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Natural Hazards and Earth System Sciences
Discussions

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

Asem Salama et al.

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Please find attached the reply to RC2 (P. Costa) on our manuscript nhess-2018-62 titled "Paleotsunami deposits along the coast of Egypt correlate with historical earthquake records of eastern Mediterranean.

We are grateful to the referee that helped us to clarify our text and figures and improve the presentation of our article. All comments, remarks and questions of the referee are addressed in our revised version (see underlined sections in article) and a detailed answer has been prepared in order to clarify the article.

RC2 (P. Costa) refer to several major issues of the manuscripts and we have addressed each one of them in detail. Although the referee made numerous harsh remarks with rather severe conclusions regarding our data analysis and interpretations, we have found no difficulty in addressing his questions. The majority of his ten main comments are similar to those of RC1, e.g., moving sentences and some paragraphs to discussion, and using high energy sedimentary deposits instead of tsunami deposits (see table in separate sheet). RC1 found our list of references poor and outdated and we have added 16 new references (see underlined) and among them 8 are post-2010. RC2 apparently does not appreciate our data analysis and interpretations but we have addressed all his issues in order to clarify the correlation we make with historical earthquake tsunamis in the East Mediterranean.

All answers to comments and remarks of RC2 are here below:

Section 1: General comments:

Comment #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. Reply The authors agree with the reviewer suggestion of moving some paragraphs of results to discussion. Indeed, some sections of text needed to be transferred and the current structure of manuscript is now more consistent with the aim of the paper.

Comment #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. Reply The reviewer made a point here and we have removed the mention on the tsunami deposits and replaced it with high energy sedimentary layers all manuscript except in the discussion section (see all changes as in following table):

Previous line Previous mention New mention New line 97 Paleotsuanmi high energy sedimentary 109 235 tsunami high energy sedimentary 255

261 tsunami high energy sedimentary 278

298 tsunami high energy sedimentary 306

307 tsunami high energy sedimentary 316 320 tsunami high energy sedimentary 324

320 tsunami omit 325 327 tsunami high energy sedimentary 332 329 tsunami high energy sedimentary 334 333 tsunami high energy sedimentary 338 333, 337 tsunami omit 340,344 341 tsunami high energy sedimentary 348 344 tsunami high energy sedimentary 352

345 tsunami omit 353

346 tsunami omit 355

350 tsunami high energy sedimentary 358

355 tsunami high energy sedimentary 363

357 tsunami high energy sedimentary 365

359 tsunami high energy sedimentary 367 359 tsunami omit 367 361 tsunami omit 369 363 tsunami omit 371 369 tsunami high energy sedimentary 377

370 tsunami high energy sedimentary 378

372 transport of tsunamis omit 380 374 tsunami high energy sedimentary 382

375, 378, 379 tsunami omit 382, 386,387 383 tsunami high energy sedimentary 391 384 tsunami high energy sedimentary 392 385 tsunami omit 393 389 tsunami omit 397 392 tsunami high energy sedimentary 400 393 tsunami omit 401 397 tsunami omit 405 401 tsunami high energy sedimentary 409 402 tsunami high energy sedimentary 411 405 tsunami high energy sedimentary 412 406 tsunami omit 413 410 tsunami omit 417 414 tsunami high energy sedimentary 420 415 tsunami high energy sedimentary 422 418 tsunami high energy sedimentary 425 419 tsunami omit 426 423 tsunami omit 430

429 tsunami high energy sedimentary 436 434 tsunami high energy sedimentary 440 435 tsunami high energy sedimentary 441 443 tsunami high energy sedimentary 450 454 tsunami omit 461 460 tsunami high energy sedimentary 467

Comment #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. Reply The reviewer discusses the reworked sedimentation and reverse order of dating. However, one has to pay attention from the field work in trenches and cores that samples are not easy to find and to collect, especially before and after the presumable tsunami layer. The constraint of past tsunami chronology is based on 5 samples in 1 meter stratigraphic section at Kefr Saber, and 8 samples in 2 meters of sediments at El Alamein site. Taking into account the difficulty and effort made in collecting valuable samples for dating, we disagree with the reviewer that our results is based on “poor dating chronology”. In presenting the 46 samples including reverse dating order, our work shows the difficulty of sampling and dating in such environment (with sometimes recrystallization and/or remineralization, contamination). Clearly, we are not in the ideal case-study of collected samples showing a straightforward chronological and stratigraphic order in such coastal environment. In our case, we found it interesting that all obtained dating are presented together with the reworking difficulty that is openly discussed in lines 578-588. We also show how to separate the dated materials in groups and how with our processed data the dating analysis becomes consistent with the historical earthquake tsunami catalogue.

Comment #4: A - There is poor quantification of data in this manuscript Reply In our manuscript we have analyzed 120 samples as following: 1- Grain size analysis (mean grain size and sorting) 2- Bulk mineralogy (XRD diffractions) 3- Total organic and inorganic matter, in addition of a- Detail descriptions (color , microfossil content) b- High-resolution of photograph of sedimentary sections c- X-ray scanning of cores d- Microfossils identification 4- Radiocarbon dating of 46 samples at two sites, and 5- Geochemical analysis in the Suppl. Material (Table S1 to S12 and Figs. S4-S15).

We do not think that this can be called “poor quantification of data”.

B - 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. Reply We have added details in lines 113-116 to explain that our sampling rate was 15 cm in each core for geochemical analysis, and every 3 cm for the magnetic susceptibility. As for the Pyrite and/or geothite, they are found with minor percent (less than in most of cores with relative high value at the base of event layer (557-563).

Comment #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. Reply Perhaps the reviewer did not find enough references of paleotsunami studies in the East Mediterranean, this is unfortunate but it is the reality. Although we disagree with the qualification of “outdated literature” (much of our references concern reports on past earthquakes and tsunamis in historical documents), and the aim of our manuscript is not meant to do a review on the 2004 Sumatra and 2011 Tohoku earthquake tsunamis. Nevertheless, we have added 16 new references (see underlined) and among them 8 are post-2010. Although we find the request of a total rewrite of the introduction somehow excessive, we have included some changes. We have been submitting papers for publication in the past 30 years or so, and our introductions were generally considered as well written.

Comment #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! Reply Our criteria to recog-

nize signatures of tsunami event layers (see also section 4) are also those reported in Donato et al. (2008), Font et al. (2010), Chagué-Goff, et al. (2011), De Martini et al. (2012; with our direct observations of tsunami layers during field investigations of our colleagues in Sicily), Malik et al. (2015), Matsumoto et al. (2016) along with our post-earthquake tsunami deposit observations (mainly in coastal Honshu following the 2011 earthquake). Beside the trenching and coring analysis of section 5, we summarize in section 6 (lines 478 to 521) our results based on detailed description of sedimentary successions that include units rich in organic matter with bioclasts, laminations, where X-rays, magnetic susceptibility, and determination of heavy minerals with radiocarbon dating of 46 samples are applied. The identification of four high energy sedimentary layer with the discovery of the similar mixed white sand sheet layers with broken shells at two sites (Kefer Saber and El Alamein), located $\sim$200 km apart, and their dating with correlation of three of them with past tsunamigenic earthquakes is a striking evidence of tsunami deposits. This is extensively addressed in sections 4, 5 and 6 of our manuscript and cannot be considered as limited evidences.

Comment #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 explain because organic matter and carbonates have very low magnetic susceptibility values. Reply We clarify this relevant issue in text-lines 557-563. The low magnetic susceptibility peaks reflect high content of organic matter and carbonates and these analytic results characterize the tsunami related deposits. However, in some cases minerals like pyrite or Fe oxides (goethite) in sediments are found in the bottom of tsunami layers (or intercalated) and they correspond to relatively higher peak of magnetic susceptibility (20-100 10-6).

Comment #8 The manuscript needs proofreading. There are several mistakes and misspellings and the work will benefit from the input of a English native speaker. There are parts that are just to wordy and redundant. Reply The new version of manuscript is revised for the English syntax and grammar.

Comment #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. Reply This is corrected and updated (see lines 982-983).

Comment #10 You need to provide the regional wave regime. How frequent are storms? Can they over-top the 2m high coastal dunes? Reply In the Mediterranean, the tropical to subtropical cyclones storms are frequent seasonal events, with ∼100 recorded tropical like storms between 1947 and 2011. From tide stations recorded in front of Alexandria, the maximum wave height surge is 43 cm between 1971-2004 (Hamed et al., 1988), the maximum wave height surge is 76.9 cm between 1996-2000 (Hussein et al., 2010). See also supplementary material. The comparison between storm and tsunami depends on the strong waves and their content of reworked deposits, fossils or organic matter and the sorting of grain size. Tsunami deposits tend to show much sorting and contain much bioclasts due to its powerful waves.

Section 2: Comments in the text with requested changes in manuscript and authors changes in text:

C1: Line 82-84 - How about Storegga? Landslides tsunamis can cause widespread effects. R1: We have added explanations in lines 82-84

C2: Line 83 - "recent example" Tinti et al. (2005) has 13 years. R2: Corrected

C3: 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? R3:We add Tyuleneva et al., 2017 as a second example in the Mediterranean (lines 106-108).

C4: Line 97 and Line 105 - repetition of idea in the same paragraph R4: We removed

lines 118-119.

C5: Line 108-Tsunami catalogue of Egypt - is there a specific reference? where can we access it? R5: Ambraseys et al. (2005) is the specific reference for Egypt (added in line 121)

C6:Line 115-119- Please rewrite. R6: We have rewritten in lines 128 to 133.

C7:Line 124 - Please remove "in". R7: Removed

C8: Line 125 - Please write "Rhodes" R8: Rhodes is rewritten (line 138).

C9:Line 126-128 - Please rewrite R9: The sentence is rewritten in lines 147-150.

C10:Line 130-132 - Repetition of 1st sentence of the paragraph R10: The repeated sentence is removed

C11:Line 136-150 - Please rewrite, simplifying the text. R11:151 to 160 updated with simplified text

C12: Line 169 Please replace "designated" by "likely sites to preserve past tsunami deposits". R12: Done in line 179

C13:Line 178-179 - is a challenge everywhere. R13: Yes sure, but here the problem is in the Eastern Mediterranean region

C14:Line 180 - Please add a more recent reference R14: We add Morton et al., 2007 (updated lines 191)

C15:Line 185 - Please correct reference. R15: Corrected at line 196

C16: Line 185-200 - 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. R16: The paragraph is rewritten to point out common features of tsunami deposits. We also add Chagué et al. (2011) in line 208.

C17: Line 212 - Please change here and elsewhere in the results chapter reference to "tsunami deposits". Change it to "event layer". R17: Except in discussion and conclusions, the “tsunami” term is changed in “high energy sedimentary layer” (see also table in above reply to comment #2).

C18: Line 225 - You should cite Folk and Ward (1957) for grain-size distributions (line 225). R18 & R19: Done in updated line 238

C20: Line 238 -Please change the name of this section to results. R20: In order to be more specific, we prefer the title Description of sedimentary layers in trenches and cores with C14 dating results, updated in lines 258.

C21: Line 254-257 -Please pass it to the discussion. R21: The change is in discussion section (lines 553- 555)

C22: Line 261 - Change it to "event layer". R22: We used instead high energy sedimentary layer in line 278

C23: 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! R23: This is corrected in line 290 with 25-55 cm depth

C24:Page 11-page 19 - all this results section deserves the following comments: a) In P2 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 constrained sedimentation rates in these lagoons? We answer the question in lines 578-588. The lagoon sedimentary environment is often made of mixed and reworked marine and continental deposits. The interpretation of these deposits as a chronological order is problematic. Sea level change is negligible in the late Holocene time [see also Fleming, K. et al. Refining the eustatic sea-level curve since the Last Glacial Maximum using far-and intermediate-field sites. Earth Planet. Sci. Lett. 163, 327-342 (1998)].

b) I acknowledge and appreciate that you assume the shortcoming of the dating ob-

tained 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. We provide explanations in lines 578 to 588 for the overlapping dates and also in our reply above for comment #3.

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. Regardless of the reworked deposits, we consider the stratigraphic succession of neighboring trenches (P1 to P4) at Kefr Saber, and their relative sedimentary chronology of units deposited in the same lagoon as comparable. In order to overlook the old ages due to reworked deposits, we select radiocarbon dates younger than 2000 year BP and obtain a consistent chronological succession. (see changes in lines 292-294).

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 had 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? The common species type found in boulders is Dendropoma Petraeum and Vermetus Triquetrus. The boulder surfaces are fully submerged in the sea with Dendropoma species and then transported by tsunami waves or storms waves. Some Dendropoma and Vermetus are stuck on the boulder and hence well preserved.

C25: Line 269-270 should be moved to Discussion R25: Moved in discussion section (lines 579 to 581)

C26: Line 281-288 should be moved to Discussion R26: Moved to lines 519 to 528 (section 6)

C 27: Line 300 the layer had brown clay sediments or consisted of brow 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)?

R27: It consists of brown clay sediments (see line 300 – 301). These methodological details are added in the supplementary material (methodology section).

The main values of each layer are given in the core figures according to the detailed description of layers and the bulk minerology (including weight percent of minerals, Tables S1 to S12 and Figs. S4 to S15 in the supplementary material).

C28: Line 303 - please replace "extremely bad sorting" by "very poor sorting". R28: Done in line 311

C29: Line 305 – please replace "bad" sorting by "poor sorting". R29: Done in line 313

C30: Line 307 - "some turbiditic structures". Which ones? Be clear and specific about which sedimentary structures you are describing R30: We mean by turbiditic structures like rip clasts, cross bedding and laminations (line 307). X-ray scanning show vertically and horizontally oriented gastropods seen in cores before opening and cut in two longitudinal half. These structures are used to identify the tsunami deposits The mention to the turbiditic features is added in the supplementary material (Fig S3).

C31: Line 310-318 should moved to Discussions R31: We have omitted these lines because they are repeated in section 6

C32: Line 328- articulated shell? R32: No, the two samples dated in core 2 are gastropods and not articulated shell.

C33: Line 337 -"Organic matter >2" in which unit is this expressed? % of dry weight? % of total sediment fraction? R33: In Core 4, the white sand at ∼12.5 cm depth, where the organic matter > 2 % of dry weight of total sediment fraction, at line 354.

C34: Line 349-352 - Discussion and again repeating the same explanation R34: We here necessarily describe once again why we have the shell age 32887-34447 BC. We consider that our explanation on the strong wave action during catastrophic events may stay in this section.

C35: 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. R35: Yes, we agree.

C36: Line 383 - the date range obtained is almost 1000 years! You need to constrain the ages much better and more accurately. R36: The dated sample is made of shell, and the large age range is from the laboratory dating on which we proceed with correction of the reservoir effects (line 390-391).

C37: Line 384-391 and elsewhere why are these layers considered to be tsunami related? R37: As previously explained (see table of comment #3), we have changed “tsunami deposits” by “high energy sedimentary layers” until section 6 of the manuscript.

C38: 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 R38: We do not rush to conclusions. The manuscript has long sections of layer descriptions with sample analysis, and we provide results that lead to the identification of tsunami deposits. We appreciate the suggested and helpful publications of Chagué et al. (2011) and Costa et al. (2014); (see lines 204 to 208, and lines 88 to 91).

C39: Line 466-477 - Please move it to Discussion. R39: We have moved these paragraphs to Discussion section (lines 564-577).

C40: 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. R40: Please see our reply of comment #10 here above and lines 541 to 556 in discussion section.

C41: Line 495 - You only mentioned Pyrite on core 7 and now.... Heavy minerals? Which ones? Did you counted them? Please provide quantitative data. R41: We add the bulk mineralogy semi quantitative data in supplementary material (Tables S1 to S12 and Figs. S4 to S15).

C42: Line 500 - pebbles and loading structure- please clarify text. R42: The loading structure is a typical sedimentary marker of deposits. It also means that the heavy pebbles and coarse sediments transported by the tsunami wave in the lagoon end at the base of the sedimentary succession.

C43: Line 506- You wrongly cite Folk (1968) and state he mention ">5" mark for organic matter in tsunami deposits?!? R43: Yes indeed, we removed Folk 1968 (update line 496-497)

C44: Line 508-522 - this paragraph belongs in the discussion. R44: This section is part of summary of results and we prefer not to move it.

C45: Line 525-527 - Please rewrite this sentence. R45: Changed in 531 to 532.

C46: Line 534-536 - sentence not supported by the data presented. R46: The reviewer apparently does not accept our results and interpretation.

C47: Line 538-540 - Do storm layers exist? If no, why? If yes, please compare them with your "event layers". R47: The discrimination between storm and tsunami deposits is largely treated in the manuscript and in discussion (see lines 188-208, 541-556). We explain in our manuscript that frequency and signature of tsunami deposits significantly contrast from those of storm events that leave a faint sedimentary signature.

C48: Line 545-547 - a bimodal curve only represents two likely sediment sources.

Please update references and clarify idea R48: Bimodal means the existence of fine and coarse grain size of sediments. The bimodal sediment distribution is a common feature of tsunami deposits that also depend on the proportion of fine and coarse particles, and degree of erosion during the wave propagation. (new reference: Scheffers and Kelletat, 2003 in line 551)

C49: Line 549 - "consistent depth". Well, below surface yes but you need to provide height above mean sea level to make this correlation credible. R49: Done in updated lines 982 to 983.

C50: 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? R50: Our observation on the radiocarbon hiatus [i.e., in between 13430 year BP and 5065 year BP] may simply be due to erosion processes (taking into account the sea level and exposed continental domain during the late Pleistocene and early Holocene). However, we have no documented work with precise data on this issue.

C51: Line 559-561 - Strongly disagree. You have not proven this point. R51: All evidences (proofs) are presented in the manuscript and we do not share the reviewer opinion. Our interpretation supported by the presented data and results in manuscript suggests that the three high energy sedimentary layers made of mixed white sand and coarse layers with broken fossils (also observed 200 km apart for one of them) are the trace of tsunamis events in AD 365, 1303 and 1870.

C52: Line 565 - "chemical characteristics". You could also provide geochemical data. Which elements have you measured? R52: We did bulk mineralogy using XRD and identified the minerals according to the fingerprint (Å) of minerals with semi quantities analysis. We provide the bulk mineralogy analysis in supplementary material (Tables S1 to S12 and Figs. S4 to S15).

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-62/nhess-2018-62-AC2-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2018-62, 2018.