

Bryn Mawr Classical Review

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Response: Fazzo on Golitsis on Fazzo, Il libro Lambda della Metafisica di Aristotele. Response to [2013.06.32](#)

Response by Silvia Fazzo (silvia.fazzo@unitn.it)

Pantelis Golitsis' review of my *Il libro Lambda della Metafisica di Aristotele* is puzzling on more than one account. Its apparently dismissive tone is likely to impress those readers who are not familiar with the material at hand. Hence some clarifications might be helpful.

It should be noted, first, that Golitsis does not seem to have found any major problem in most of the book's constituent parts: not in the edition of the text (which differs from Ross's or Jaeger's or both in some 90 places), not in my translation (which diverges in various respects from most previous ones even when the Greek text is the same), not in the notes to the text (Part III, 223-308), which include a number of new proposals, some of which affect the overall understanding of Aristotle's argument (e.g. at 1069a31-2, which I further develop in *Phronesis* 2013, 160-175; 1069b23; 1071b22; 1072a15; 1072a24-5; 1072b2, on which see already *Elenchos* 2002, 357- 375; 1072b23; 1075b19). What Golitsis has done is to revise details of my critical apparatus¹ – suggesting interesting additions for which I am grateful – and to formulate objections against the stemma codicum (a revised form of Harlfinger's, as discussed in my 2010 article, which he refers to at his n. 5), and the dating of Aristotle's manuscripts and their relationship to one another.

Now, one peculiar feature of Golitsis' review, apart from the fact that he does not argue for his views (which makes discussion difficult), is that I have hardly expressed any opinions on dating or any of the related chronological hypotheses he opposes, either in the book under consideration or in my 2010 article.² However, Golitsis' "objections" are themselves open to rather severe criticisms on other grounds, especially since they deal with what I take to be crucial matters that require special care.

1) A general remark first. Accuracy is especially required when talking about the most important manuscripts in the story of the *Metaphysics*, namely J (9th century), E (10th century), Ab (12th-14th century). We already agree on the relevance of detecting the extent of the β -section. Likewise, according to the same criterion of stemmatic authority, the first hands in the *vetustissimi*, J and E, must be carefully distinguished.

2) Golitsis says that J and J² are "identical". They are not, as a look at even the folios of *Lambda*, 185r-189v, will show. It is not just that J² has a script smaller in size, and is full of abbreviations, whereas J has none: more significantly, J²'s letters are less round and regular; J²'s ductus is more inclined; ligatures differ in the two hands (e.g. εσ, εξ); the letter ξ stands on the line in J, but goes down under the line in J²; and so on. While denying this distinction, which has a stemmatic value, Golitsis fails to recognize a series of differences, unnoticed by former editors too, which I first detected by inspecting J in Vienna: by rasura, J² often corrects J's obvious errors, most remarkably at 1069b34-5, where he supplies J's homeoteleuton (as a look at my app. cr., quoted verbatim by Golitsis, will show). J²'s intervention escaped the editor's attention at 1069a36 as well: there J² cancels J's reading μαθηματικά in favour of μαθητικά found in his own exemplar, an apparently unparalleled *lectio difficilior* which deserves closer inquiry. Both of Golitsis' claims, that the corrector of J is the same as the original scribe, and that he occasionally follows Ab's model (which sounds unlikely even in dating, since J and Ab are so far from one another, and since already Jaeger recognized that Ab's model must have been relatively recent and corrupted, see the "Praefatio" of his 1957 edition, p. x), deny the relevance of a crucial distinction, and downgrade the importance of J itself.

3) In fact, Golitsis claims that J is not the oldest manuscript of the *Metaphysics*; he says it is Y (belonging to the β-tradition; it contains only two fragments, 1056a12-1057a26, 1059a19-1060a15). Now J is dated pretty precisely around the middle of the 9th century; Y is by far the oldest testimony to β – and the only one before Michael's time; but is apparently later than J, dating to the 9th / 10th century according to Harlfinger. Moreover, how can Golitsis be sure, with such a narrow evidence as Y gives, that in the whole of β "had acquired its actual form by the time of Michael"?

4) Indeed, Golitsis dates Ab (except the final section, 1073a1 ff.) to the 11th century. But this date, so far as we know, does not fit any of those hands at work: none has been working before the 12th century, since all of them copied, together with Aristotle's text, sections of Michael of Ephesus' commentary, which was composed around 1118-1138.

5) Golitsis rejects as "manifestly false" what seems to be an obvious fact, namely that the β-tradition is transmitted with Michael's commentary. Yet, sections of Michael are included in both main witnesses to β: in the β part of Ab, and also in the β-manuscript M (from *Epsilon* to *Theta*). This does not mean that Michael's commentary can testify to the text of β, or to Ab's text (whatever branch it belongs to), as I argued more than once (thus criticizing the way it is used in current critical apparatuses). I am both glad and perplexed, therefore, at Golitsis' very last line, where he expresses, as if it were his own conclusion: "Michael ... is not an absolutely safe guide for reconstructing β." Does he realize that he actually agrees with what I argued at length?

6) As for what appears to be E's third hand, E^{Σ} , Golitsis locates it in the 12th century, whereas it is dated to the 13th / 14th century, see Hecquet-Devienne, *Scrittura e civiltà* 2000, 103-171, § 4. Golitsis does not explain his view, although he seems to attribute some kind of stemmatic authority to E^{Σ} 's reading (see below, n. 9).

7) Golitsis criticizes my stemma – i.e. basically Harlfinger's, in the revised form I proposed in 2010. Still, Golitsis's arguments presuppose partial agreement with my revision: he recognizes that Ab splits away from the M-C subfamily in *Lambda* 1-7³. However, he gives an interpretation of this distance which differs from mine, but is based on some new data (which can be read in different ways, though; see below, n. 10). As a matter of fact, this just confirms that he is also working, as I first have been, on the revision of Harlfinger's stemma (which had remained unexplored since 1979). This too, judging from his further comments, requires clarification (see here below, n. 8).

8) In order to edit book *Lambda* based on Harlfinger's stemma, the necessary condition was a first revision, which I produced and announced in 2008; moreover, I found out that the β branch (not only Ab in itself) is lower in value than α ; then every effort was expended on Aristotle's *usus scribendi*, whose careful preservation is a distinctive character of my edition (thoroughly commented upon in Part III of my book). Ground-breaking research toward a new edition of the *Metaphysics* was carried on with running discussion both in Trento and in my Lille seminar on *Lambda*, 2004-2008 (with M. Crubellier and A. Laks). So, I am not sure what Golitsis means when he says that the soundness of my Greek text is "fortunate". He adds that this is due to suggestions by Frede, Patzig. Yet, Frede and Patzig (et al.) edited in 1988 *Metaphysics Zeta*, a book among those whose stemmatic position is crucially different from that of *Lambda*, as my revision of the stemma shows, so that no direct suggestion can be taken from their case (Ab belongs to β in Zeta, to α in *Lambda*); in any case, they opted for a non-stemmatic edition and do not acknowledge (*pace* Golitsis) any β -family at all. Strictly speaking, my edition, submitted as a PhD thesis in 2009, was probably the first stemmatically based edition of a book of the *Metaphysics*, thus opening a path which is now accepted as a common procedure.

9) Yet coherence with the stemma does not always emerge clearly in Golitsis' remarks. Even though he refers to Harlfinger's stemma as based "on safe paleographical grounds", he refers to sources without recognized stemmatic authority. For instance, he adduces readings and remarks due to second or third hands in manuscripts (Vd^2 , E^{Σ}) in order to argue about Ab's controversial stemmatic position. These notes should be uncontroversially located in the stemma before being used.⁴ As for now, they could derive, e.g., from a vulgate influenced by Ab, a case in which their value is none. Even when Golitsis introduces new and interesting data, it seems that he speculates on the seminal role of a manuscript like Ha, which does not play any crucial part in the higher and more relevant part of Harlfinger's stemma. Does he mean that this latter has to be revised on this regard too? Probably so, and this would be a very

interesting finding.

10) But the way this should be done, as he puts it, does not appear clear enough. It is surprising that Golitsis gives as safe and necessary his hypothesis of a common, direct source for M and C, a source "corrected", as he says, with α -readings taken from Ha. For example: their common, distinctive reading at 1072a11 is an obvious lacuna (the main verb εἶναι, which E, J, Ab give, and which we would normally expect, has fallen away). How can it be a correction? At 1073 too, they share a plain omission. All other alleged common readings are shared by other α manuscripts. Moreover M and C clearly do not have the same exemplar (as Bernardinello has shown; M rather shares the exemplar with Ab, up to a certain point, see also Harlfinger's *Theta* collations, p. 34).

11) In all of this, no real argument is given by Golitsis (in spite of his criticism) to question the discovery which I made through my collations: that Ab has already moved from β to α by 1065a26, thus is an α -manuscript for the whole of book *Lambda* (1069a18 ff.). Further confirmation of this can now be gathered from a paraphrase of *Lambda* derived from β in Vk (Vat. gr. 115), discovered by S. Alexandru (publication of whose work is announced by Golitsis at n. 1): Vk entirely confirms my editorial principles and my established text, since it sides with M (and C) and does not support Ab's individual readings.

For now, therefore, Ab's readings can still be explained as degradations of the α -family, as I argued, even if a number (actually, the most significant, or the less insignificant) of M's and C's readings, if Golitsis further develops his findings in this direction, could turn out to be degradations of the α -family too, albeit different from Ab's.

It remains true, by contrast, that these *vetustissimi*, E and J, although independent of each other, are so close to α that their reciprocal agreements are the safest key to α , the most ancient recognizable source of Aristotle's text: *recentiores non deteriores, quandoquidem. sed vetustissimi potiores persaepe, nec sine certa quadam ratione*.⁵

Notes:

1. Golitsis revises or points out two accents, one breathing, a spelling alternative, a more careful record of C at 1070a32 and of M at 1070b22, two elisions with apostrophe, one of which, 1069b3, must imply a correction in my text's constitution. Golitsis also has one suggestion, on 1072a11, but that seems unnecessary ("always" is missing in the protasis too), and uneconomic: it requires four errors (misunderstanding of an uncial letter, haplography, transposition, graphic variance, the first three of which in every chief manuscript except Ab) for one controversial word to be added, ἀεί.

2. E.g. where do I say or agree that M and C are direct copies of β , or of any common exemplar? Or that the α part of Ab is a direct copy of δ ? (By the way,

I take that Harlfinger's uninterrupted line from δ to Ab means that no intermediate manuscript is known so far, not that there has been none, see n.

2.) Or that J and J² are separated by 150 years? Or that Michael's commentary is a safe witness to β ?

3. Golitsis does so more straightforwardly than Primavesi (see reference in Golitsis' note 7) who, at 395-6, expresses perplexities concerning those *minutiae*, as he calls them, i.e. trivial variant readings which are in common between Ab and the β family. But common readings occur after 1073a1 as well, in "Harlfinger's α section of Ab", as well as before, in "my α section of Ab" (quite remarkably at 1075b19, and at 1075b28): once more, the objection applies, as apparently does, to Harlfinger's stemma rather than to my revision of it.

4. The same applies to sources from indirect tradition. Golitsis calls into question a bunch (as it may appear so far, until an accurate analysis can be given and safe criteria are established) of spare, often trivial coincidences of Ab with Arabic translations, which I have myself collected in my apparatus. He notices their relative frequency, but in comparison to what? They cover only a minority, in quantity and relevance, of Ab's readings; as such, they cannot be taken as significant on either part. Generally speaking, as it appears, no safe method for the use of indirect tradition in the text constitution has been fixed yet, as I often pointed out. A joint contribution on the Arabic is planned by Mauro Zonta and myself.

5. I freely reverse Giorgio Pasquali's motto, *recentiores non deteriores*, as quoted at the end of Harlfinger 1979. After all, Pasquali never meant this to be always the case.

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