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A REPLY TO PROFESSOR HAMMOND'S ARTICLE

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The Editor has kindly invited me to respond to Professor Hammond's note.

Let me first stress that H[ammond]'s note refutes hardly any of the arguments I advanced against his various interpretations of the document and its putative background, as the reader will readily see. My case need not therefore be reargued. Despite his changes of mind, he did not succeed in arriving at a plausible interpretation and his note merely deals with some details. I must also repeat the general point I made both in that article and in ZPE 79 (1989), also cited by him, that no historical conclusions should be based on restorations, since, especially in a non-stoichedon text and in the case of major gaps, any number of restorations are normally possible. The restorations I myself suggested were therefore merely meant to demonstrate that the game is not worth playing, whether or not any of them ultimately turn out to be correct.

They must, however, at least be technically possible, and H. has not proved that any were not. As for διεπ]ρεσβεύσαν[το, a single use by Xenophon proves nothing. We do not know how the word could be used at the time of the inscription, nor indeed for a long time after: there are very few examples. It *can* obviously mean 'to send envoys to various places' (or 'to one another'). But by the time we pick it up in plentiful use, in the second century BC, it can be a general term for sending an embassy (see Mauersberger's examples from Polybius), without any 'distributive' sense. As for κοινῆ, I sufficiently explained it in my article and need not return to it. My restoration (one of two alternatives I suggested, in order not to weary the reader with more, to the one accepted by H.) will do quite as well as his.

By calling it 'his' restoration, I obviously did not mean to imply that he first proposed it: his quotation from my article in his n. 12 sufficiently shows that. He made it 'his' by accepting it and using it for his purpose, and no attentive reader could possibly have mistaken my meaning. Nor, of course, would I ever accuse a colleague of being a 'card-sharper', as H. charges. The card-sharper has many tricks up his sleeve, but the naive one that I called 'the method of the forced card' would hardly be among them: that is the method of the magician's card trick, which we have all seen - not criminal deception, but sleight of hand for entertainment; hence my comment that it is no way to argue in serious scholarship. Again, I cannot believe that an attentive reader could have mistaken my meaning, and I am sorry that H. apparently did. And what I was referring to (again obviously) was not the actual restoration (hence H.'s assurance, correct in itself, that he did not first suggest it is irrelevant), but the statement that the gap 'has to' be filled precisely in that way, that the restoration was 'almost certain' - and that it therefore proved what he was in fact assuming. The reader is again welcome to check this .

As for the four letters that are all we have of the first line, they might well be part of an ambassador's name, but a plausible one has yet to be offered, and until it is, that possibility remains purely theoretical. Nor does H. deal with the main point at issue: that the possibility that Alexander was in or near Persia does fully explain the quick publication of an order intended to be temporary, until the ambassadors returned - a point that, as I showed (and H. does not contest it), none of his alternative reconstructions succeeded in explaining. This, chiefly, is why that must remain the conjecture of choice, where (of course) only conjecture is possible.

As for the Chios text cited as a parallel (I would agree with H. in assigning it to 332), I simply do not see the parallel: the layout, as the surviving heading shows, is totally different. No reconstruction of the Philippi text can produce anything really parallel.

Nor is there much to be said on the supposed pluperfective meaning of a perfect indicative, which H. mistakenly called a pluperfect. Although it is well known that the perfect infinitive and participle can have pluperfective meaning, nothing of the sort has ever been said (certainly not in the grammar-book H. cites) about the indicative, which he called a pluperfect.

Finally, I must repeat that the introduction of the royal title begs an important question.¹ A text recently published shows its potential importance. In *Hesperia* 62 (1993) 249ff. Stephen V. Tracy, one of the most careful of epigraphists, published a partly unpublished fragment of an Athenian decree, the date of which is lost. It is restored as referring to ... τῶν τ[ο]ῦ βασιλέως φίλων καὶ Ἀντιπάτρ[ο]ου who have been honoured by the People and is assigned to Archedicus Naucritou Lamptreus, known as having served under Antipater's oligarchy in 320/19 - both very plausibly. However, the editor conjecturally assigns this text to 'a. 338 *vel* 337 a.?' and suggests that it may belong after Chaeronea, when Antipater and other prominent Macedonians were indeed honoured by the Athenians.

I note and appreciate the question-marks. Yet it is to be regretted that this date should be printed as being the date of prime choice when it is in fact the *least* likely. I.a., Tracy

¹ M.B.Hatzopoulos, in a comment in *Bull.épigr.*, REG 104 (1991) 505 - a note which I could not yet know when, in 1991, I sent my article to *ZPE* - also rejects H.'s reconstruction of the background of the Philippi document. In an earlier note (*ibid.* 103 (1990) 530) he rightly pointed out that if - as both he and I think - the first line refers to Alexander's stay in Persis, the engraving of the document cannot be earlier than c. 330, hence is not relevant to the use of the royal title before Alexander's campaign. However, it would still be of interest to know for certain whether, even at that date, Philippi would use the title, as cities in Asia probably did. In any case, I agree with H. in feeling unable to accept Hatzopoulos' suggestion that, in the middle of his campaign, Alexander sent two of his senior commanders all the way back to Macedon, on a mission that (as far as we could tell) was trivial and that is unnoticed in any of our sources. That interpretation is necessitated by his refusing to permit the possibility that the aorist infinitive can refer to past action and that the reference is therefore to an earlier action by the men named. Again, I must leave the reader to judge. Hatzopoulos also suggests that H. is probably right on the usage of the royal title in Macedon. But he would probably agree with my belief that any statement on this is at present purely intuitive: there is no evidence, and I prefer to have no opinion on this question.

himself later (p. 250) remarks that it might well belong to the period of over ten years when Antipater was acting as viceroy while Alexander was in Asia.² That long span seems quite unobjectionable and ought to have been the first suggestion.

First, a small point: among those honoured after Chaeronea there was, most prominently, the crown prince Alexander. It is unlikely that his name would be omitted when Antipater's was mentioned.

More important: the interpretation suggested involves taking τοῦ βασιλέως as applying to Philip II; and, natural as it may have seemed to the editor (who does not discuss this point), that is totally unattested. I leave aside the question of whether the royal title was used with or without the name in Macedonia itself before Alexander crossed to Asia: we can hardly have a legitimate opinion on that. But the plain fact seems to be that no inscription by a Greek city (Athens or any other) gives Philip that title.³ What is more, in the plentiful evidence provided by the Demosthenic corpus there is not a single example of the title being attached to the name of a Macedonian king, let alone of the phrase τοῦ βασιλέως (used *tout court*), as applying to Philip.⁴ If I am right in this (and I stand ready to be corrected, since anyone may overlook a piece of relevant evidence), then it is *prima facie* literally incredible that the date suggested for this decree could be correct.

The use of the royal title for Macedonian kings, like many small points of philological *akribeia* of which some historians are impatient, turns out to be more than a pedantic quibble.

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NOTIZ DER REDAKTION: Damit schliessen wir die Debatte.

² Tracy refers to a forthcoming article by A.B.Bosworth, suggesting that the decree could have been passed early in 322/1. I have not seen the article and cannot comment, except that that date would certainly be preferable to Tracy's suggested date.

³ The title is indeed never given to any of Philip's predecessors, but is quite probably applied to minor kings: see *IG* I³ 89 line 69.

⁴ To be precise, there is just one example, though the context is quite different from what we have in this inscription: it is used of the Macedonians' attitude to 'their' king - and it occurs in [Dem.] 11.11 (the 'Reply to Philip's Letter'), known to be a late fabrication.