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Interactive comment on "First tephrostratigraphic results of the DEEP site record from Lake Ohrid, Macedonia" *by* N. Leicher et al.

Anonymous Referee #1

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It is by now clear from the wide literature of the last decade that the late Pleistocene-Holocene tephrostratigraphic framework of the Mediterranean area-in particular, the central portion- is getting more and more accurate both in terms of proximal-distal and distal-distal correlations. On the contrary, we still have scattered informations from the older deposits and this is mainly due to scarcity of data at the eruptive source and the rare occurrence (or availability, if marine) of long and continuous sedimentary records (>200 ka) where distal products can be preserved. In this context, the paper by Leicher et al. may represent an important contribution for the chronostratigraphic reconstruction of Italian explosive volcanism during the middle Pleistocene because, in addition to the well-known Late Pleistocene-Holocene marker tephras (e.g. Mercato, Y-3, Y-5 and X-6), it provides very original and interesting data concerning the tephrostratigraphic record of the Lake Ohrid (Macedonia) down to MIS 15. The paper in its present state,

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however, has several weak points and blanks which need to be carefully revised.

My main comments concern the discussion section i.e. the data analysis. I have some difficulties in taking into account the proposed correlations mainly for the ancient tephras (prior to X-6). The correlation of a tephra with a volcanic source and/or event cannot be based only on the TAS classification diagram as the authors did. It is known that the sum of alkalis cannot be a diagnostic criterion due to the mobility of the two oxides. Moreover, there is heavy overlap of compositions related to the Italian volcanic rocks. The use of TAS as the only instrument of major element analysis can lead to misinterpretations and ambiguous correlations. I suggest to the authors to perform a more accurate data analysis and to provide significant figures concerning this issue. The ancient tephras analysed here represent the most original aspect of this paper and therefore deserve a more significant approach.

I think that a research paper should provide all the main informations to let the reader follow the discussion and the aimed results in the proper way. This work deals with the correlations of 13 tephras with a number of likely correlatives both at proximal and distal sites. This means a large dataset of major element compositions from literature which the authors should have used in order to make comparison with OH-DP-tephra and to establish the proposed correlations. A table where at least the average composition of all the many tephras, used in the TAS for comparison e cited in the text, must be reported. It is hard for the reader to have such a long text without a reference table.

In detail:

1-It is not clear to me if the cryptotephra correlated with the Mercato event has been analysed in terms of major element content. Actually, there are no data in table 1. Is the correlation merely based on similar tephrostratigraphic features in other cores from Lake Ohrid and Lake Prespa? Please, specify.

2-Please, locate in Fig.1 all the drilling and outcrop sites cited in the text and discussed to establish correlations with OH-DP-tephras. The paper must be easily man-

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aged by anybody interested in the field but not necessarily expert of Mediterranean tephrochronology.

3-Since some of the analysed tephras in this work aim to be good markers beyond Italy and the Balkan region, I suggest to insert a figure where the tephrostratigraphic framework for the area might be sketched. This figure can sum the conclusions of the paper which are too long in the text after all.

4-The authors report in the text a low and high silica end-member for tephra OH-DP-0115/Y-3. This feature should be displayed in table 1 with two average compositions for the layer.

5-Trimodal composition of the CI deposits? I don't see anything of this in your OH-DP-O169/Y-5 tephra.

6-In Fig. 3g the label for the tephra plotted in the TAS diagram is OH-DP-0617 but actually it should be OH-DP-0624 according to the text.

7-The authors report a trachy-andesitic to phonolitic trend for tephra OH-DP-1955. Are you sure it can be considered one population instead of two? Such magmatic trend is very unlikely. Moreover, since Sr-isotope ratios of the correlative SC5 tephra infer an origin for these layers from the Roccamonfina volcano, why do the authors discuss a possible origin from Sabatini vents despite geochemical differences? It is a useless part of the discussion.

8-Concerning tephra OH-DP-2010/Fall A, the authors cite the Tufi Terrosi Eruptive Cycle of the Sabatini volcanic district as the source for the deposit. Please, report the age of this cycle and the reason why they make this correlation.

9-In section 5.14 the authors mention two flux standards: FCs and ACs-2. I suppose the one used to recalculate 40Ar/39Ar ages is the latter one (as it is reported in the caption of table 2). Please, correct the sentence.

10-The exact reference for the TAS diagram is Le Bas et al., 1986 and not Bas et al.,

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1986.

11-Please, note in the Introduction section that the tephrochronological record published for KC01B core in the Ionian Sea (Insinga et al., 2014) extends down to 200 ka, then Middle Pleistocene.

In conclusion, I think that the established correlations proposed in this work are not fully supported by a correct analysis of data. The discussion should be revised and rewritten with an adequate number and types of figures and tables. Two figures (TAS and Al2O3/FeOtot for one tephra) cannot support such a large dataset which includes original data from Lake Ohrid and published data from other sites of the Central Mediterranean area.

According to these considerations, I suggest major revisions.

Interactive comment on Biogeosciences Discuss., 12, 15411, 2015.