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Interactive comment on "Extreme flood events reconstruction during the last century in the El Bibane lagoon (Southeast of Tunisia): A Multi-proxy Approach" *by* A. Affouri et al.

Anonymous Referee #1

Received and published: 10 May 2016

General comments:

This is basically an interesting case study dealing with the tracking of palaeo-flood events in the El Bibane Lagoon (SE Tunisia) during the past century. The main objective of the study is to investigate sediment sources in the lagoon and to discriminate between fluvial, aeolian, marine end-members by using sedimentological and geochemical data. The patterns observed in modern sediments are expected to help deciphering ancient flood events in lagoonal deposits, as preserved in a core covering the past century based on a combined chronology using 210Pb and 137Cs data. I must be honest in saying that, if the study is relatively sound and acceptable, I've not been convinced in general by the novelty of the approach, and have in addition

several reservations regarding the interpretations (see the Specific Remarks). In particular, most of the results have been presented between the Results chapter and the Discussion, which renders the manuscript confusive and difficult to read. Alternatively some parts of the text have been totally overlooked and would benefit from further consideration/exploration before the manuscript can be accepted. The quality of the figures is overall acceptable, albeit some figures are of very poor graphical quality. The manuscript is not really well written, and should absolutely be revised by a native English before further consideration. I also regret that no tentative comparison with other regional datasets is provided in the Discussion, although I am pretty convinced that such a perspective would help to build a bigger picture of palaeoflood activity regionally. Finally, I do not believe that the manuscript provides the sort of conceptual and fundamental advance in our understanding of the processes and mechanisms governing lagoonal sedimentation and past central/southern Mediterranean climate that has been published elsewhere. For these reasons, I would not recommend this study to be published in Climate of the Past. However I leave this decision to the editorial board, who should appreciate the other reviewers' comments and recommendations.

Specific remarks:

1. Introduction :

Page 3, Lines 1-3 : Please provide more information dealing with the study of Raji et al., 2014 in Morocco, and show how the outcome of this work is related to the present study.

Page 3, end of the introduction : I would have appreciated to find here, as a foremost objective of the study, a perspective of data integration with other dataset covering the same time span, at a regional/larger scale.

3. Climate and hydrology

Page 5, lines 10-12: Please check the phrasing of that sentence. This is a regular

problem throughout the manuscript, which would highly benefit from a thorough cross-reading by a native English.

Page 5, lines 13-16 : When you refer to Figure 3, please also introduce the Medenine and Tataouine watersheds here (and not later at the beginning of chapter 4). There is a mistake with the spelling of Medenine on Figure 2.

4. Materials and Methods

Page 6, lines 11-13 : Please provide a general lithological description of core BL12-10, since we are not provided with any information with respect to the sedimentology at that stage.

In general the methods are described in an extremely concise way, and would perhaps merit more devotion. The information provided in the present version of the manuscript are indeed very limited (XRF, grain size analysis and age model using 210Pb and 137Cs). Why did you opt for a 1cm-resolution (only) with the XRF data, and not a higher resolution ? Is the sediment too homogeneous, thus rendering this perspective not promising ? Please elaborate on that.

Page 7, lines 11-12 : Please rephrase as I do not understand this sentence.

Page 7, lines 11-13 : I find this introduction for the PCA analyses far too simple ! Could you elaborate more on that ? For instance, since you use percentage values (both for grain-size and XRF data), have the raw data been square-root transformed, centred and standardized before applying the PCA analysis ? This is of great importance regarding the reliability of the results. Please clarify it.

5. Results

Page 8, lines 2-4 : On Fig. 6 the distribution of grain sizes appear different and more complex between S7 and S10 (fluvial end-member). For instance, the mode at 100μ is not present on sample S10. Similarly, the mode 20-63 μ is not really obvious in S10. Is the pattern so tricky when considering other samples from the fluvial component (e.g.,

S8, S9, S1-S16) ? Please comment on that and eventually show more plots for the fluvial components.

What about the 4th group, i.e., the lagoonal samples ? It is neither presented so far in the text, nor shown on Figure 5 (although it does on Fig. 6, interestingly). The distribution looks rather complex for this fraction in Fig. 6, and obviously show a mixture between the different modal distribution (with at least a great contribution of fluvial samples).

Page 8, lines 15-24 : Here we are provided with XRF data given as percentage values. Please explain how these values have been obtained. Have the semi-quantitative XRF core-scanning data been calibrated by discrete XRF measurements as to determine linear regressions in cross plots and calculate percentage values from scanner data ? Please clarify on that, and above all, please show the raw XRF data (in cps) obtained on core BL12-10.

Another issue : Taken into account the very low ranges of variations (0-1,5% for Fe ; 0-0,2% for Ti), how can you be confident with the interpretations (i.e., the discriminations into different environmental pools) ?

Page 9, line 5 : Please change Fig. 7 into Fig. 8.

Page 9, lines 18-19 : Please rephrase here, a verb is missing.

Page 10, lines 5-6 : Do you mean mud or clay layers ? Mud is usually enriched in organic matter, whereas clayey sediments are not. What do you mean by mud layers typically composed of clay and silt sediments ? By the way, there is no mud shown on Figure 9 ;

Page 10, lines 6-7 : Where do these flood layers appear on Fig. 9 ? How did you identify it ? I regret that the quality of Fig. 9 is so poor ! Please redraw this figure accordingly.

Page 10, lines 16-18 : I do not understand this sentence ! It provides a very simplis-

tic explanation for the discrepancies observed between 210Pb and 137Cs data. Did you also measure 241Am throughout core BL12-10, which would help in solving this apparent mismatch?

6. Discussion

Page 11, lines 20-23 and thereafter: I do not understand why the Discussion chapter still contains results/interpretation. The outcome of the PCA analysis should definitely be treated in the Results or Results/Interpretations chapter, but not in the Discussion !! Please modify this accordingly. The Discussion should be the locus where the results are integrated regionally, and at a larger scale, regarding the main scientific question identified in the introduction. Here we are provided with results in the Results chapter, followed by results in the Discussion chapter. See also my comment Page 7, lines 11-13.

Page 11, line 4 : I do not understand why this reference is suitable and of interest here. Please check this and correct accordingly.

Page 11, lines 13-15 : I do not agree that Fe and K (at least) showing negative loadings on Factor 2 !!

Overall, I am not convinced by the application of a PCA analysis here to discriminate between different sources. Please explain why the PCA analysis brings compelling useful evidence for the interpretation of environmental proxies.

Page 12, lines 3-9 : Are these results really unexpected ? What do we learn here ? Do we really need geochemical proxies, grain-size data and PCA analyses to show that lagoonal sediments are made of a mixture of continental and marine sources ? Why this still is presented in the Discussion ??

Page 12, lines 12-15 : Looking at the data, it is not really obvious that one could define genuine palaeoflood events. How do you discriminate between a background fluvial influence within the lagoon and genuine palaeoenvironmental disruptions (e.g.,

exceptional flood events recorded in the sediments = eventites) ? Is there a threshold to be considered in the data ?

Page 13, lines 17-19 : May this alternative explanation account for the apparent discrepancies observed between 210Pb and 137Cs data ? Apart from that, if the BL12-10 core consists of a background sedimentation disrupted by occurrences of flood events during the past century, it should definitely be taken into consideration when calculating average sedimentation rates. Did the FL1, FL2 and FL3 flood layers excluded for the estimation of sedimentation rates ? If not, this has to be commented.

Moreover, if the FL2 layer represents more than one flood deposit (e.g., 3 floods events as suggested), why do all sedimentological proxies (i.e., Fe/Ca and Ti/ca, clay+silt fractions) peak only once in FL2 ? What about the 1984 flood recorded in the Tataouine watershed as shown on Figure 3 ? May this correspond to the peaks observed in Fe/Ca and Ti/Ca at the lower end of FL1 (around 10 cm) ?

References :

Many references are listed in the reference list but do not occur in the text. There are listed here below, but please check the reference list in general.

Beker (1989) is missing in the text. Guelorget et al. (1982) is missing in the text. Plewa et al. (2012) is missing in the text. Prospero et al. (1981) is missing in the text. Raji (2014) is missing in the text. Richter et al. (2006) is missing in the text. Torres-Padron et al. (2002) is missing in the text.

Figures :

Figures 1, 2, 3, 4, 5, 7, 8 are of good visual and graphical quality in general. In contrast, Figures 6, 9, 10 and 11 are of poor quality (Fig. 12 acceptable) and should definitely be improved before the manuscript can be reconsidered.