

Interactive comment on “Paleotsunami deposits along the coast of Egypt correlate with historical earthquake records of eastern Mediterranean” by Asem Salama et al.

P. Costa (Referee)

ppcosta@fc.ul.pt

Received and published: 26 March 2018

Dear authors, the manuscript by Salama et al., focus on the identification of paleotsunami deposits in the northern coast of Egypt and attempt to correlate "event layers" with historical tsunamis that affected the eastern Mediterranean.

This work could be of interest to improve our knowledge of tsunami risks for this region and also to provide a useful physical validation of past historical events. Scientifically this manuscript addresses a very relevant subject.

I also would like to acknowledge the authors for their extensive fieldwork and impressive

C1

list of radiocarbon dates obtained. However, I have several major concerns regarding this manuscript. I think there are several (major) aspects that need to be addressed before this paper is considered for publication. I regret to say but as it is this paper is not acceptable to be published by Natural Hazards and Earth System Sciences.

In my opinion the authors need to: 1- Restructure the manuscript. As it is results, discussion and conclusions are confusing. There are several paragraphs of results that need to be moved to discussion. 2- The authors try to guide the reader. That is wrong. From an early part they assume the "event layers" are tsunami deposits. They should let the reader get to that conclusion and I think it is wrong to state the layers are associated with a tsunami event in the results. You should only do that in the Discussion. 3- The authors are not convincing explaining the poor dating chronology established. I accept you could have dates in reverse order in the deposits (incorporation of older material). However, that should not happen in the immediately overlying and underlying layers. These should be in chronological order...and they are not. 4- There is poor quantification of data in this manuscript. For instance in Figure 5, one cannot understand what was the resolution used. How many samples have you analyzed? On another topic you mention Pyrite on the Discussion has being widespread in the deposit when in fact it only appears in Core 7. 5- The literature review is extremely poor and outdated. Introduction needs to be totally rewritten. There is a insignificant number of papers published after 2010. In particular, after the Tohoku-oki tsunami in 2011, a relevant number of papers were published moving forward this field of science. They should have been referred to. 6- The authors identify "event layers" based in a very limited number of lithostratigraphic evidences and none (or even all together) are sufficient to ascribe a layer as a tsunami deposit. They need to address this! 7- Furthermore, there are several paradoxes like relying on (volume) magnetic susceptibility to identify the layers as tsunami-related. For example, if you have coarser material it is likely you could have more lithic material and more magnetic minerals. However, you mention on lines 566 and 567 that your magnetic susceptibility peaks correspond with the higher values of organic matter and carbonates. This is something difficult to

C2

explain because organic matter and carbonates have very low magnetic susceptibility values. 8- the manuscript needs proofreading. There are several mistakes and misspellings and the work will benefit from the input of an English native speaker. There are parts that are just too wordy and redundant. 9 - In the figures, and also elsewhere, you need to level the coring to m above mean sea level. You make correlations on Figure 7 assuming the samples are all at the same height above msl. That is wrong. 10 - You need to provide the regional wave regime. How frequent are storms? Can they over-top the 2m high coastal dunes?

There are other aspects that need to be corrected: Line 82-84 - How about Storegga? Landslides tsunamis can cause widespread effects. Line 83 - "recent example" Tinti et al. (2005) has 13 years. Line 85-96 - extremely poor literature review. Why do you cite two papers from the Indian Ocean and the Pacific and only one from the Mediterranean? Line 97 and Line 105 - repetition of idea in the same paragraph. Line 108- Tsunami catalogue of Egypt - is there a specific reference? where can we access it? Line 115-119- Please rewrite. Line 124 - Please remove "in". Line 125 - Please write "Rhodes". Line 126-128 - Please rewrite. Line 130-132 - Repetition of 1st sentence of the paragraph. Line 136-150 - Please rewrite, simplifying the text. Line 169 - Suggest you replace "seawater outlets" by inlet or barrier. Line 169 - Please replace "designated" by "likely sites to preserve past tsunami deposits". Line 178-179 - is a challenge everywhere. Line 180 - Please add a more recent reference. Line 185 - Please correct reference. Line 185-195 - needs to be rewritten and to be reorganized to clearly state which are the common tsunami deposit features. There are many missing. Please check papers by Chagué et al. (2011; 2012), etc. Line 212 - Please change here and elsewhere in the results chapter reference to "tsunami deposits". Change it to "event layer". Line 225 - You should cite Folk and Ward (1957) for grain-size distributions (line 225). Line 238 -Please change the name of this section to results. Line 254-257 - Please pass it to the discussion. Line 261 - Change it to "event layer". Line 273 - here the deposit is 30-73 cm in all trenches P1 to P4 but on line 250 is just from 30-50 cm! Page 11-page 19 - all this results section deserves the following comments: a) In P2

C3

you assume to have >5000 years sedimentation in 27 cm. How come the top 70cm is just app. 2000 years? What changed? How do you explain this difference? How about sea-level changes, how do they constrain sedimentation rates in these lagoons? b) I acknowledge and appreciate that you assume the shortcoming of the dating obtained but how come not a single date in several cores are in stratigraphical order? Again, if it was just the event layers...you just get samples in the right order in the under and overlying layers. You need to offer a convincing explanation for this fact. Just saying that this was due to reworking by the "tsunami" is not enough. c) how come (on line 275) you state "related chronology are comparable in all trenches" when you assume dates have such a wide range? You need to support this sentence with clear data correlation. d) *Dendropoma* shell and its dating. What species was dated. There are some *Dendropoma* species that live beyond 50 m below msl. If these boulders were transported inland and the shells are well-preserved they had to have been transported in suspension (if they were dragged or rolled the shells would break). You state they were dragged on line 286. Can you try to explain this more consistently? Line- 269-270 - Should be moved to Discussion Line 281-288 - Should be moved to Discussion. Line 300 - the layer had brown clay sediments or consisted of brown clay sediments? The poor sorting was measured how (visually or after grain-size analysis)? What were the main components of these populations (Shells, quartz and clay material)? Line 303 - please replace "extremely bad sorting" by "very poor sorting". Line 305 - please replace "bad" sorting by "poor sorting". Line 307 - "some turbiditic structures". Which ones? Be clear and specific about which sedimentary structures you are describing. Line 310-318 - Should be moved to Discussion Line 328- articulated shell? Line 337 - "Organic matter >2" in which unit is this expressed? % of dry weight? % of total sediment fraction? Line 349-352 - Discussion and again repeating the same explanation. Line 356 - well, could be the limit of tsunami coarse deposition. Not the inundation limit. Only with geochemistry you will be able to establish more accurately the likely limit of inundation. Line 383 - the date range obtained is almost 1000 years! You need to constrain the ages much better and more accurately. Line 384-391 and elsewhere

C4

- why are these layers considered to be tsunami related? You mention on lines 460-462 that these "tsunami" layers have been identified based in "photography and x-rays, magnetic susceptibility, organic/mineral content and by the existence of mixed coarse and fine sand with broken marine shells". This is poor and insufficient. You need to provide more data and go through a vast list of sedimentological criteria before you rush to conclusions. See papers by Chagué et al., 2011 and 2012, Costa et al., 2012 and 2016, etc. for comparison. Line 466-477 - Please move it to Discussion. Line 489-490 - Another crucial topic. Why you say they are more likely to be a tsunami than a storm? Have you detected any storm layers? But you state they are more frequent and they are likely to over-top the dune field. Line 495 - You only mentioned Pyrite on core 7 and now.... Heavy minerals? Which ones? Did you counted them? Please provide quantitative data. Line 500 - pebbles and loading structure- please clarify text. Line 506- You wrongly cite Folk (1968) and state he mention ">5" mark for organic matter in tsunami deposits?!? Line 508-522 - this paragraphs belongs in the discussion. Line 525-527 - Please rewrite this sentence. Line 534-536 - sentence not supported by the data presented. Line 538-540 - Do storm layers exist? If no, why? If yes, please compare them with your "event layers". Line 545-547 - a bimodal curve only represents two likely sediment sources. Please update references and clarify idea. Line 549 - "consistent depth". Well, below surface yes but you need to provide height above mean sea level to make this correlation credible. Line 557 - You have a lack of radiocarbon dates between the Younger Dryas and Holocene sea-level stabilization. Is there a scientific justification for this fact? Or a methodological one? Line 559-561 - Strongly disagree. You have not proven this point. Line 565 - "chemical characteristics". You could also provide geochemical data. Which elements have you measured?

I hope you understand my review from a constructive perspective. It is with regret that I say the authors jumped to conclusions and need to address the items above before the manuscript is published. Regards Pedro J.M. Costa

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-C5>

2018-62, 2018.