

## ***Interactive comment on “Past African dust inputs in Western Mediterranean area controlled by the complex interaction between ITCZ, NAO and TSI” by P. Sabatier et al.***

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We acknowledge the anonymous referee#2 for this constructive review. We revised it in detail as described below from the document in supplementary which summarize major and minor comments.

detailed Comments: Page 1, l. 27: The term “long-term” may be a bit confusing. I would recommend the authors to clearly specify in the manuscript what they mean by using “long-term” In the same phrase be precise “Millennial”

Page 1, l. 31: explain the acronym for NAO We add

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Page 2, l. 19-20: “the influence of the Holocene variability ... remains poorly quantified”: the authors may add some references as Cockerton et al., 2014, JGR – DOI:10.1002/2013JD021283 > We add this reference thank Di Rita et al., 2018, Nature, Scientific Reports - DOI:10.1038/s41598-018-27056-2 > We do not see any link with this publication Hayes & Wallace, 2019, QSR - https://doi.org/10.1016/j.quascirev.2018.11.018 > This publication is a synthesis and we previously published dust reconstitution, thus we prefer cited the related reference for Holocene variability > (Mulitza et al., 2010)

Page 3, l. 9: explain the acronym PSA Thank we add : potential source area (PSA)

Page 3, l. 6: replace the coma after (Moulin et al., 1998) by a dot OK

Page 3, l. 1-2: I recommend the authors to mention in the abstract that the study is based on analytical approaches (including grain-size, mineralogy and geochemistry) as well as on statistical analysis Mineralogical, geochemical and wavelet analyses are already mentioned in the abstract (P1, L21, 30)

Page 3, l. 28: why is there a difference between the XRD sampling resolution (2 cm) compared with grain-size and LOI (1 cm)? Because this analytical method is time consuming with chemical long chemical preparation, thus we choose to analyse less samples compared to faster acquired analysis such as grain-size and LOI

Page 5, l. 18: explain AR (Auto Regression?) Yes Auto Regressive, we add: Auto Regressive model (first order: AR(1))

Page 6, l. 12: in the sentence “ a high organic content”, do you consider the LOI as reflecting the organic content? Does it mean that the uppermost part of the core contains up to 20% of organic carbon? The respective use of LOI550 and LOI950 is not very clear as described in the method page 3, l. 25-27. I do not understand why the authors use both the LOI550 and NCIR on figure S1 since NCIR= initial dry weight - LOI550. Need clarification. LOI550 and LOI950 represent respectively the organic and

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carbonate content of a sample express in %. The NCIR is the non-carbonate ignition residue which correspond mostly correspond to silicate. We add this sentence in the method part LOI550 is not equal to Organic Carbon because during Organic burning CO and CO<sub>2</sub> are created and we do not a monitoring of these molecules ratio. We use these two curves for a better reading of this figure even on is the opposite of the other

Sedimentological parameters of the uppermost part of the core seem rather different from the rest of the core: dark coloured (figure S1, left); slightly coarser grain-size; large range of variations for LOI550 and NCIR. Do you have any explanations? Yes, this explanation is previously publish, we add the reference to this work by adding the following sentence: “This upper increase in organic matter content is related to the lake eutrophication induced by recent increase of atmospheric nitrogen deposit which enhance lake primary productivity (Roche and Loÿe-Pilot, 1989).”

Page 6, l. 16: You mentioned terrigenous supply from the watershed. Could they give some details about this supply? In this result part we just hypothesis that the NCIR is probably due to biogenic silica and terrigenous input from the watershed and eolian deposit and thus in the discussion we conclude that the input from the watershed is negligible. Thus we do not have any detail to provide on this supply because we do not observe any watershed input.

Page 6, l. 16-18: “The grain-size presents a homogenous content (median (D50) =  $32 \pm 8 \mu\text{m}$ ). The average median is  $32 \mu\text{m} \pm 8$ . Is it consistent with remote eolian supply? Add some references. On figure S1, the grain-size distribution is slightly different on the upper 10 cm. (see comment above) Grain size in this lake sediment is mostly affected by biogenic silica (see figure in the answer of Reviewer 1 comment) thus these grain size data could not be used to track thin particle from Aeolian origin. We just use here grain size to illustrate that there is not coarse deposit link to flood or terrigenous events. For the upper centimetre it is probably due to lake eutrophication with larger Diatom, see above comment. We add the following sentence: “Grain size... is mostly affected by biogenic silica not remove before analysis. However, not

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large grain size variation related to flood events (Sabatier et al., 2017; Wilhelm et al., 2015) “

Page 6, l. 19-20: I agree that chlorite and illite display an increasing trend throughout the record, but kaolinite seems to first increase between 100 and 50 meters, before varying around its average value between 50 and 20 meters. How do you explain this discrepancy between clay minerals? In the text me add “. . . with a stabilization for kaolinite around 50cm “, In this result part we do not want to interpret the clay mineral profile, the interpretation is provide in the discussion part

Page 6, l. 20-21: How do you explain the peak in palygorskite around 6 cm, in phase with the NCIR% ? Probably a short term input of African dust, this peak is also correlated to the Fe proxy of dust, but again in this result part we do not what to interpret the result.

Page 6, l. 24: I am not sure that I did understand the use of the composite section. The BAS13PA core seems to be the most detailed sequence and most analyses were performed on P4, but all radiocarbon analyses except one were performed on the BAS13P1. Please clarify. We used a composite section because the longer core P4 is sample with hammering thus we can observe a more compacted part over the upper section related to high water content, thus we prefer use for the upper part a better preserved section. The P3 is too short for long term reconstitution. For C14 age it is quite difficult to find macroremain thus we different core to find enough material and then we correlate age on the composite core thanks to XRF data

Page 7, l. 8: (and Figure 2a) CaO seems to correlate with granodiorite watershed while K<sub>2</sub>O seems to correlate with Quaternary deposits. What is the main composition of these Quaternary deposits? Unfortunately, we do not have a full description of these deposits, but there are moraine and delta deposits with some organic material and me by more thin material. But it will be too much speculative to interpret is information. The important thing is that lake sediment is low influenced by CaO and K<sub>2</sub>O as illustrated

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in figure 2 and discuss in the manuscript

Page 7, l. 30-31: I would rather say, "This lake system is ideal for recording centennial variations of atmospheric inputs" according to the well-constrained chronology of the core I'm not fully agree with this, because if this lake sediment record well define lead pollution coming from atmospheric deposition it mean that this lake is also good to track atmospheric input and yes the chronology is also well constrain

Page 8, l. 8-10: " The I/K ratio is typical of Saharan sources (so PSA1 or PSA2)..." "the low value of C/K is typical of western Sahara (PSA2)..." But in details, according to figure 4: - The I/K ratio mainly varies between 1 and 1.6, suggesting that Sahara is the main source of dust, pointing out PSA1 ( $1 < I/K < 2$ ) as the main provenance (except ca. 2650 yr. cal. BP, 2150 yr. cal. BP, and between 900 and 700 yr. cal. BP when I/K is below 1, suggesting some sahelian supply from PSA3 ( $0.3 < I/K < 0.7$ ), and except ca. 2400 yr. cal. BP and in the most recent part of the core when some influence of PSA2 ( $I/K > 1.6$ ) cannot be ruled out); - The C/K ratio varies between 0.2 and 0.8, suggesting western Sahara as the main source, with PSA2 ( $0 < C/K < 0.8$ ) (and PSA3 with  $0.2 < C/K < 0.9$ ) being the main dust supplier. The C/K ratio reached 1 in the uppermost part of the core, suggesting potential contribution of PSA1 ( $C/K = 1.5$ ) - why PSA3 is ruled out as a contributor? Thanks for this constructive comment, Yes, the I/K ratio of the record (around 1.2) is typical from PSA1 ( $1 < I/K < 2$ ) or by a mixing between PSA2 ( $I/K > 1.6$ ) and PSA3 ( $0.3 < I/K < 0.7$ ) if there are multiple sources contribution. C/K ratio is typical of PSA2 ( $0 < C/K < 0.8$ ) or PSA3 ( $0.2 < C/K < 0.9$ ) and far from PSA1 ( $C/K = 1.5$ ), except for the upper part of the record. Thus if we consider these two ratios together the main dust sources are PSA2 and PSA3 except for the recent part. So we thank the reviewer for this comment and modify this part according to this interpretation in the new version of the manuscript.

- The increase of palygorskite throughout the whole time interval may indicate enhanced contribution of sahelian source as PSA3 which is not consistent with the I/K and C/K ratio How do you reconcile these apparently contrasting results (I/K indicating

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PSA1 as the main provenance, C/K suggesting PSA2 as the main source)? Could you add the Sahel-Sahara limit on Figure 1? We add the Sahara/Sahel limit on figure 1 The review probably wants to said "the decrease of palygorskite" as the long term trend over the 3000 ys record present a decreasing trend. Palygorskite content in variable between each potential sources, PSA3 is clearly depleted while PSA1 and PSA2 are rich in palygorskite (Formenti et al., 2011; Scheuven et al., 2013). However, there are large variations in palygorskite content in PSA1 and PSA2, as these PSA are large areas. For Grousset et al., (1992) and Bout-Roumzeilles et al., (2013), PSA2 (W Sahara) seem more rich in palygorskite than PSA1 (N Sahara). Thus, we interpret this decreasing trend in palygorskite as a result of a more important influence from PSA1 through time, attested by the increasing trend of C/K ratio and higher value for the upper part of the record. In the new version of the manuscript we add the reference to the work of Grousset et al., (1992) to support this long term interpretation.

Page 8, l. 10-13 "The decreasing trend in palygorskite (check the spelling a it appears as palygorskyte) content probably reflect progressive trend with more input from PSA1 over (? – A word is missing line 11), as also ...". Why not considering PSA3 as a potential increasing palygorskite-depleted source? May be PSA1 is less rich in palygorskite compared with PSA2? Please add average percentages of palygorskite on figure 1 Thanks we verify the spelling of palygorskite over the whole manuscript. Yes a part of the sentence is missing, we add "over the last 3150 yrs cal BP" and it is in agreement with higher PSA1 supply in regard to higher C/K and I/K ratio. See above comment for palygorskite interpretation. In figure 1, we prefer keep the current notation for palygorskite contents (Paly+ for PSA1 and PSA2, Paly- for PSA3) because precise palygorskite percentages are variables.

Page 8, l. 22-25: This sentence is a bit confusing. I suggest to modify "we used ..." by "According to PCA analysis of the geochemical dataset, the ratio Fe/Ca vs. Ti/Ca and Fe/K vs. Ti/K were used in order to compare the compositions of lake sediments and associated watershed with dust deposits in NW Med..." Thanks

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Page 8, l. 25-26: "These data allow to identify that geochemical composition of lake sediment is similar to noncarbonated dust samples" I do agree that the Fe/Ca vs. Ti/Ca ratio indicate a good relationship between lake sediment and non-carbonated dust (figure 2b top), but this is not obvious when looking at the Fe/K vs. Ti/K ratio diagram: the lake samples plot on a line with a nice negative correlation while dust samples display a positive correlation! Please clarify the sentence line 25-26 We used this diagram to illustrate that geochemical signature of lake sediment is in agreement with dust signature. To facilitate the reading of this figure we add an orange area corresponding to African dust signature from available bibliography data and lake sediment samples are present in this area in both panel. We do not try to make any proportion calculation or correlation; we just compare geochemical signature in term of possible signature. The negative correlation for lake sediment is not so obvious including samples uncertainties. We add a sentence to clarify this

Page 8, l. 28: "Moreover, Fe and Ca, K contents present the same variations in lake sediments" – data not shown? I agree for Ca, but I am not convinced for K We add the correlation coefficient: Fe vs Ca  $r=0.63$ ,  $p < 10^{-16}$ ; Fe vs K  $r=0.63$ ,  $p < 10^{-16}$  It is also visible on the PCA in Figure S3

Page 9, L. 10: "significant increase from approximately 1000 yr. cal. BP" There is one peak in the Fe signal around 1000 yr. cal. BP but if ignoring this peak, it seems that the enhanced supply in Fe started around 700 yr. cal. BP. No, with the eyes the baseline without peak increases from around 1000 cal BP and the breakpoint statistical analysis identify also around 1000 cal BP

Page 9, l.14: "we observed an African dust increase that reached . . ." In the Fe record? In previously published data (give refs)? Yes in Fe data in agreement with the publication of Evan and Mukhopadhyay, (2010). To precise this statement we add "in Fe data"

Page 9, l. 21: I suggest adding Northern Hemisphere insolation record on figure 4 next

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to the ITCZ variations In the Figure 4 we add the 20°N June insolation

Page 9, l. 24: I agree with the idea of the relationship between the southward position of the ITCZ and dust emission event if Doherty et al., 2012, 2014 mainly deal with dust transport rather than with dust emission Thanks

Page 10, l. 12: "ENSO" not shown? We precise that the wavelet analyses is not shown in this paper Page 10, l. 13-14: the period seems to be slightly higher for dust compare with ITCZ Figure 5: why the y-scale is different for NAO+ (seems to be vertically compressed)? Could you align the xscale of the diagram in order to make the comparison of NAO+ with the other data? Yes we change this figure and we align X and Y on the same scale. The period around 2700-3100 years is not well constrain in age for ITCZ (120-250) but match with the one of dust signal

Page 10, L. 20 and Figure 6: what is the period for cross wavelet analyses for dust input vs. NAO? It seems to be around 450-500 yr.? What is the robustness of this period considering the length of the analysed record (1000 yr.)? We thank the reviewer for this point. We forgot to precise that because of wavelet analysis, Yaxis is dyadic. More accurately, the main period is around 415 years but ranges from 350 to 470. This is longer than the periods published in literature for NAO. The meaning of wavelet coherency is not directly linked to wavelet structure of individual series but to the covariation between each scale. A speculative interpretation should be that there is a buffering between the trigger (NAO) and the effect (Dust transport) explaining the discrepancy between NAO known periods and this wavelet coherency. About the robustness of this period, this analysis is only interpreted in the cone of interest (COI) where no edge effect could interfere the wavelet analysis then the coherency analysis. Indeed, such analyses couldn't have been done with FFT analysis because of FFT based assumptions

Page 10, l. 30-31: "which correspond to the period of long-term increase in African dust " Do you mean timeinterval, "suggesting that the long-term forcing through ITCZ

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migration have an impact on the NAO/African dust correlation” I am not convinced that your dataset evidence that the progressive southward migration of the ITCZ has an impact on the NAO/dust correlation. Could you clarify?

Page 10, l. 34: “The position of Westerlies are influenced by the NAO” indeed, modification of the Westerlies is one of the consequences of the north Atlantic Oscillation (Moulin et al., 1997) Yes for sure, but for the demonstration it is important to call back this.

Page 11, l. 2: The positive phase of the NAO is modelled in winter but with an impact on ITCZ during spring (april) The NAO reconstruction is based on different proxies that are not specifically sensitive to winter. Thus, the Franke et al. (2017) NAO reconstruction is annual, including also atmospheric variation during the other seasons that resemble NAO-like structure. This is a classical approximation in NAO reconstructions. For present-day, annual NAO are mainly dominated by winter variations, but the other seasons can also have an influence. Thus, we argue that this may explain the linkage we have found between annual NAO and spring ITCZ.

Page 11, l. 14: “large changes in solar radiation” What is the range of variation over the last 3000 yr.? Does this range of variations large enough to promote the proposed modifications of the climatic system? Could you consider adding a record of calculated irradiance over the last 3000 yr.? The range of variation of the solar radiation are subject to strong debate. The main consensus is that the amplitude of centennial variability may be of the same order of magnitude as the well-observed 11-year cycle. In that sense, the regression analysis we produced in Fig. 7 is well-adapted to show that such an amplitude is likely sufficient to have a significant impact on the climatic system, even if very weak. Thus, we agree with the reviewer that the changes are not that large, and we have removed this adjective in the manuscript. Indeed, the former sentences were rather insisting on the UV part of the spectrum which is more changing than the whole TSI, so that the changes in TSI are usually moderate, while changes in UV can be larger. We do not feel that adding a record of calculated irradiance will

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bring much, since we are mainly looking at qualitative relationship in this part of the manuscript, and are not using climate models there.

Page 11, l. 25: “strong negative anomaly” I do not get it! Could you give additional details on order to highlight this negative anomaly on figure 7? Figure 7 of the initial manuscript is showing the regression of SLP on TSI. Thus, we have positive values when TSI is increasing, which are shown in the Figure. When TSI is decreasing (what is described in the sentence), the anomalies are getting negative, as discussed here. Nevertheless, we agree that this may be misleading since we are mainly discussing negative TSI anomalies in the paper, so we have reversed the sign of the regression to make things easier to follow and we have modified the figure as follow:

Page 11, l. 17-34: I am not convinced by this paragraph since I am not able to evaluate the potential impact of changes in irradiance on dust emission. May be of interest to have a look on studies about the impact of dust on surface solar irradiance since the emission of dust may modify the effect of solar irradiance on surface Kosmopoulos et al., 2017, Atmos. Meas. Tech., <https://doi.org/10.5194/amt-10-2435-2017> Alonso-Montesinos et al., 2017, Atmospheric Environment, <https://doi.org/10.1016/j.atmosenv.2017.09.040> Granados-Munoz et al., 2019, Atmos. Chem. Phys., <https://doi.org/10.5194/acp-19-523-2019> We agree with the reviewer that the mechanism we are proposing is not supported by simulations including the dust emission and transport (although it is supported by a modelling paper concerning the link between TSI and atmospheric circulation). We have added a discussion in the manuscript to highlight this caveat and we also discussed the potential feedback of dust emission on solar irradiance reaching the surface, using the three papers mentioned by the reviewer.

Page 12, l. 8: I am not convinced by the conclusion “with an increase since 1070 yr. cal. BP in response to a gradual orbitally-induced decrease in northern Hemisphere insolation”. I do agree about the long-term relationship (over the last 3000 yr.) but I disagree with the 1070 yr. inflexion point The break-point analyses on the linear

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regression model between reconstructed dust signal and ITCZ clearly identify that 1070 yr is the main change with a broken-line relationships. To integrate the reviewer caution we change “have been forced” by “could be forced” in the new version of the manuscript

Page 12, l. 11: “since 1070 yr. cal. BP, the NAO is dominant. . .”. The wavelet analysis was performed over the last 2000 yr., so you cannot rule out any influence of the NAO on the 3000-2000 yr. interval Ok, we moderate this sentence: Even if precise NAO reconstitution is not available before 2000, wavelet analysis show that between 3150 and 1070 yr cal BP (ITCZ in more northern position) the centennial increases of Saharan dust inputs are correlated to low TSI.

Typo: Page 2, l. 16: delete the extra “in” before “At the scale. . .”

Page 3, l. 6: replace the coma after (Moulin et al., 1998) by a dot

Page 4, l. 28: delete the extra bracket after (Aitchison et al., 2002))

Page 4, l. 30: delete the extra brackets “((van den Boogaart and Tolosana-Delgado, 2008))”

Page 5, l. 22: replace “Dust” by “dust”

Page 6, l. 25: delete the extra “;” after the bracket

Page 8, l. 21: I suggest to replace “with” by “:”; “while” by “and” and “watershed is” by “watershed samples are”

Page 8, l. 24: add a coma in between Sicily and Tomadin (also in figure 2, Page 21 l. 8)

Page 13, l. 10: delete the extra ‘ ’ after Barcelo’

Page 13, l. 13: add a space before Berlin

Page 15, l. 23: use lowercase for the title

Page 14, l. 26: use lowercase for the title

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Page 16, l. 16: change “M.D. Loÿe-Pilot” by “Loÿe-Pilot, M.D.”, also Page 8, l. 15-16

Choose between millennium Page 1, l. 17 and Page 10, l. 17 and millenniums

Page 10, l. 8 Check the spelling of palygorskite throughout the manuscript since it appears sometimes as palygorskyte Thank, we correct all these typo

Figures: Figure 1: Can you consider adding major winds and palygorskite percentages? Indeed the palygorskite varies between circa 5 to 20% and the PSA mineralogical signatures would help to interpret the observed variations. If palygorskite is <5% in PSA3, then you need to consider some contribution from this source area in the uppermost part of the core In figure 1, we prefer keep the current notation for palygorskite contents (Paly+ for PSA1 and PSA2, Paly- for PSA3) because precise palygorskite percentages are variables (Bout-Roumazeilles et al., 2013; Grousset et al., 1992). For sources contributions and palygorskite variation over the upper part see above comment.

Figure 2: symbols (brown diamonds and orange squares) on figure 2b are hardly readable; can you consider having the figure 2c in an individual supplementary figure? We modify the symbols colours: Orange > light orange, brown > dark brown

Figure 3: the blue symbols are not readable We enlarge the blue symbol

Figure 4: replace “palygorskyte” by “palygorskite”; add mean value for palygorskite Thanks Figure 5: use same x- and y-scale for all 4 parameters (NAO+); check the legend Page2, l. 3-4: “labelled in white” since I do not see white labels We modify this figure to have the same X and Y scale and change the label white to black, thanks

Figure S1: replace “palygorskyte” by “palygorskite” Thanks

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of northern African dust and its source sediments – A compilation, *Earth-Science Reviews*, 116, 170–194, doi:10.1016/j.earscirev.2012.08.005, 2013.

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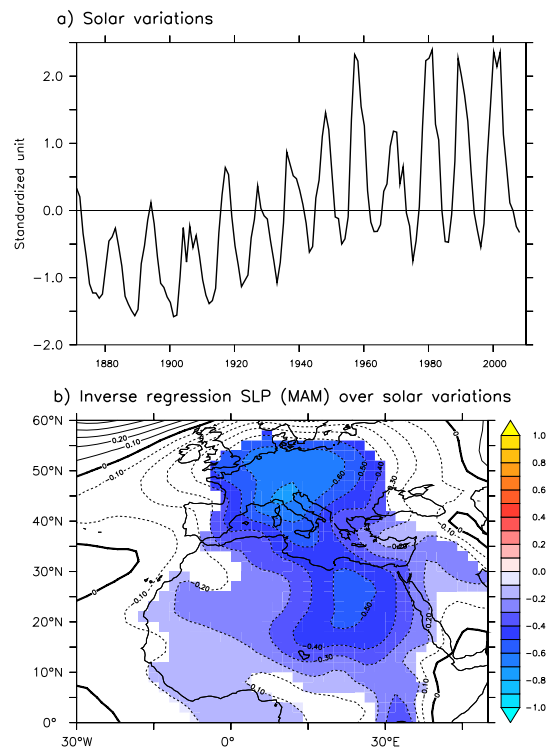


Fig. 1.