth century as example of their forefathers courage and bravery. Of course, the clause concerning the temples does not fit this description and, so, its absence from this inscription is not a basis from which any notion of 'invention' can be substantiated. Similarly, SIEWERT's second objection is somewhat arbitrary. For example, he maintains, p. 104 with n. 234, because the clause concerning the temples is more verbose than the other clauses (in Diodorus' version «20 Wörter nach moderner Worttrennung»), that this is reason to suspect its authenticity. There are, however, more words in this clause not because it is spurious, but because the promise not to rebuild the temples had to be qualified in a way in which the other clauses did not. In the first clause there is no need to explain the Greeks' swearing not to abandon their leaders and to bury the dead – the reason for such a promise would be obvious to all. On the other hand, it is certainly not out of place to note that the unusual act of leaving the temples in ruins was intended as a memorial to Persian impiety: καὶ τῶν ἱερῶν τῶν ἐμπρησθέντων καὶ καταβληθέντων οὐδὲν ἀνοικοδομήσω, ἀλλ' ὑπόμνημα τοῖς ἐπιγινομένοις ἑάσω καὶ καταλείψω τῆς τῶν βαρβάρων ἀσεβείας. Excluding those words used (understandably) to emphasize the totality of the Persian destruction (τῶν ἐμπρησθέντων καὶ καταβληθέντων) and those words used to explain the purpose for leaving the temples in ruins, I, employing a count of words based on SIEWERT's «moderne Worttrennung», count eight words which are essential to the clause's meaning: καὶ τῶν ἱερῶν ... οὐδὲν ἀνοικοδομήσω, ἀλλ' ὑπόμνημα ... ἑάσω. This number of words is certainly not excessive and is not out of line with the number of words used to express the essentials in the other clauses. Furthermore, in Diodorus' version the clause concerning the temples is structurally very similar to the first clause of the Oath which calls on the Greeks to stand fast, that is we have in both cases a main clause with a negative separated by ἀλλὰ from a positive subordinate clause (οὐ ποιήσομαι περὶ πλείονος τὸ ζῆν ... ἀλλὰ τοὺς ... τελευτήσαντας θάψω / τῶν ἱερῶν ... οὐδὲν ἀνοικοδομήσω, ἀλλ' ὑπόμνημα ἑάσω). This parallelism is strong proof against SIEWERT's claim that the clause concerning the temples is so unlike the other clauses that it must be a fourth-century accretion. SIEWERT's third objection has already been countered by SHEAR's, 32ff., thorough analysis of the archaeological evidence. Since SHEAR's valuable dissertation remains unpublished, a repetition of his conclusion, 43, is not idle: «Apart from the two temporary shrines in the temenos of Athena Nike and in the temenos of Athena Polias, there seems to have been no other construction of religious buildings on the Acropolis between 479 and 449 B. C. No more elaborate reconstruction was undertaken in this period; and only necessary measures to preserve the city's most ancient relics from the very violence of the weather itself was thought to be consistent with the terms of the Oath.» In this connection, SHEAR also pointed out that the sanctuaries in the lower city which were destroyed were not rebuilt. R. MEIGGS, AE 104, comes to a similar conclusion regarding the archaeological evidence. Finally, I would like to note that, although SIEWERT is aware of the evidence from Plut. Per. 17 and does believe that the decree is authentic, p. 103 n. 226, he does not seem to have recognized that the Athenian proposal to discuss the status of the temples destroyed by the Persians, supports the authenticity of the clause in the Oath which specified that the ruined temples were not to be rebuilt.

not meet.⁵⁹ However, Plutarch did not write the two parts of this sentence to tell his reader the same thing twice. The first part of the sentence should mean that nothing mentioned in the decree as a possible course of action (7–11) was done as a direct result of the decree. The second part of the sentence highlights the fruitlessness of the decree by pointing out that the cities did not even come together for the discussions. If Plutarch had written only ἐπράχθη δ'οὐδέν then his reader could be left with the mistaken impression that there had been discussion, but the participants could not agree on any course of action. Not only were there no discussions about the safety of the seas, the Peace, and the temples, but there was also no action taken on any of these matters, for example a building program, in the period which followed shortly after the decree's proposal. This evidence must be taken *au pied de la lettre*. A consideration of Plutarch's reason for including the decree in the *vita* reinforces the conclusion that absolutely nothing was done as a result of Pericles' proposal (25–26). Pericles' futile call to the Greek cities to act ἐπ' εἰρήνῃ καὶ κοινοπραγίᾳ τῆς Ἑλλάδος is a very good example of his μεγαλοφροσύνη. The total failure of Pericles' vision (after such a grand beginning) becomes even more noteworthy when it is known that his plan was really so far from delivering anything which it promised that the cities did not even meet. It was, after all, Pericles' greatness of mind, not his greatness of action which the congress decree illustrates.

One might try to discredit this unequivocal statement about the outcome of the decree by claiming that Plutarch was here somehow misled or misinformed. But Plutarch's account of the Periclean building program, set out in detail in Per. 12–15, agrees with the archaeological and other evidence.⁶⁰ In this particular point (a point as important as whether the Periclean building program was, in any way, a result of the reference to the burnt temples in the congress decree) there is no reason not to follow Plutarch when he implies that none of the proposals in the decree was acted upon. Had the Periclean building program in general, and the Parthenon, in particular, been a result of the congress decree's program, as proponents of a date ca. 449 argue, then it could not have been so simply said that nothing was done following Pericles' attempt to call the Greek cities together. And, conversely, since the Periclean building program is correctly dated to the early 440's, it cannot, I would argue, be held that the decree was passed shortly before the building activity of the early 440's.⁶¹

⁵⁹ Cf. KOEPP, *op. cit.*, 286: «Aber die Lakedaimonier hintertrieben die gute Sache, und der Congress kam nicht zu Stande;» MEIGGS, AE 153.

⁶⁰ SHEAR, *op. cit.*, 215 (with some minor discrepancies) and (contra) A. ANDREWES, JHS 98, 1978, 1, who believes that Plutarch's account of the building program is entirely worthless.

⁶¹ It should not be doubted that the Periclean building program began in the late 450's or early 440's. It may be that in proposing the decree in the 460's Pericles intended something like the building program which was realized in the early 440's. My point is simply that in view of the comment ἐπράχθη οὐδέν, the decree recorded in Per. 17 cannot have been passed shortly before the building program was begun. I realize, of course, that if this brief statement by Plutarch were all the evidence we had, it would be impossible to build this thesis on it alone, but the other evidence

WADE-GERY presented another reason in favor of a date ca. 449 for the decree.⁶² It is his view that the clause ὅπως ... τὴν εἰρήνην ἄγωσιν (11) refers to the Peace of Callias and, consequently, that the congress decree must have followed shortly after the Peace. He is correct – the congress decree must follow the Peace. In a recent study of the difference in meaning between τὴν εἰρήνην ἄγειν and εἰρήνην ἄγειν it has been shown that the former means 'to observe the terms of a particular peace' whereas the latter means 'to be at peace'.⁶³ The clause ὅπως ... τὴν εἰρήνην ἄγωσιν embedded, as it is, in a section of Per. 17 which speaks so clearly of the fighting with Persia as over (ἐμάχοντο) must refer to the Peace of Callias. Other aspects of the decree point to the conclusion not only that the decree followed shortly after the Peace with Persia, but also that Pericles' important diplomatic initiative was actually occasioned by the Peace. According to lines 8–10, the Greeks were to discuss performing the sacrifices which they pledged when they were fighting the barbarian. The relationship between the sacrifices, the war, and the congress decree is clear: these were sacrifices which were to be performed only when the war was over and, so, the decree must have been passed after the Peace was made. Another detail of the decree supports this conclusion. Pericles' suggestion in line 10 that the safety of the seas be discussed also betokens an end of fighting. Now that the League fleet was no longer needed in an active war against Persia, the possibility that it be used to keep the seas safe is an expected, obvious topic for discussion.

We can be sure, then, that discussion of sacrifices and of the safety of the seas appears on the congress' agenda precisely because the war was over. The close bond between the decree and the end of hostilities is further confirmed by the compass of the invitation. The authors of ATL have correctly noted that an invitation to congress was extended primarily to those who had fought against the Persians, that is the member-states of the Hellenic League: «The heralds reached every city named on the Serpent Column, except possibly Leukas. We think it was most probable that Leukas was also invited.»⁶⁴ The breadth of the invitation and the topics on the proposed agenda make it clear that the congress decree was an attempt, at the end of the war, to call together the members of the Hellenic League for the purposes of discharging their common religious obligations, celebrating their victory, and planning for the future of the League. WADE-GERY was correct in concluding that the congress decree must follow shortly after the Peace of Callias and, in fact, the decree is a strong proof of the authenticity of the Peace, but, as has been seen, there is every reason to believe that the Peace of Callias was made in the 460's and not in the 440's. This chronology will receive added support by the examination of the evidence presented in the following section.

brought forth in the course of this paper in support of the chronology for which I am arguing suggests that this is what Plutarch meant to say and that he was correct.

⁶² Hesperia 14, 1945, 222.

⁶³ R. SEAGER, *Historia* 18, 1969, 134.

⁶⁴ ATL, 279 n. 20.

*The case for a Date in the 460's*⁶⁵

AD. SCHMIDT has argued that the first two lines of Per. 17 provide a precise chronological indication that the decree was passed before the First Peloponnesian War. His point is simply that the congress decree was passed when the Lacedaimonians were *beginning* to be angered at the growth of the Athenians and since the Lacedaimonians cannot be said to have *begun* to be angered by Athens growth at any time after the hostilities of the First Peloponnesian War, the decree was passed before this war.⁶⁶ A comparison with the evidence from the fifth and fourth centuries confirms SCHMIDT's conclusion. Both Thucydides and the author of the 'Menexenus' describe a very similar period of unrest which resulted in the First Peloponnesian War.⁶⁷ Plutarch's language places the decree in the period which led up to the διαφορά φανερά between Athens and Sparta. He may have included this chronological indication so that the reader, perhaps with Thucydides' well-known account of the διαφορά φανερά in mind, would be given an indication of the date of the decree by relating it to an event which was better known. Our only source for the decree is a tradition which placed it in the 460's between the Peace of Callias and the First Peloponnesian War.⁶⁸

Further evidence from Thucydides may enable us to be a little more precise about the decree's date. As Sparta's war with the Helots wore on, she appealed to others and the Athenians as allies: ἄλλους τε ἐπεκαλέσαντο ξυμμάχους καὶ Ἀθηναίους (1,102,1). When Sparta later dismissed the Athenian contingent, Thucydides tells us she dismissed them alone of the allies: μόνους τῶν ξυμμάχων ἀπέπεμψαν (1, 102, 3). Thucydides' repeated reference to the alliance by which Athens and Sparta were, in the 460's, still bound is essential to his description of the 'open split' between the two cities. The full importance of the Athenians' dismissal is appreciated only when it is realized that Ithome marked the beginning of the open split between Athens and Sparta as well as the end of the ξυμμαχία between them – the Hellenic Alliance.⁶⁹ It is not likely that Athens in 450

⁶⁵ As far as I have been able to determine this date was first suggested by K. O. MÜLLER, *Commentationes tres de Phidiae Vita*, Göttingen 1827, 9. Arguments in support have been added by A. SCHMIDT, *Das Perikleische Zeitalter*, Jena 1877, 47; A. HOLM, *Gr. Gesch.* vol. 2, Berlin 1889, 271. KOEPP, *op. cit.* in my note 57.

⁶⁶ SCHMIDT, *op. cit.*, 47: «Plutarch's (Per. 17) resultierende Zeitbestimmung wird, was man übersehen zu haben scheint, schlagend erhärtet durch Plat. Menex. 13 p. 242, der als die *ersten* Folgen der Eifersucht, d. h. nach Plutarch der Beunruhigung Spartas, die Kämpfe von 459 und die Schlacht von Tanagra setzt; mithin ist auch nach ihm der «Anfang» der Beunruhigung oder der Eifersucht Spartas *vor den Kämpfen von 459* zu setzen.»

⁶⁷ For further confirmation of this point, see my note 51.

⁶⁸ MEIGGS, AE 512, has recently recognized that lines 1–2 of ch. 17 point to a time before the First Peloponnesian War. He chose, however, not to pursue this lead.

⁶⁹ MEIGGS, AE 92, is certainly wrong in claiming that the Athenians, as a result of Ithome, formally renounced the anti-Persian alliance of 480. Thucydides (1, 102, 5) tells us only that Athens renounced her role in the alliance only with Sparta: ἀφέντες τὴν γενομένην ἐπὶ τῷ Μήδῳ ξυμμαχίαν πρὸς αὐτοῦς.

or 449 could have sent out such an invitation to Sparta, based so obviously on common membership in the Hellenic Alliance, after she, in 462/1, dissolved this alliance between them. I am suggesting that Athens' abrogation of the Hellenic Alliance with Sparta was the result of two affronts which she, as a member of the League, had received from Sparta – the dismissal from Ithome was preceded by Sparta's refusal to attend the congress.

It is equally difficult to see how Sparta in 449, after the First Peloponnesian War, could have been expected to attend a congress one of whose objectives (line 10) she (rightly or wrongly) would have construed as an attempt to provide a warrant for the continuance of the League's fleet. Proponents of a date ca. 449 for the decree have been sensitive to the implausible circumstances under which they believe the invitation was extended. They are, however, hard pressed to find a convincing explanation for Pericles' suggesting a congress of Greek states which was bound to be resisted by Sparta. WADE-GERY, for example, believed the decree was a calculated attempt by Pericles to put the Spartans in the following dilemma: Sparta could either attend the congress and be faced with the possibility of authorizing the continuance of the League fleet, or she could decline the invitation and bear the burden of refusing to co-operate ἐπ' εἰρήνῃ καὶ κοινοπραγίᾳ τῆς Ἑλλάδος.⁷⁰ Although this motivation is not impossible, there is

⁷⁰ WADE-GERY, *Hesperia* 14, 1945, 222. Another explanation of this awkwardness has been offered by COBET and BALZER, loc. cit., but both ride roughshod over the evidence by trying, in spite of what Plutarch tells us to the contrary, to put forth an argument which shows that Sparta was not really invited to the congress. So BALZER: «By removing the Peloponnesian League from route 111 [sic] (Sikyon, Corinth, Epidaurus, Sparta, Tegea, Phlious, Mykenai, Troizen, Orchomenos, and Hermione), the «Congress» is limited in membership to only the Delian Confederacy and the mainland states in addition to Achaia.» In support of his removal of the Peloponnesians from the invited, BALZER claims that route iii was poorly mapped and disproportionately large. But we should remember that there were five men assigned to each envoy-group and we need not suppose that they travelled together. The task given the third envoy group may have taken time, but their mission was by no means so strenuous that the difficulty BALZER sees in their completing their assignment justifies the violence which this interpretation does to the evidence. Sparta would not have been invited only if Athens had changed her mind between the passage of the decree and the dispatch of the envoys. If this were the case there would have been an amendment to the decree and it is reasonable to believe that Plutarch would have noted the amendment as, for example, he makes note of Hagnon's amendment to Dracontides' decree in *Per.* 32. BALZER's claim becomes all the more untenable in view of Plutarch's remarks, first, that all the Greeks in Europe and Asia were invited (4) and, second, that the congress failed because the Lacedaimonians opposed it (23). (This and the information in the opening lines of ch. 17 could not have stood in the decree. Plutarch does not name his source. He introduces this information with a simple «as it is said», but this note and the note in the introduction of the chapter (1–5) were most probably taken from the commentary, the τεκμήριον ἔγγραφο from *Arist.* 26, which we know normally accompanied Craterus' text). Perhaps BALZER was led to misinterpret the evidence by his failure to understand lines 24–25: «Since the first attempt at censure occurred in the Peloponnesos» (Italics are mine). A more accurate translation would run as follows – «As the attempt the call to congress was first put to the test in the Peloponnesos». BALZER claims that these words really mean that the Lacedaimonians encouraged the Delphic Amphictyony to torpedo the congress. There is, of course, no evidence which

no evidence which supports WADE-GERY's ingenious explanation. In fact, what we do know about the nature of Pericles' public appearances suggests that he proposed the decree intending that it be passed and that the cities agree to meet – not with the hope that the proposal fail and thereby he could win an ill-defined propaganda victory over Sparta. At Plutarch *Per.* 7 we read that it was Pericles' policy to appear only seldom in public and then only for important occasions. When Pericles' proposal is seen in the context of his rare public appearances, the weakness of WADE-GERY's special explanation becomes clear. It is unlikely that Pericles chose to appear before the assembly to suggest a plan which, in the early 440's – even if it won the support of the Athenian *Demos*, was bound to be rejected by an unwilling enemy, Sparta. And, if it is permitted to respond to a psychological argument based on no evidence with one based on some evidence, it should be noted that it was a Pericles who obviously valued the successes of his public appearances whom the decree's failure would have embarrassed and not the Spartans; the latter could not have been expected to attend a congress one of whose objectives they would have construed as an Athenian attempt to strengthen the League fleet. This is certainly a move to be questioned if made by a seasoned Pericles. WADE-GERY's attempt to explain the awkwardness of Pericles' proposing this decree as late as 449 lacks political and diplomatic plausibility and cannot be easily reconciled with the evidence on Pericles' policy of public appearances. On the other hand, there is no awkwardness in motivation if the decree were passed before the First Peloponnesian War. If proposed in the 460's, the planned congress had some chance of success and, consequently, requires no special explanation: the proposal of the congress was simply the miscalculation of a bold young man whose political successes lay in the future.

The Hypothesis that the Congress Decree is not Authentic

Forgery was first suggested by I. CALABI and has more recently been re-argued by two other scholars.⁷¹ R. SEAGER believes that the congress decree might have been invented in the middle 340's as Athenian propaganda against Macedon. A. B. BOSWORTH also contends that the decree is invented propaganda, but sets it in the context of Philip's policies after Chaeronea, that is the congress decree is Macedonian propaganda directed against Persia. There is no need to discuss the contexts proposed by SEAGER and

supports this claim. Plutarch's words should be taken at their face value – the Spartans were invited and were the first to reject the offer. For a clear and concise statement of the argument (COBET's) which BALCER makes, see A. W. GOMME, *HCT*, 366 and n.1. It should be remembered that the criticism outlined in this note was made against a decree made in the 440's and not against a decree made in the 460's.

⁷¹ CALABI, *Ricerche sui rapporti fra le poleis*, Firenze 1963, 69. R. SEAGER, *Historia* 18, 1969, 134; A. B. BOSWORTH, *Historia* 20, 1971, 608. I am responding at length to SEAGER's arguments because his study is the fullest expression of the forgery hypothesis.

BOSWORTH because the forgery hypothesis is unnecessary. In other words, the objections to the decree's authenticity are unconvincing.

Since there is no ancient testimony which suggests that the decree is a forgery, SEAGER has necessarily based his case on the belief that the details of the decree, the ideas expressed in the decree, and the language of the decree are obscure and out of place in a fifth-century decree, but perfectly understandable as an invention of the fourth century. A few selected examples of the kind of argument used by SEAGER should suffice to show the weakness of his case. SEAGER sees a major stumbling block to accepting the decree's authenticity in lines 4–5 of ch. 17. He objects: «A further feature of the decree which may point in the same direction [sc. forgery] is the division of Greeks into those who live in Europe and those who live in Asia. This viewpoint seems to be the product of the situation which obtained under the hegemony of Sparta, ... It might be argued that this view goes back to the «Peace of Callias» itself, and that lack of any allusion to it in the fifth century is an accident to which no weight should be attached. But, even if the Peace of Callias is genuine the equality of Europe and Asia created by it might not have been stressed until the Peace of Antalcidas.»⁷² I am not at all sure just what SEAGER finds objectionable in these lines of Per. 17. He seems to believe that the notion that the Greeks in Asia and the Greeks in Europe were separated was, in the fifth century, a very dim one which only became crystalized sometime soon before the Peace of Antalcidas. Reference to the debate of the Greeks after the battle of Mycale (Hdt. 9, 106) on the proposed depopulization of Ionia is sufficient to show that men of the fifth century were acutely aware of the very real difference of the position of those Greeks who lived in Europe and those who lived in Asia. Nor is there anything incredible in Plutarch's mentioning the division for the Peace of Callias did contain a clause which insured the autonomy of both the Greeks in Asia and those in Europe. Lycurgus (in Leocr. 73), among others, writes that the Greeks were to be self-governed, not only those dwelling in Europe, but also those dwelling in Asia.⁷³ The point of the autonomy clause was to proclaim and insure freedom for Asiatic Greeks and, at the same time, to reaffirm the freedom which the European Greeks had successfully defended in 479. It would seem, then that the congress decree does reflect a very important division of Greeks which need not be assigned to the realm of fourth-century invention. This division was a natural one and the Greek of the fifth century was certainly capable of making the kind of distinction which SEAGER feels was lacking in his conceptual vocabulary.

SEAGER sees a similar obscurity in the phrase *καὶ μικρὰν πόλιν καὶ μεγάλην* in line 6. In an effort to explicate this obscurity, SEAGER begins by drawing a distinction between an «Athenian context» and a «Spartan context» in which variations of this phrase occur. He argues that, since the phrase does not appear in what he calls an «Athenian context» until the fourth century, its presence in Plutarch's account of a fifth-century decree

⁷² SEAGER, 138. On this point, see IMMERWAHR, op. cit., 41.

⁷³ Similar reference to an autonomy clause in the Peace is found in Diod. 12, 4, 5 and 12, 26, 2.

renders its authenticity questionable. Yet, here again we cannot be sure that the phrase was part of the decree's wording and not Plutarch's paraphrase of something very similar as we saw was the case with *κατέπρῃσαν* (7) and may be the case with the phrase *τὴν εἰρήνην ἄγειν*.⁷⁴ Furthermore, SEAGER never explains what he understands the difference between an 'Athenian context' and a 'Spartan context' to be. The two examples he gives of the phrase in a 'Spartan context' were written by Thucydides, the Athenian. And finally, I question SEAGER's assumption that the phrase is a politically meaningful formula which can, in any way, be the basis of an attack on the authenticity of the decree. It is more likely that the phrase should be viewed as one of many similar polar expressions common in Greek as a periphrasis for 'all'.⁷⁵

The objection SEAGER makes to lines 8–9 is particularly hard to credit. He claims that the sacrifices are difficult to identify, implies that they never took place, and concludes that their being proposed is therefore fictitious. There is nothing obscure about these sacrifices. We have no record of them because the congress never met and consequently they were never made.⁷⁶

The last of SEAGER's objections to be considered is his claim that the decree is not authentic since 'of the freedom of the seas as a topic for diplomatic discussion there is no trace until the fourth century'. This assessment is not quite accurate. In the appeal of the Thessalian merchants to the Delphic Amphictyony for help against the Dolopian pirates (Plutarch, *Cim.* 8) we have a clear example of an instance in which the freedom of the seas was the topic of diplomatic discussion.⁷⁷

SEAGER, to be sure, has other arguments,⁷⁸ and I do not wish to make light of the position he holds by not meeting each of his arguments with counter arguments, but

⁷⁴ See note 50 for an analysis of *κατέπρῃσαν*. SEAGER recognizes, *op. cit.*, 136, that the presence of this phrase in Plutarch's account is no guarantee that precisely these words were written in the decree and, consequently, the absence of the phrase from all surviving fifth-century decrees is no objection to the authenticity of the decree.

⁷⁵ Variations of the phrase (not, however modifying *πόλις*) appear also at Aristoph. *Wasps* 489; *Hdt.* 3, 62, 3 and 5, 106, 2; and *Soph. Tr.* 324. In SEAGER's two Thucydidean examples of a 'Spartan context' we find that in one (1, 125, 5) Thucydides is writing about Sparta; in the other (5, 77, 5) a Spartan is speaking. On SEAGER's reasoning the presence of *μείζονα* and *ἐλάσσονα* at *Soph. Tr.* 324 leads one to the unwanted conclusion that the phrase appears here in a 'Trachinian context'. For other examples of this common polar expression see, E. KEMMER, *Die polare Ausdrucksweise in der griechischen Literatur*, Würzburg 1903, 118 and 132.

⁷⁶ One might be tempted to compare the sacrifices mentioned in the congress decree with the sacrifices which were instituted after the battle of Plataea and then performed annually (Thucyd. 3, 58, 4). But, there is no indication that the sacrifices proposed in the decree were to become anything like the yearly Plataean sacrifices. The most natural interpretation of *Per.* 17 suggests that these were to be sacrifices of thanksgiving to be performed only once – at the end of the war.

⁷⁷ The freedom of the seas is also the subject of 'diplomatic discussion' in *ML* 65, 18 and *IG I²* 58, 10. Other examples can be found in E. ZIEBARTH, *Beiträge zur Geschichte des Seeraubs und Seehandels im alten Griechenland*, Hamburg 1929, 9 and 100 where the evidence is conveniently collected.

⁷⁸ SEAGER's strongest argument is one which affects only the possibility that the decree was passed in the 440's. He observes correctly that the optimism expressed in this proposal is, for the

there is no point in presenting a more detailed study of the case he makes since his other arguments are so similar to the ones I have noted and no more convincing. In sum, the forgery hypotheses fail twice over: there is no ancient authority which questions the authenticity of the Peace of Callias or the congress decree and there is no modern theory of forgery which makes better sense of our evidence than does the view that both events belong to the 460's.⁷⁹

I would like to conclude this study with a word on the absolute chronology of the events considered in the course of this paper. My view of the evidence suggests the following relative chronology: battle at the Eurymedon, Peace of Callias, congress decree, voyages of Pericles and Ephialtes. The victory at the Eurymedon has been variously dated. Some, for example the authors of ATL, place it in the year 469/8.⁸⁰ This conclusion rests primarily on a report in Plutarch *Cim.* 8 that on the occasion of Sophocles first dramatic victory in the archonship of Apsephion (469/8) Cimon and the nine other generals were, by popular acclaim, asked to be the judges of the competition. This has been thought to reflect the respect the Athenian Demos had for these men as a result of the victory at the Eurymedon. On the other hand, R. MEIGGS does not agree

440's anomalous, but, as I have argued, if passed in the 460's, this document is understandable as a miscalculation, on Pericles' part, of Spartan willingness to co-operate 'for peace' and the common good of Greece'. Still, W. K. PRITCHETT, *The Greek States at War*, 1, Berkeley 1974, 68 n. 91, has found the argumentation on behalf of forgery so convincing that he suggests the congress decree 'should be added to the list of documents which Habicht (*Hermes* 89, 1961, 1–35) has shown to be fourth century creations.

⁷⁹ There remains one problem with the forgery hypothesis which its proponents have not adequately explained. It is not possible to deal with this matter in detail in the present study, but it is worth noting that the precise manner in which these forgeries became part of the historical record remains unexplained. For example, A. B. BOSWORTH, *op. cit.*, 615, claims that it is difficult, if not impossible, to identify the culprit responsible for the invention of the congress decree, but he believes that it may be the work of one Anaximines, 'a noted forger'. SEAGER barely addresses the problem, *op. cit.*, 141: 'This would satisfy everyman's general knowledge of history, and no questions would be asked.' This may be true enough, but the question which must be asked is not what 'everyman' would have accepted as history, but what the professional writer, researcher, or more important, the opponents of the orators who made reference to events like the Peace of Callias, would have accepted as history. C. HABICHT, in an influential article *Hermes* 89, 1961, 1 ff., believing that numerous documents relating to the Persian Wars were invented, suggests that they were created as tools of persuasion in fourth-century debates. Are we to believe that Isocrates, for example, only for the sake of embellishment (and these references really are only rhetorical decorations, not references to prove points of substance) created the Peace of Callias and to back up this creation caused an inscription to be cut and deposited in plain sight? Surely before we have recourse to this counsel of despair, serious consideration in the question of all forgery hypotheses should be given to the possibility I suggested earlier in this paper, namely that the renewed conflict with Persia in the fourth century and the proliferation of Peaces encouraged the orators to look back to the famous conflict of the fifth century to find *exempla* of their ancestors' manly and correct behavior which they could hold up for their auditors. These events of fifth-century history suddenly merited frequent reference, not because of their importance in the fifth century, but because of the rhetorical use to which they could be put in the fourth-century.

⁸⁰ ATL, 160.

with this chronology. He believes that the battle took place somewhat later (467/6).⁸¹ I am inclined to agree with MEIGGS on this point. First because his argument is fuller and more convincing than that found in ATL. My second reason for following MEIGGS is a result of one of the conclusions of this study. It has been argued that Callias was in Susa to make Peace shortly after Artaxerxes' accession in 465. This suggests that the victory at the Eurymedon did not take place as early as 469/8, but shortly before the conclusion of Peace in 465/4. The congress decree, then, was passed perhaps in 464/3, but there is no reason why it could not have been passed sooner after the Peace than this, that is sometime in 465. The voyages of Pericles and Ephialtes must have taken place before the death of the latter in 462/1. Since the voyages can be reasonably assumed to be yearly events, one probably took place in 464/3 and the other in 463/2. Plutarch mentions Pericles' voyage before that of Ephialtes, but I do not think that any more precise chronology can be inferred from this.⁸²

⁸¹ MEIGGS, *AE* 81.

⁸² On this chronology, see W. JUDEICH, *Hermes* 58, 1923, 12 n. 2.

