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Interactive Comment

Interactive comment on "Aligning MIS5 proxy records from Lake Ohrid (FYROM) with independently dated Mediterranean archives: implications for core chronology" *by* G. Zanchetta et al.

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Dear Editor,

Please find here the reply to the reviewers comments on the manuscript: "Aligning MIS5 proxy records from Lake Ohrid (FYROM) with independently dated Mediterranean archives: implications for core chronology" by Zanchetta et al. (published in BGD). We thank both reviewers for their comments, which we feel have been largely addressed.

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Firstly, it is important to note that the revised Francke et al. (2016) manuscript (scheduled for the same issue) based on the interactive discussion and the referees suggestions produced a modified age model, which is now substantially different from the age model published in Francke et al. (2015 BGD). The Francke et al. (2016) final age model now includes a revised chronology for a time-control point for the tephra layer OH-DP-0499, which derives from our manuscript (Zanchetta et al., 2015) in BGD. This is an excellent example of how open reviewing can permit real-time integration and revision of research. The introduction of this point strongly reduced the differences between the age models of Francke et al. (2015) and Zanchetta et al. (2015) during the last glacial/interglacial transition.

Nevertheless, we feel the discussion of the effects of using different age models is an important contribution to better evaluate the reliability of age models. Therefore, our revised manuscript will discuss the use of the different age models from the original version of the Francke et al. (2015, BGD) time series alongside the new time series (Francke et al. 2015, and this manuscript).

Below there is a point by point reply to the referees comments.

Reviewer #1

Setting up a sound chronology is essential for discussing the relative timing of paleoenvironmental changes in the terrestrial and marine realms – especially considering the dramatic changes occurring during glacial terminations. Therefore, the paper of Zanchetta et al. is a timely contribution for improving the fine-tuning of the MIS 5 stratigraphy from Lake Ohrid. Setting up a reliable chronostratigraphy for Termination II and MIS 5 has furthermore implications for adjusting the time-frame of the earlier glacials/interglacial cycles and glacial terminations. In general, the authors present good arguments for revising the timing of Termination II. The shape of the δ18O signal as well as its amplitude (approx. 4‰ decrease during T II) match these of the other presented archives (marine records, speleothems and lake records) quite well – also

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with respect to the location of the P-11 tephra. However, there are some issues that should be considered before publication, especially concerning the later stages of MIS 5. My major points of concern are listed below:

1) I would like to see the TIC-δ18O record being extended in order to fully cover the proposed duration of T II. This might be even more important because the TIC-δ18O record is essentially the foundation of the revised tuning! These are only a few measurements but would certainly strengthen the case of the authors.

Unfortunately, the TIC and δ18O records by Lacey et al. (2016; this volume) and Francke et al. (2016; this volume) are restricted to the interval between 128 and 78 ka. Lake Ohrid sediments comprise abundant endogenic calcite only during the interglacials, and the availability of isotope data is intrinsically linked to the presence of TIC. Carbonate is almost absent during the glacials, apart from discrete and discontinuous bands of early diagenetic authigenic siderite. The calcite and siderite therefore formed at different times in different parts of the lake (Lacey et al., 2016), and the record cannot be extended.

2) The tuning of the younger part of MIS 5 is not convincing. On first sight it seems plausible to tune the marked δ18O increase between the POP2 and POP4/X6 tephra with the respective δ18O increase in the Popoli section and Corchia Cave. However, the location of the tephra within the isotope record of the Popoli section is different from that of the tephra within the DEEP Site (presuming these are the same tephra). It is therefore doubtful if the tuning point (green dot) between both tephra is robust.

Chemical data supporting the correlation between OH-DP-043 & POP4 and OH-DP-0404 & POP2 are provided in Leicher et al. (2016; this volume), so these tie points of correlation between the Sulmona and Ohrid record are independent of climate interpretation. However, of note the position of the two tephras when compared to the δ18O data from Sulmona is perfectly aligned with the δ18O data from Ohrid. Both POP4 tephra and OH-DP-043 occur just after a “negative” spike in δ18O within the

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GS24 stadial, whereas POP2 and OH-DP-0404 are precisely placed at the beginning of a well pronounced positive δ18O spike within the GI23. This δ18O spike in the Ohrid record is also replicated in the TIC and TOC time series data. Therefore, both the chemistry and the fine-scale "climatostratigraphic" position of the two tephra layers are quite convincing, we have improved Figure 3 accordingly.

3) I wonder why there is no comparison of the DEEP data to established pollen records from the Mediterranean realm: For the classical Tenaghi Philippon (T.P.) site, high resolution pollen records are available for MIS 5 (e.g. Milner et al., 2012; Milner et al., 2013; Pross et al., 2015). This record has been tuned to the speleotheme-dated MIS 5 pollen record of Iberian Margin core MD95-2042 (original data by Sánchez-Goñi et al., 1999). Hence, a similar timing as the Lake Ochrid record should be expected for Termination II and MIS 5. Interestingly, comparison of the Iberian Margin pollen record to the planktonic foraminifera δ18O data from the same core show a lag of approx. 4 kyrs by the terrestrial biomes to temperature change, similar to the lake Ochrid data set – another argument for the revised stratigraphy. A similar shortcoming is that no attempt was made to compare the Lake Ochrid record to the Monticchio sequence of the last Interglacial (Brauer et al., 2007) which is independently dated based on varve counting and tephrochronology. While tuning pollen records naturally includes the assumption of synchronous paleoenvironmental changes, a comparison of the DEEP data to these archives should be included for a thorough discussion of the stratigraphy. Yes, we agree with both reviewer#1 and reviewer#2 that using pollen data would improve the discussion. In the Zanchetta et al. (2015; BGD), we aimed to provide suggestions of ways to improve the chronology, allowing the proxy data to be presented in other papers of the special issue. In the revised version we have added a figure (Figure 5) showing the Ohrid pollen record for MIS5 (Sadori et al., 2016) with the new age model proposed here, and have compared this to Monticchio (after Brauer at al. 2007) and Tenaghi pollen records (after Milner et al. 2012 and 2013). We now also discuss this new figure in the text but avoid an in depth discussion of pollen from the Mediterranean region as this is irrelevant for this paper. We have added the following sentences:

"With this new age model it is also possible to attempt a more precise regional correlation of pollen records. In Figure 5 pollen records from Tenaghi Philippon, (Fig. 1, Milne et al. 2012, 2013; Pross et al. 2015) and Monticchio (Fig. 1; Brauer et al. 2007) are plotted against the DEEP site pollen record (Sadori et al. 2016). The sharp increases in the AP percentages at ca. 130 ka is almost synchronous in all the mentioned records, and simultaneous to the highest rate of SST increase in the western Mediterranean (Fig. 4). A comparison of the chronology from different records after the end of the Eemian forest phase is more problematic, since the first clear forest opening coincides with the C24 cold event in North Atlantic (Sánchez-Goñi et al. 1999). In the DEEP core, two tephra layers and a robust alignment point at the end of GI24 probably make this chronology the most reliable even if in the younger part of the record there are no further alignment points."

4) A comparison to summer insolation at 42° N might be helpful as well – note that the TIC peak at MIS 5e coincides with maximum summer insolation in the revised stratigraphy. While this makes sense in terms of climate forcing, this relation breaks down for late MIS 5 in the new stratigraphy. Here the original stratigraphy with the pronounced TIC peak at ca. 82 ka fits better to summer insolation than in the revised version (c.f. figure attached). This offset is odd and should be discussed, if the revised stratigraphy remains as is.

We have added insolation data to Figure 4 and associated text. We would like to point out that when we are building an independent chronology it is necessary to avoid the use of the "tuning insolation paradigm" (which was not completely the case for Francke et al. (2015; BGD)), because it can become a circular argument. When looking for leads and lags within a climate system it is necessary to only deal with independent chronologies. For instance we note that the maximum warming in the SST temperature of Martrat et al. (2014) tuned with Marino chronology (Marino et al. 2015), occurs later than the maximum insolation (the Marino et al. (2015) record is supported by tuning with speleothems). Alternatively, in the Sulmona basin the end of GI24 (or rather its

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isotope expression) corresponds precisely to the insolation maxima/precession minima (Regattieri et al. 2015). This is very similar to that indicated by TIC data for the DEEP core in Lake Ohrid for the subsequent insolation maxima at ca. 80 ka. Without doubt, insolation is the general pace-maker of glacial to interglacial alternation, but we would like to point out that seasonality and reorganization of the climatic system can produce leads and lags between different system which advise against simple tuning with insolation, especially when proxy records are examined in detail.

4) No sedimentation rates are discussed, I would also suggest to show them in Fig. 4. How do the sedimentation rates compare between the old and new chronologies?

Sedimentation rates are now included in the new Figure 4 and discussed in more detail.

“Figure 4 also illustrates the change in sedimentation rate in the different age models. It is possible to see that increasing the number of aligning points make the sedimentation rate significantly different, suggesting a faster decrease at the time of the interglacial inception. Sedimentation rate increased again around ca. 120 ka, and then remained stable since ca.105 ka. We note that the Francke et al. (2015; this volume) age model (and most other age models too) are based on the assumption of gradually changing sedimentation rates. This might be true, if studying long sequences and on a low resolution. However, changes in sedimentation rates become more important when examining a sequence at higher resolution. On the long-term scale, and using the chronological tie points of the 9 tephras from the orbital tuning used in the Francke et al. (2015; this volume) age model, relatively constant sedimentation rates are inferred for the DEEP core site record. On closer inspection, however, there might be significant changes, particularly at the MIS6-5e transition, as inferred for the new age model, as it is highly unlikely that a decrease in clastic matter input from the catchment (prevailing during glacials, even if partially compensated by a reduced input of organic matter, and indicated in lithofacies 3 of Francke et al. 2016) is completely, simultaneously and equally compensated by an increase in carbonate precipitation reaching > 80% during the interglacial (MIS 5e peak, Fig. 4). This means that it is highly likely that there

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are significant changes in sedimentation rates, which can only be detected by high resolution studies and by a detailed comparison of different records as indicated in this study".

Specific comments p. 16981, lines 23-24: "the marine isotope signal: infers: " this wording is odd because the isotope signal itself cannot infer something. Rephrase to e.g. "The marine isotope signal has been used to infer. . .. . ."

Done.

p. 16982, l. 6: "Woolbreak" – I guess this should read "Waelbroeck"?

Yes, done.

p. 16982, l. 9-10. "could indicate different processes" is quite a vague statement. Marine and terrestrial proxies naturally report different processes. Please specify what is meant here.

We have rephrased this sentence to make it clearer, this is also a comment of reviewer #2. The sentence now reads:

"However, when marine records are used for tuning terrestrial archives there is an implicit assumption of synchronicity between climatic events recognized in marine proxies and those in terrestrial archives often identified using different proxies. Under scrutiny such a relationship may not be sustainable, as terrestrial and marine proxies could indicate different processes at local and global scales with different responses to climatic forcing."

p. 16982, l. 25-27: a reference to (Hodell et al., 2013) might be useful. This citation refers to the speleotheme-based tuning of Iberian Margin Sites MD01-2443/2444 via the synthetic Greenland ice core by (Barker et al., 2011). As suggested earlier, the comparison to the pollen records from the Iberian Margin might be of use for this study as well. We have added this reference. p. 16991, l. 14: there are no 95% confidence limits given in Fig. 4

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We have corrected this sentence. p. 16992, l. 18: it should read “that the Francke. . .” (add “the”)

Done. Table 1: What do the number in brackets denote in the column with the new age control points? If this is the 95% confidence interval, please note if this is 1 or 2 sigma.

We have corrected Table 1.

Fig. 1: Why is Lago Grande di Monticchio shown if no data is presented from this location (although I encourage inclusion of the Monticchio data)?

The new Figure 5 includes Monticchio data. Figure 1 includes the Tenaghi data.

Figs. 2 + 3: Both figures are too small, the text is hardly readable in print-out.

We have enlarged the text in both figures.

Fig. 3: the y axis for the Corchia Cave does only reach to -3 ‰ it does not cover the full range of values.

We have corrected the axis on both figures.

Fig. 3 Caption: Add a “:” after “From the bottom”. Please write all species names in italics. Pre-last sentence: correct “Ohrid” to “Ochrid”

We made the first two corrections. The official name is Lake Ohrid.

Fig. 4: It might be useful to plot the other target records used for tuning here as well in order to judge how well the new stratigraphy fits to the other records (especially the Popoli section, Corchia, possibly Tenaghi Philippon, Monticchio)

In Figure 4 we have now included the sedimentation rates based on the new age model of Franke et al.(2016; this volume) the old age model of Francke et al. (2015; BGD), and that obtained from our age modelling. Moreover, we have plotted the TIC data alongside the different age models and insolation. As an external proxy we also show the proposed SST from Martrat et al. (2014) on the Marino et al. (2015) chronology.

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The comparison with pollen is now given in the new Figure 5 and discussed in the text. Figure 4 is now complete without adding further proxies.

References: Barker, S., Knorr, G., Edwards, R.L., Parrenin, F., Putnam, A.E., Skinner, L.C., Wolff, E. and Ziegler, M., 2011. 800,000 Years of Abrupt Climate Variability. Science, 334(6054): 347-351. Brauer, A., Allen, J.R.M., Mingram, J., Dulski, P., Wulf, S. and HuntleyyâËŸAaËŻ, B., 2007. Evidence for last interglacial chronology and environmental change from Southern Europe. Proceedings of the National Academy of Science of the USA, 104(2): 450-455. Hodell, D., Crowhurst, S., Skinner, L., Tzedakis, P.C., Margari, V., Channell, J.E., Kamenov, G., Maclachlan, S. and Rothwell, G., 2013. Response of Iberian Margin sediments to orbital and suborbital forcing over the past 420 ka. Paleoceanography, 28(1): 185-199. Milner, A.M., Collier, R.E.L., Roucoux, K.H., Müller, U.C., Pross, J., Kalaitzidis, S., Christanis, K. and Tzedakis, P.C., 2012. Enhanced seasonality of precipitation in the Mediterranean during the early part of the Last Interglacial. Geology, 40(10): 919-922. Milner, A.M., Müller, U.C., Roucoux, K.H., Collier, R.E.L., Pross, J., Kalaitzidis, S., Christanis, K. and Tzedakis, P.C., 2013. Environmental variability during the Last Interglacial: A new high-resolution pollen record from Tenaghi Philippon, Greece. Journal of Quaternary Science, 28: 113-117. Pross, J., Koutsodendris, A., Christanis, K., Fischer, T., Fletcher, W.J., Hardiman, M., Kalaitzidis, S., Knipping, M., Kotthoff, U., Milner, A.M., Müller, U.C., Schmiedl, G., Siavalas, G., Tzedakis, P.C. and Wulf, A.S., 2015. The 1.35-Ma-long terrestrial climate archive of Tenaghi Philippon, northeastern Greece: Evolution, exploration, and perspectives for future research. Newsletter on Stratigraphy, 48(3): 253-276. Sánchez-Goñi, M., Eynaud, F., Turon, J. and Shackleton, N., 1999. High resolution palynological record off the Iberian margin: direct land-sea correlation for the LastInterglacial complex. Earth and Planetary Science Letters, 171(1): 123-137.

Anonymous Reviewer #2

General comments: The question how the age models for records for the interval preceding, during and after the MIS 5 can be generated is a very interesting topic, par-

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ticularly for the terrestrial realm. This is a generally well-written manuscript which discusses different approaches for age models for a very important terrestrial site from the central Mediterranean region. I thus think it is worth publishing. What I immediately wondered when reading the manuscript was why there is no comparison with the Tenaghi Philippon record and other important records from the region (e.g. Monticchio) – as I have just seen, reviewer 1 takes a similar view and mentions recent publications in this context. Generally, while introduced in the method section, the DEEP site pollen record is then only mentioned once again at the beginning of the "Results and discussion" section and shortly in the conclusions – I think the authors waste potential here (see also remark to conclusions), and I would have expected in the discussion the implications of the new chronology for vegetation development in the Mediterranean region.

This is a similar comment to reviewer#1 and has been addressed (see above).

In the last figure of the manuscript, the authors show how the newly introduced age model shifts the DEEP total inorganic carbon curve and compare this with the ODP-975 SST record (Martrat et al., 2014 using Marino et al., 2015; see below). Such a figure I would have expected for interesting terrestrial proxies from, e.g., Monticchio, Tenaghi Philippon and Lake Ohrid. This is the more important since Sadori et al. (2015) use the medium-resolution pollen record from Wijmstra (1969) for comparison between Lake Ohrid and Tenaghi Philippon, while there are now records in higher resolution available as mentioned by reviewer 1.

As requested by reviewer#1 we have added a new figure and the discussion has been expanded.

Concerning language: I am not a native speaker myself, but I am sure that the English could be slightly improved. What I particularly noted is the frequent unnecessary use of the word 'however' in the "Results and discussion" section (see below for detailed remarks).

Two co-authors are native English speakers, they have now improved the language throughout the text.

Detailed remarks:

1 Introduction Page 16982, Lines 2 and following: The whole paragraph is difficult to understand. Of course, one can guess what the authors mean, but it is imprecisely stated. For example, “assumption of synchronicity between marine and terrestrial events”: What is regarded as a marine event? An event restricted to the marine realm, or a signal in a marine proxy (which might be caused by an event taking place in the terrestrial realm!), or an event reflected by a terrestrial proxy transported into the marine realm (sediments, pollen etc.)?

We have improved this sentences including also the observation of reviewer#1 (see above)

Page 16983, Line 4: “A large literature:” Not sure if this is grammatically correct, though the expression can be found in other publications.

We have change it thus:

“An increasing number of studies are now devoted to the use of tephra layers for correlation and synchronization of archives (see e.g. Lowe (2011) for an extensive review).”

Line 19 (and later): The term “paper” appears like scientist colloquial language to me. Why not “publication”?

Changed.

4 Results and discussion

Page 16989, Lines 20 and following; and Page 16991, Lines 2 and following: These are sections where “However: : :” is used in two subsequent sentences, and I would suggest to avoid phrases like “: : : we have to note: : :”

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Correction made.

Page 16992, Lines 18 and following: Even though I understand that this is not a high resolution study, I wonder why, if they are discussed, sedimentation rates are not shown in a figure.

The sedimentation rate is now included in Figure 4.

Same line (Page 16992, Lines 18): It is probably a matter of personal taste, but phrases like "As last point, it is important to remember: : :" waste place and are unnecessary. If it was not important, you would not write it, I guess.

Correction made.

5 Conclusions Page 16993, Line 11: While AP % has been introduced in the text, I wondered here if it would be better not to use the abbreviation since arboreal pollen are only mentioned in one other section. Since you claim the concomitance with increasing temperatures here, I wonder if you should not show at least another pollen curve for thermophilous species. The AP % curve could as well be tied to an increase in precipitation.

Pollen data are now discussed . AP was introduced and is a widely accepted term in the International literature.

Line 21: "It is important to remark: : :" Again, a matter of personal taste: Would you remark this if it was not important?

Yes, personal taste, but we have made the change.

Figures: Please increase the size of almost all figures in the final manuscript (perhaps this is just a problem of the upload process?). Apart from the size, I think the figures are generally well-made.

Done.

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Figure 2: What is used as pollen reference sum? Why is only Pinus removed from AP? (In Sadori et al., 2015 it is mentioned that this is due to over-representation of Pinus, but if so, should the reasons for over-representation not as well be immanent for Abies, Picea, and, if occurring, other bisaccate pollen?) Please write “Pinus” in italics. (Ironically, I am not sure how to use italics in the comment form...) The abbreviation “AP” is not explained in the figure text. Even if this is from already published records:

Figures and figure text together should provide all necessary information! (And Sadori et al (2015) is a discussion paper!) I would even consider explaining “TIC” and “TOC” – though I guess everybody interested in the topic knows these abbreviations, it would still be appreciated by readers who do not often work with carbon content.

We have checked all the text in detail to be sure that all the acronyms are correctly quoted at their first mention. The full explanation for pollen data are from Sadori et al. (2016), but are not discussed in this paper.

Figure 3: Change “LC21 planctik” to “LC 21 planktic”; change “Ohird” to “Ohrid”.

Done.

Figure 4: Compare general comments.

Done.

Finally, we hope in the details above we have satisfied all the changes that the reviewers suggested. The final text will include all their comments as discussed. We thank them for their great efforts in improving the manuscript. Attached the two completely new figures and the new table 1 with related captions.

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tuning points		mcd depth	DEEP core age model				This study				Age differences	
			Final AM		Discussion AM		New used age		New modelled age			
			Age (ka)	2σ (ka)	age	2σ (ka)	Age (ka)	2σ (ka)	Age (ka)	2σ (ka)	Final	Discussion
tephra	POP2	40.49	101.8	2.4	101.8	3.2	102.0	2.4	103.6	3	-1.8	-1.8
tuning	end GI24	41.63	104.8	4.2	103.1	3.6	105.4	0.9	105.4	1.8	-0.6	-2.3
tephra	POP4	43.51	109.8	2.0	109.7	2	109	1.5	109.7	2.4	0.1	0
tuning	TII TCU	48.58	127.7	6.6	124.4	2.7	129.6	0.9	129.4	2	-1.7	-5
tephra	P11	49.945	133.0	2.0	129.4	6	133.5	2.0	132.7	2.7	0.3	-3.3

Table 1- Chronological tie points discussed in this study. DEEP core ages and associated 2σ uncertainties are from Francke et al., 2015 (Discussion AM) and Francke et al, 2016 (Final AM) age models.

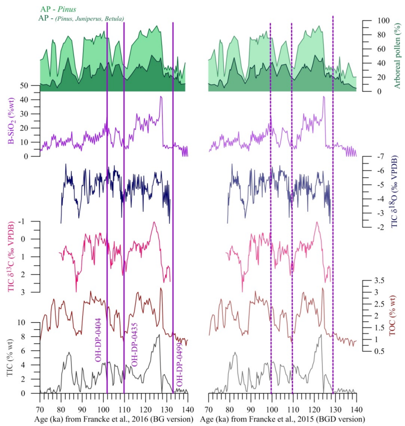

Figure 2- DEEP site proxy series plotted on age models from Francke et al., 2016 (left) and Francke et al. (2015, Discussion version). Purple lines represented tephra layers.

Fig. 1.

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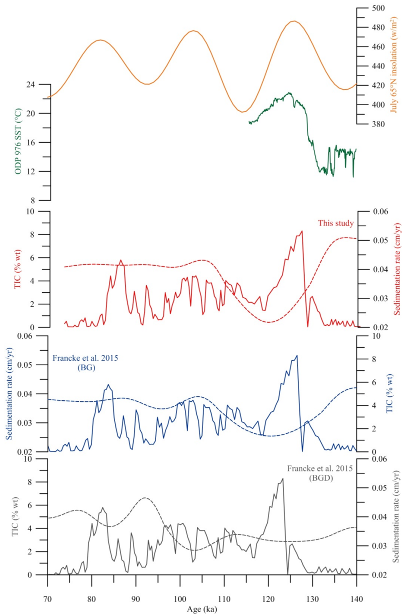

Figure 4- From bottom: TIC (%wt) and sedimentation rate of DEEP site plotted on age models from Francke et al., 2015, Discussion version, grey); Francke et al. (2016, blue); This study (red); Alkenone SST (°C) for core ODP-976 (Marino et al., 2015, green); Summer (July) insolation at 65°N (orange)

Fig. 2.

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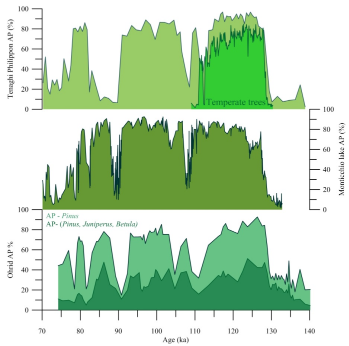

Figure 5- From bottom: DEEP site pollen record (AP- *Pinus* and AP- (*Pinus, Betula and Juniperus*), Sadori et al., 2016) plotted on chronology proposed in this study; Monticchio Lake arboreal pollen (Brauer et al., 2007); Tenaghi Philippon, % of temperate trees from Milner et al. (2012) and total AP from Tzedakis et al., 2006

Fig. 3.

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