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ALEXANDER AND PHILIPPI

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It is embarrasing to respond to a response, and particularly so to one written by an eminent scholar like N.G.L.Hammond with his characteristic courtesy and urbanity.[1] He has been right on many things, particularly topographical, but like all of us, he has also at times been wrong, and his *ipse dixit* should not take the place of evidence and serious argument. Moreover, a surprising misunderstanding of my own argument in this journal 79 (1989) 64-71 requires correction, since the main point of that article appears to have been missed, and I still consider it an important point of method.

But let me first apologize to Dr. Hatzopoulos for not noting his suggestion regarding the supplementation of the word in line 2, to which I did not have access when I wrote my article. I gladly yield him priority and am happy to record my acceptance of an eminent epigraphist's suggestion. H[ammond]'s idea (pp. 169-70) that the letters ρσιδ are part of an ambassador's name will deserve serious consideration when he proposes an actual name. Professor Bagnall has kindly informed me that a computer search revealed only the following names: Anacharsis, Chersidamas, Magarsis, Karsis, Charsis, Thyrsis. None of them looks particularly plausible for an ambassador of Philippi. But H. may well have a more extensive data base to draw on.

H. claims that I misrepresented him on where he had earlier (CQ n.s. 38 (1988) 383) suggested the embassy met Alexander, and complains that I failed to give a precise reference. Let me quote what he said (loc. cit.): he suggests that the embassy was sent "presumably when Alexander was known to be back in Macedonia, i.e. in November or December 335". I admit I may have been hasty in jumping to the conclusion that an embassy sent to meet the king when he had returned for the winter would meet him in Pella, where embassies sent to meet a king of Macedon are normally reported to have met him whenever a locality is named. It may be that H. was thinking of (say) Dium. But I cannot see that it makes a serious difference, or called for protest.

I am certainly not misrepresenting him when I state that this does not matter now, since he has completely changed his mind on the date and circumstances of the meeting. He now holds (p. 173) that the ambassadors joined Alexander in *spring* 335, "either at Amphipolis or en route to the Haemus area," travelled with him as far as the Danube, and stayed there (whether with or without Alexander we are not told), either because Alexander was too busy to attend to them or because of "unsafe conditions in Central Thrace for unarmed and unescorted ambassadors" (his n. 13). It is as well to make this clear, since he invites the

[1] ZPE 82 (1990) pp. 167-175.

reader to compare "the divergent views on the dating" as earlier expressed and leaves him to gather for himself that he is about to propose an entirely new one.

The new view, rather vaguely propounded, needs more careful study. For background, he tells us that "Alexander had probably familiarised himself with the problems of land tenure in the vicinity of Philippi in the winter of 336-335, and they were in his mind when he set off on the spring campaign in 335" (p. 173). He then, as we have seen, picked up the envoys of Philippi and took them with him on a long and tiring march north. Now, it was one thing to suggest (reasonably) that Alexander had been told about, and had perhaps even seen, Philippi's problems on his march in spring 335, and so was ready to deal with them when the envoys met him in the following winter. It is quite another to suggest (less reasonably) that he will have steeped himself in those problems of land use and tenure in the winter of 336-335, when the young king, insecurely on the throne for three or four months and challenged from all sides, both at home and abroad, had other things to think of -- and when, as far as we can tell, no one had yet given him any indication that there were even problems to be studied.

Probability decreases as the story unfolds. Alexander now took the embassy with him right up to the Danube (p. 173) -- having, after all, not had enough time to familiarise himself with Philippi's problems. When he sent Philotas and Lysanias[2] back with the booty captured in the battle of the pass, he still had only part of the answers ready and would not yet let the poor envoys return with them. So he entrusted Philotas (this Philotas now, not -- as we had all, including H., believed -- the son of Parmenio) with the text of "his decisions so far" to take to Philippi, which he would probably pass through in any case. Once he got there, it was (very strangely) not Philotas and Lysanias, who had heard Alexander's instructions and had a copy of them, but Philotas and *Leonnatus,* who had somehow got to Philippi to join him (he "may have been left by Alexander to investigate the situation" (ibid.)), who delivered the instructions: it seems that Alexander did not wait to hear the outcome of Leonnatus' investigations, nor did Leonnatus have a chance of hearing the king's instructions "so far" except at second hand. This must have been a disadvantage, for Alexander had instructed Philotas personally "in the details of the boundary-fixing", no doubt memorised from his previous study, and "asked him to publicise the ban on selling timber, which was to stand 'until the embassy shall return'."[3]

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[2] Hammond deduces the status of Philotas and Lysanias from that of two officers given a similar task in Arr. 1,4,5, who later appear as taxis commanders at the Granicus. By an unfortunate oversight he omits any reference to A.B.Bosworth's standard Commentary on Arrian (1980), where the same conclusion is reached by the same argumentation on pp. 56 and 63.

[3] On the idea that it was the sale of "timber" that was forbidden, see my discussion below. As here stated (a temporary ban), the interpretation is correct. But H. imports confusion by almost at once referring to "a *continuing* ban" (p. 173: my emphasis): as we shall see, the correct interpretation (i.e. that the prohibition was to end when the embassy returned) is difficult to reconcile with the motive for it alleged by H.

If this is not sufficiently bewildering, there are further questions. Why should the prohibition "on the sale of timber" (on which see further below) have to be "publicised" by engraving all the "decisions so far"? Would not a notice ("Timber not to be sold until further instructions") prominently posted on the spot have served the purpose? There is nothing in the text we have that orders publication. And what was engraved (no doubt on the city's initiative) was the *whole* of the decisions, probably (as in numerous other Hellenistic examples) because they confirmed the city's rights. The temporary ban on selling whatever it was merely appears near the end, as a minor restriction of those rights, soon to be lifted. Nor is H.'s idea that the sale of "timber" is temporarily forbidden because Alexander needed it for building a fleet (ibid. with n. 14) any more probable. That the king of Macedon should be short of shipbuilding timber unless he forbade the sale of timber from the woods of one city is a paradoxical thesis. That he should then want to forbid it only for a few months at most, until the embassy returned, and should insist on having this temporary ban engraved at the end of a lot of other temporary decisions ("so far") does nothing to increase plausibility.[4]

We do not actually know that Alexander intended to build a Macedonian fleet. (H. states it as a fact without giving evidence.) If he did, he must have changed his mind, for there is no evidence of any Macedonian fleet in the early stages of his campaign. Moreover, despite H.'s frequent references to timber for ships (see especially "the reading 'timber'" (*sic*) in n. 14: he in fact means the reading [ὕλ]ην, which, perhaps rightly, he prefers to the first editor's [γ]ῆν), there is no such clear reference in the text. The Greek word ὕλη can certainly mean building timber, but it is in fact very rare in the sense of "*ship*-building timber". I have not (though H. perhaps has) found it in that meaning without the definition ναυπηγήσιμος. Even thus defined, it is by no means common; but see, e.g., Plato, *Leg.* 705-6, in variation with ξύλα. The normal Greek word for the timber of which ships are made is ξύλον, in fact: thus in Thucydides, Xenophon *et al.* (I shall risk H.'s ire by not boring the reader with quotations), as also in Amyntas' treaty with the Chalcidians (SIG^3 135 line 10). It is, to say the least, surprising to have the meaning of ship-building timber posited, for the sake of proving a thesis, without any discussion. (As we have seen, it will not make satisfactory sense of the text in any case.) The word ὕλη, if indeed here present, will mean "woodland" and no more. Why Alexander did not want it to be sold until the embassy returned (no doubt with more detailed instructions), we simply cannot tell. But the obvious guess is that the issue was too complex to be settled in the text delivered by messenger: there were presumably problems of tenure, and possibly alternatives to be explored on the spot. That the prohibition was to stand only until the embassy returned is in any case made quite clear, and it follows that the ambassadors themselves would bring

[4] H. specifies "a merchant fleet as well as a navy" as being built by Alexander. As to the navy, see my text. There must, of course, have been merchant ships, but we have no idea how many of them were Macedonian, or how many new ones needed to be built.

instructions that would allow the prohibition to be lifted, subject to whatever conditions were imposed.

Enough of this. H.'s new reconstruction does not explain the text any better than his original one: it merely creates new difficulties and implausibilities. But we must now ask: what led him to this complete change of mind, unsuccessful though it turns out to be? I must suggest that, in the light of my article, much as he disliked admitting a possible solution I proposed (on this, see further below), it dawned on him that there was a major problem that he had overlooked: the wording of the temporary prohibition on the sale of the specified land or woodland (constrasted with the full property rights over the marshlands -- and the contrast, incidentally, almost suffices to confirm the obvious meaning of "woodland" for the "ὕλη") shows that the embassy was not expected to return for a long time: H. had totally overlooked this crucial point, until I drew attention to its importance. He had done nothing to explain why the edict was at once engraved, before the return of the embassy would allow the status of the woodland (?) to be finally recorded as well. An alternative to his original time-table (in his article of 1988) obviously had to be developed, to take the uncomfortable fact into account -- as we have seen, it was developed rather hastily -- if his actual supplements in the lacunae, and the conclusions he wanted to draw from them, were to be upheld.

This brings us to the inscription as such. Here H. shows his brand of humour, which should be explained in case it passed the reader by. After a straight-faced disquisition (p. 169) on the recondite fact that some supplements are obvious (e.g. where nearly a whole word survives or because of known formulaic usage), he opines: "The worst restorations are those which are affixed to a minimum of surviving letters and are inspired by a historical theory. Examples of this last category are provided by Badian's proposed restorations." The point of the joke is obvious enough: it is, of course, that H. precisely repeats the point of my own argument about "history from square brackets", and that he is referring to precisely the same parts of the inscription to which I referred as examples of this dubious practice. Indeed, I pointed out that several of the restorations accepted (though not initially suggested) by H., and his whole interpretation of the text based on them, were accepted by him, and suggested by his predecessors, out of a desire to substantiate the "historical theory" (if we may thus dignify it) that the king of Macedon was normally described as βασιλεύς in official texts before c. 331 BC. (See my p. 65.) I did not (and do not) maintain that this theory is false: for all I know, a new inscription may confirm it tomorrow. But I did (and do) maintain that there is at present not a shred of genuine evidence for it, i.e. evidence not derived from "square brackets" and wishful thinking. There is one major difference, in fact, between H.'s approach and mine; and I fear he has totally failed to understand it and has thus inadvertently misrepresented (and massively so) the main point of my article.

Whereas he (still) optimistically regards his preferred supplements in the major gaps as "almost certain" (see his p. 170, top, on one of the more important ones), it was the sole point of my article to make it clear that alternatives were possible, and might in some places provide better sense. I never claimed any status approaching certainty for them: see, e.g., my comment (pp. 66-7) on one of the passages where I proposed an alternative to H's: "It seems obvious that the required sense can be got out of the text ... it would be rash to argue that it is impossible." I merely showed that it was not *necessary*. It seems that, in his conviction that his own preferred conjecture was "almost certain" and his dismay at having it challenged, H. missed this basic difference in our approaches to such conjectures. See further in my summing up (p. 68): "My concern has been to point out how ... we must consider alternative interpretations of this very fragmentary document .... Perhaps some of the missing fragments will turn up ... to settle the matter."

There is unfortunately not a word in H.'s response to show that he has grasped this. The repetition of his "*ipse dixit*" style of argument makes it necessary to repeat the point.

It would clearly be absurd to go over the whole ground again. But one or two examples of our different modes of argument must be given. First, the restoration of line A3, where H. finds his theory of the title of Macedonian kings conveniently confirmed. I quote his exposition (p. 170): "The interval between ἐπ[ρέσβευσαν (in fact, read ἐπ]ρέσβευσαν) and Ἀλε[ξά[νδ]ρον (*sic* H.; in fact, read Ἀλέ]ξα̣[νδ]ρον) has to be filled by πρός and by a definition of Alexander. ... The restoration πρὸς βασιλέα then is 'almost certain', as I wrote..." Of course, once H. has told us what "has to be" there, his restoration becomes more than "almost" certain: it becomes inevitable. Once the conjuror has palmed the card, it is bound to appear when wanted. But this is no way to argue in serious scholarship. H. nowhere says why it should be *necessary* for πρός to be the first word in the lacuna; why could it not immediately precede Alexander's name, with space left before it and not after it? See my p. 65 n. 15, making that point and suggesting two possibilities on that assumption. H. does not like my particular supplements. That hardly matters: I could provide others, even though his refutation of the two suggested is none too effective.[5] It is the method of what one might call the forced card that I objected to -- the presentation of the actual evidence in such a way that the desired result "has to" follow. The word πρός "has to be" where H. places it, only (it seems) in order to *make* his restoration "almost certain". It is easy to find inscriptions in which H's scheme did not appear to be inevitable: e.g. Meiggs-Lewis, GHI 65, lines 16 ff.; cf., e.g., SVA II^2 231 (p. 178) lines 1 ff. and 264 (p. 220) lines 1 ff.: in

5 His two arguments (n.4) are: (1) "To refer back to the lifetime of Philip seems otiose when the inscription contains instructions for the future." The argument is circular. I suggested, of course, that this clause does *not* refer to the future, but to action taken under Philip which (so Alexander may be taken as making clear) need not to be repeated and which, incidentally, would provide a solid basis for some of the adjudications now needed. (2) "... the periphrasis 'my father', instead of 'Philip' - ... is an English rather than a Greek variation." As we shall see, H.'s ideas about Greek grammar and style are sometimes not the standard ones. In this instance, one might refer to a well-known passage from a letter of Alexander quoted in a literary source (Plut. Alex. 28,2: τοῦ τότε κυρίου καὶ πατρὸς ἐμοῦ προσαγορευομένου) where Philip is not mentioned by name but only by a periphrasis. Whether the letter is authentic is beside the point: it was obviously thought so in antiquity, i.e. it was taken to be in perfectly good Greek, not influenced by English.

none of these is there any "definition" of the king of Macedon. In fact, I am not aware of any document of a city in which one of Alexander's predecessors is called βασιλεύς. The insertion of this "definition" of Alexander, straight after his accession (on H.'s interpretation), in a large lacuna can only be met by polite scepticism, and by scrutiny of the method by which it was arrived at.

As regards the interpretation of the aorist infinitive in line A6, I never (of course) doubted the propriety of using that form to issue an order (see my p. 66, quoted above): H.'s elaborate proof of it tilts at windmills (p. 170). I merely pointed out that the clause in which it occurs could be taken as a parenthetical insertion referring to a past action. H. states (ibid.) that in that case "we should expect to find a pluperfect tense, such as we have in l. A12 ἐ]πεισβεβήκ[ασιν". There are various things wrong with this suggestion. First, the form is, of course, a perfect and not a pluperfect. H. seems as vague about Greek grammar as he is about Greek word usage (see above). In any case, the infinitive is demanded by the construction, and the pluperfect has no infinitive (or rather, the perfect infinitive would take its place). For, clearly, the document conveys instructions from Alexander: the constructions will be those of indirect speech, so that the two occurrences of the perfect indicative (also in A8) will be in subordinate clauses, irrelevant to the form here discussed. Second, the aorist infinitive in such constructions, when not expressing an order, regularly stands for a past. See Kühner-Gerth II[3] 1, p. 193: "Nach den Verben des Sagens und Meinens [apart from its jussive use] bezeichnet der Infinitiv des Aorists in der Regel eine vergangene Handlung (entsprechend dem Indikative des Aorists in direkter Rede)." There is therefore nothing wrong with proposing that it may do so here: there would be no overwhelming demand for the perfect infinitive. As for H.'s point that we "should be guided by usage within the text", his examples of aorist infinitives denoting "a single action" are, apart from this instance, in parts so fragmentary that we cannot properly define their function. Finally, he asks what would have been the point of "including a completed action of the past in the arrangements for the future?" The answer, of course, cannot be confidently given, with the text in the state in which we have it. But where details of property rights are in dispute (as they seem to be), a reminder that the boundaries as such have been fixed would by no means be out of place.[6]

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[6] This may the place to deal with his objections to another proposed alternative restoration, no more cogent than the ones discussed in the preceding note. He continues, after the words there quoted: "The restorations which (I offer) in n. 15 (on line A3: the line where he thinks πρὸς βασιλέα "almost certain") are no better; for κοινῇ is redundant as embassies always represent a community, and the mention of their journey is irrelevant, except in as far as ἀναβάντες was chosen to imply an anabasis like those described by Xenophon and Arrian." What I suggested (omitting brackets, not relevant here) was διεπρεσβεύσαντο κοινῇ πρὸς 'Αλέξανδρον, or ἐπρέσβευσαν ἀναβάντες πρὸς 'Αλέξανδρον. I do not understand the last part of H.'s sentence: I did not mean to refer -- and no reasonable reader would take my second suggestion to refer -- to a military expedition, which the word need not imply. As for the first part, H. must surely have heard of instances where more than one embassy was sent by different factions (at one time four Spartan embassies were in Rome): unity might well be worth stressing, in a case where difficult property rights were apparently under discussion. In any case, it is as difficult to say what is redundant or irrelevant in a Greek inscription of the fourth century or of Hellenistic times as (e.g.) in a modern scholarly article. In particular, references to

I see no reason to withdraw my suggestion, as an alternative to (and *not* as superseding) the one supported by H. The difference, as I have said, was the main point of my argument.

Finally, H. again misunderstands my suggestion regarding the possible interpretation of the letters in the first surviving line as referring to some form of "Persis". (In line with the purpose of my article, I added -- though H. seems unaware of it -- that "there are no doubt other possibilities".) This is how he puts it (p. 169): "Having suggested that 'the ambassadors had had to find Alexander in the heart of Asia', he proposed ... to restore 'some form of the word Persis'." And he uses this as an example of restoration "inspired by a historical theory" -- as some of the supplements he accepted, including one he presented as "almost certain", indeed were. I am at a loss to explain how he could have arrived at this misunderstanding of my argument. As the reader choosing to check (my p. 68) will at once see, the whole context dealt with the need to explain the rush to engrave the document we have, without waiting for the embassy to return and settle the one point still in doubt. My answer was that the city had good reason to want the document engraved at once, since (as in comparable Hellenistic examples) it confirmed some of its rights; and that the ambassadors were too far away for their return to be expected at all soon. I went on to suggest that this case would be perfectly met if they were "in the heart of Asia", and went on from this to use the possible restoration of the word in the first line as a pointer in that direction. In other words, my whole argument arose out of the text, not out of any "historical theory".

It is particularly surprising that H. should misunderstand the argument at this point. As we have seen, it seems that it was precisely my argument at this point that drew his attention to the need for accounting for the long delay in the return of the embassy, after the engraving of the stele. (At least, no other obvious explanation for his now trying to account for this by changing his whole chronological scheme comes to mind.) But it must now be pointed out that, whereas my suggestion would explain the delay, his attempt to do so by means of his new chronology would not. For what has to be explained is not only the actual delay and the haste to inscribe the stele. (As we have seen, H.'s new chronology is not very successful at explaining the former and entirely fails to explain the latter.) What has to be explained is the fact that a long delay in the return of the embassy could be *foreseen,* so that the citizens decided not to wait for the return. Now if, as H. postulates, the ambassadors were somewhere around the Danube, being detained either by lack of security on their way back or by Alexander's not attending to them, then these delaying factors might disappear from one day to the next: Alexander might suddenly deal with the remaining points that needed to

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journeys (to be) taken are by no means uncommon. See, quite at random, *SIG*³ 633,108 "after arriving at Heraclea they shall administer the oaths to the Demos"; 656, 38f. "The nomophylakes shall elect two ambassadors to the Teians who, after travelling to Teos and delivering the present decree, ..."; 700, 40ff., the Letaeans order the election of ambassadors who "having travelled to him and greeted him, ..." One might as well say that it is redundant to describe Alexander as βασιλεύς since everyone knew he was king.

be dealt with[7] and the route of their return might suddenly be safe once more (or Alexander might be able to have them escorted), as indeed H. seems to postulate happened "late in the summer or early in the autumn" (p. 173). On no account would a long delay in their return be positively expected: if anything, it might be difficult to understand why they had not returned together with Alexander's message.

I do not for one moment wish to maintain that my suggestion is the only one that will satisfactorily explain the delay and the decision not to wait for the embassy's return. I must repeat that it was the main point of my article to show that no such certainty may legitimately be claimed in the circumstances. But it still seems to me that it does satisfactorily explain it; and I hope to have shown that H.'s hastily conceived new idea comes nowhere near doing so.

But the matter is not only important as a point of method and, in a small way, in casting some light on Alexander's relations with Philippi: for H. its main importance appears to be that it demonstrates (on his supplementation and explanation) the use of the "definition" βασιλεύς for Alexander early in his reign. Another supposed proof of this, the Kalindoia inscription, he has now almost withdrawn, since he has had admit that that text was set up c. 323. Although he will not quite face the plain fact that the use of the title is evidence only for what was done at that time, and not whenever (early in Alexander's reign) the gift was made (I am not concerned with the question as to who received it), and is unwilling to abandon an untenable position with good grace ("Either is possible" -- p. 175!), I am no longer concerned to discuss that inscription.[8] But he has now come up with a new and (it seems) to him convincing proof: the inscription on the wall at Priene. In his earlier article he had not used this for that purpose, and I took great care not to misrepresent his position.[9] But he has now decided to do so, and invited scrutiny.

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[7] It should be pointed out that there is nothing in the text, such as we have it, that suggests that there were points remaining to be settled, apart from whatever message the ambassadors would bring regarding the land (or woodland) that was not to be sold until they brought it. The suggestion that Alexander needed a great deal of time in order to settle other points, and that this was why the envoys had to be detained somewhere near the Danube, is a pure ad hoc hypothesis engendered by H.'s having to explain what in fact it does not. It is interesting to observe that the suggestion that there *may* have been further points to be decided (p. 171 lines 7 f.: "One matter (there may have been others) on which Alexander had deferred a decision") soon becomes certainty (ibid., end of third paragraph: "The text published Alexander's decision on some matters. But decisions on other matters ... were to be brought back from Alexander by the embassy ..."). As I pointed out in my article, I am not at all sure that Alexander had not taken all the necessary decisions even on the matter of the (wood)land: I suggested that that decision may just have been too complex to be sent back at once by express messenger (see p. 134 above).

[8] H. blames me (p. 174 near end) for quoting and discussing only as much of that inscription as was relevant to my purpose, while recognising the limitaton of my purpose! Had I quoted all of it, he would no doubt (very justly) have accused me of quoting far more than I needed.

[9] See my p. 66 lines 4 ff.: "Hammond refers to a third text .... Although he does not in fact use it in connection with this discussion [of the title of Alexander], ..." I do not see why he now (p. 175) claims that I "disliked (his) citation of the title 'king' occurring at Priene": I could hardly have made it clearer that I had no objection to it *as he was then citing it.* It is his present, and different, use of it that is illegitimate (see my discussion in the text).

His argument is that the lettering of the dedication of the temple, which forms the first entry in the "archive" at Priene, is "entirely different" from the lettering of the other texts. He rightly refers to S.M.Sherwin-White's excellent article for this observation. But what is his conclusion? "No doubt the original dedication-stone with its inscription was carefully preserved as a show-piece in the Prienean 'archive'" (n. 17).

The nature of that stone is indeed somewhat mysterious, and I have no solution to propose. But if there is one thing that is certain, it is surely that it cannot be the "original dedication-stone" -- whenever *that* was inscribed.[10] It seems highly unlikely that the original dedication would be engraved at the top of an anta, rather than (as is the usual practice) centrally on the architrave. As for the date of the inscription (which I would therefore regard as an early record, rather than as the original dedication), no archon or other chronological indication enables us to fix it. Sherwin-White's comment was unfortunately not quoted by H.: "The lettering of the dedicatory inscription has a close parallel in another roughly contemporary royal inscription of the same category, the beautifully inscribed architraval dedication of Philip III and Alexander IV from Samothrace" (JHS 105, 1985, 73). She was clearly not thinking of 334. H.'s new idea, properly inspected and checked in the very work which he cities for support, turns out to be useless for his purpose.

H.'s article, largely based on unsupported general statements and misunderstanding of other views, serves to reinforce the point I tried to make my original contribution: that the desire to use lacunose documents to confirm strongly held theories of one's own is likely to lead to pseudo-history, and that the scholar using such texts must preserve due humility before the document. Even supplements arising out of the questions raised by the document itself, as mine were, and presented without extraneous bias, have no claim to certainty: at best, they will be more plausible than those forced on the document by such bias, but they may still be overturned by another find or by more careful study.[11]

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[10] As I pointed out long ago (Essays in Ancient History and Society (1966) p. 74), it is highly unlikely that the temple was awaiting Alexander's arrival at Priene, precisely in order to be at once dedicated by him. Sherwin-White (p. 70) merely says that the building was "well in hand by the end of Alexander's reign".

[11] It is interesting to observe that in his article of 1988 (see p. 131 above), H. remarked (p. 383) that "the ruling of Alexander and the recording of it at Philippi *may be dated confidently* to the winter of 335-334" (my emphasis). As we have seen, two years later we find him dating it, apparently with equal confidence, very differently indeed. Perhaps this will help in illustrating that confidence and certainty are out place in such cases.