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## ***Interactive comment on “Tephrostratigraphy and tephrochronology of lakes Ohrid and Prespa, Balkans” by R. Sulpizio et al.***

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This paper proposes a synthesis between some already published data on tephra layers found in Lake Ohrid with new information derived from new cores from both Ohrid and Prespa lakes. It is a good paper that provides, in a single framework, useful information on the tephrostratigraphic record in the Balkans and makes valuable correlations with volcanic sources in the Mediterranean. For these reasons it deserves the publication on this journal although some minor-moderate revisions are necessary.

I list here some critical issues that, once resolved, should improve clarity and efficacy of the paper:

- Number of studied cores: it's not immediate for a generic reader to understand how

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many cores have been studied here, what are the already published and what are the reviewed data. In the abstract authors mention 4 new cores and 2 reviewed, but in paragraphs 3.1-3.5 they report the description of only 5 cores (Co1200, 1201, 1202, 1205 (???), LZ1120) and in page 3935 line 4-6 they mention again a total of 6 core. Four of this (Co1200, 1201, 1202, LZ1120) are from Ohrid and two (Co1204 and Co1216) from Prespa. In Fig. 2 authors report again 6 cores. Co1205 is missing there but Co1204 and Co1216 (not described in the text) magically appear. This is really puzzling even for a willing reader. In addition it's not clear if lithological descriptions, cited at page 3935 lines 10-12 as already published (for 5 of 6 cores), include also some tephra characterization.

- Composition of tephra layers and alteration: most of tephra attributions and correlations are derived by major elements analyses. Standardless normalized (at 100%) EDS analysis, though accurate (as showed in the Table) , totally overlooks possible effects of alteration and post-emplacement glass hydration. In the manuscript no mention of this important issue is made. SiO<sub>2</sub>, which is used in TAS, is strongly affected by the renormalisation (also +/- 3%), in case of moderate hydration (H<sub>2</sub>O 4-5 %) and alkalis are also mobilized by fluids. Description of “red or pink” tephra layers (e.g. in Co1201) let suppose the possible occurrence of incipient palagonitization processes. Authors should exclude the occurrence of these processes (petrographical observations are not really indicative) before any further use of major elements data for classification or correlation purpose. This can be done performing some EDS or WDS analysis on selected relevant glasses using a different recalculation procedure. Alternatively there are several compositional indexes that help to recognize and to assess incipient alteration processes (see for example a review in the book Gifkins et al 2008 – Altered Volcanic rocks). Another important aspect related to the tephra analysis is : what are they analyzing by ICP-MS ? Glass fraction ? Bulk tephra ? This information is really important for comparisons with proximal archives.

Description of tephra layers: whereas in some descriptions there are a lot of details

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on composition, color and componentry, data on grain size (average not granulometric curve) are reported only for some layers. Authors will agree that grainsize parameter is critical in order to recognize effects of reworking, and should be reported for all recognizable tephra layers.

Correlation to proximal deposits and other distal archives: this is an important section of the paper but after a topic sentence outlining all the critical factors that allow effective correlations, I read three categoric statements in which correlations are made without any critical analysis or supporting arguments. (e.g. “. . . .The youngest volcanic deposit was correlated to the AD 472 (1478 cal. y BP) eruption of Somma-Vesuvius” or “The FL (3370±70 cal. y BP; Coltelli et al., 2000) cryptotephra occurs in cores Lz1120 . . . . .” or “. . .The Mercato (8540±50 cal. y BP; Zanchetta et al., 2010) cryptotephra occurrence is limited to core Co1202 (OT0702-3; Fig. 2; Vogel et al., 2010) from Lake Ohrid, . . . .”) This attributions needs in my opinion of a more thorough discussion. It's only matter of style (and then it's not univocal) and maybe mine is only an philosophic quibble, but I would appreciate along all this chapter, a more conservative approach in which the tephra in the core is related to the eruption products (e.g. . . .OT0702 can represent Mercato distal deposit. . . ) instead of more strong statements in which is said that “. . . .Mercato tephra occur as xxxx layer. . . “. The statement at page 3943 line 25 is in my opinion almost weak: following the same line of reasoning we should find mixed also FL and Mercato layers since their emplacement occurred in a time lapse (1.9 Ky) shorter than that separating Taurano from CI (3-5 Ky) eruptions

Figures: Figures are on the whole clear and of a good quality. Fig.2, which is critical for the correlations between cores, should be improved. My advice is to identify in the figure tephra and cryptotephra with the same code employed in the description of tephra layer (e.g OT0700. . .). This will ease the readability of the description in chapter 3. In the same figure not all the symbols are reported in the captions (e.g. stars). Last but not least : age determination (C14, IRSL, ESR) are original or derived from other papers? In that case please, cite source of data. Moreover they represent the age of

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the sediment or the age of the correlated eruption ? Fig.8 it's not fully readable and correlations are not immediate. I don't really have an easy suggestion to improve this figure.

Tables. Are on the whole informative. I would drop the "Total" column in Table 2 and 4. I'm also wondering if ClO is the correct way to report chlorine concentrations in glasses instead of the Cl as cation. It's also unclear if literature data have been renormalized to 100% in Table 4.

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