**Reply to Reviewers' Comments**

**First Reviewer**

**Review for revised version of "Dakar Niño under global warming investigated by a high-resolution regionally coupled model" by Koseki et al.**

**I appreciate the authors' effort in addressing the issues raised in my previous review. I find the revised manuscript to be improved, but there are still a number of issues that need to be taken care of before publication, in particular regarding the clarity of the presentation and the processes driving Dakar Niños.**

Thank you very much for further careful reading and constructive comments. We revised the manuscript following the comments and we reply to the comments point-by-point as below. Please note that the track changes are shown by blue color in the revised manuscript.

**Major comments:**

**A) I find it still very hard to follow the manuscript and its argumentation. Please make sure that is always clear to the reader why certain analyses are done and what is shown in the plots before getting to what the results are. As an example, in the paragraph starting from line 190, it would be instructive to first state that in order to describe the evolution of Dakar Niños and Niñas and to evaluate how well it is simulated in the model and how it might change in the future (if that is the purpose of the figure - I am actually not sure!), correlation of the Dakar Niño index with the large scale wind stress and SST field are performed. Then you can get to where these correlations are high and what this means.**

**REPLY**: Thank you very much for this comment. We added more introductive descriptions on the plots of Figs. 5, 6, 7, and 8 for enhancing readability because we found that these plots lacked some explanation for the readers. Please read lines 195-196, 213-217, 227-229, and 249-250.

**As another example, for Fig. 6, there is an abrupt transition from "climatology of SST and wind stress is given" to "In ROMP, the significant positive**

**correlation concentrates between the surface and 40m depth and decreases to 100m depth, which is about 0.4 (Fig. 6a)." Please state first what is looked at in Fig. 6 and why before getting to what one can see, i.e. that the Dakar Niño index is well correlated with the ocean temperature over the upper 40m of the water column.**

**REPLY**: First, we deleted the sentence "climatology of SST and wind stress is given", which is not adequate there. As we replied to the previous comment, we added more descriptions before the plot of Fig.6. There, we refer to Fig.S4. Please see lines 214-217.

**B) Related to point A, the discussion of the correlation figures is rather confusing. Please note that correlations can be high or low and variables can be highly/strongly or weakly correlated, but not "deeply".**

**In particular for Fig. 6, I cannot make sense of what is suggested here. The plots indicate that there are higher subsurface temperatures during Dakar Niños and weaker upwelling. To me, this implies that, in the model, weaker winds lead to reduced upwelling and turbulent mixing, warming the subsurface and subsequently the surface ocean. This process appears to get more important in the future. It does not mean that "Dakar Niños would influence deeper ocean in the future" (as stated in lines 207/208).**

**REPLY**: Thank you very much for the insightful discussion. Here, "deeply" we used means that the higher (or, significant) correlation with Dakar Index can be seen at deeper layer in $ROM_F$ than $ROM_P$. Because Fig.6 shows just correlation, the information does not indicate any strength of variability. However, we agree that such correlation pattern does not mean **"Dakar Niños would influence deeper ocean in the future"**, and so we rephrased that part. Please see lines 216-222.

**C) Again related to point A, the newly added heat budget calculation appears rather disconnected from the rest of the manuscript. While in lines 235/236, it is stated that "According to Oettli et al. (2016), the Dakar Niño is mainly driven by changes in alongshore local surface wind", this section (from line 294) starts with stating that "According to Oettli et al. (2016), surface heat flux is responsible for generating Dakar Niño events". This needs at least some more context and explanation as the previous section has not mentioned whether the wind is important because of its impact on latent heat flux. By showing vertical velocities and subsurface temperatures, it is rather implied that it is the effect of wind changes on upwelling that are important.**

**REPLY**: Thank you very much for the insightful discussion. First, the expression "Dakar Niño is mainly driven by changes in alongshore local surface wind" is not correct and we changed "driven" to "associated with". Please see line 249.

During this revision, we re-visited the heat budget of the previous version and we found something wrong in the calculation: the surface heat flux was underestimated in the code. So, first please let us correct the discussion on the heat budget. Here, we show a plot of the corrected heat budget as Fig.R1,

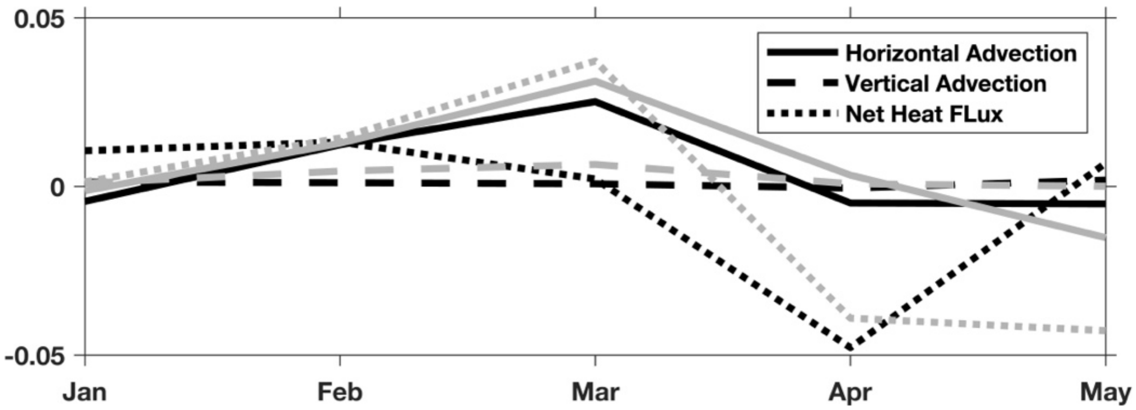


**Fig.R3**. Corrected version of composite differences in the heat budget in the box of 9N-14N and 20W-17W between the Dakar Niños and Dakar Niñas for the ROM simulations. Black is for the present ($ROM_P$) and grey is for the future climate ($ROM_F$).

As Fig. R1 shows, the ROM simulations also show a vital role of surface heat flux as Oettli et al. (2016). In March, the heat flux intensifies more in the future and this strengthened heat flux can explain the stronger Dakar Niño/Niña. This larger heat flux anomaly is due to the stronger surface wind anomaly as Fig. 8 shows (correlation of heat flux and meridional wind stress is 0.88 over the Dakar Index box in March). In addition, mixed layer depth anomaly is also larger in the future, and this can help enhancing surface heat flux contribution as we described in the previous manuscript.

However, in $ROM_P$, the heat flux anomaly for the Dakar Niño events seems to be underestimated especially in January and February. Oettli et al. (2016) showed that shortwave radiation anomaly is a main contributor to the positive heat flux anomaly inducing Dakar Niño, but it is not discussed what generates the shortwave radiation anomaly in detais. There are two possible internal factors: cloud and aerosol. Especially, the focusing region is in the vicinity of the Sahara where dust emission is the largest. As Chen et al. (2021), the dust from the Sahara is quite important in surface heat budget in the north tropical Atlantic and they showed a cooling effect of dust on SST. Because ROM implements “climatological” dust forcing (Pietikäinen et al., 2012, we cited this in the revised manuscript), the heat flux anomalies may be not well represented inducing the Dakar Niños. It is very insightful to investigate how dust anomalies can induce shortwave anomalies and consequently, SST anomalies, but it is out of scope of this study. This point should be made in one of future works.

As another reviewer suggests, we also computed the heat budget for ORAS5 given in Fig. R2. The events of Dakar Niño and Niña are the same between ERA5 and ORAS5 because Dakar Indices give almost identical characteristics as shown in Fig. R3.

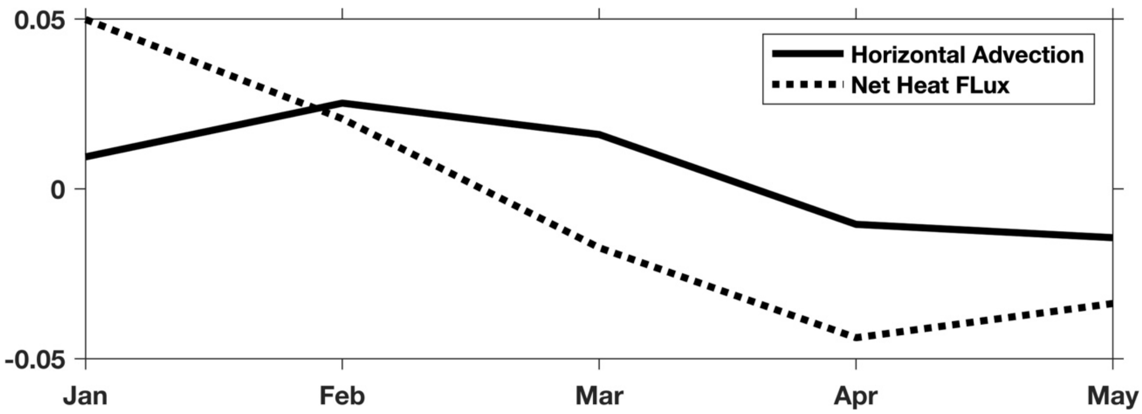


**Fig.R2**. Composite differences in the heat budget in the box of 9N-14N and 20W-17W between the Dakar Niños and Dakar Niñas for ORAS5.

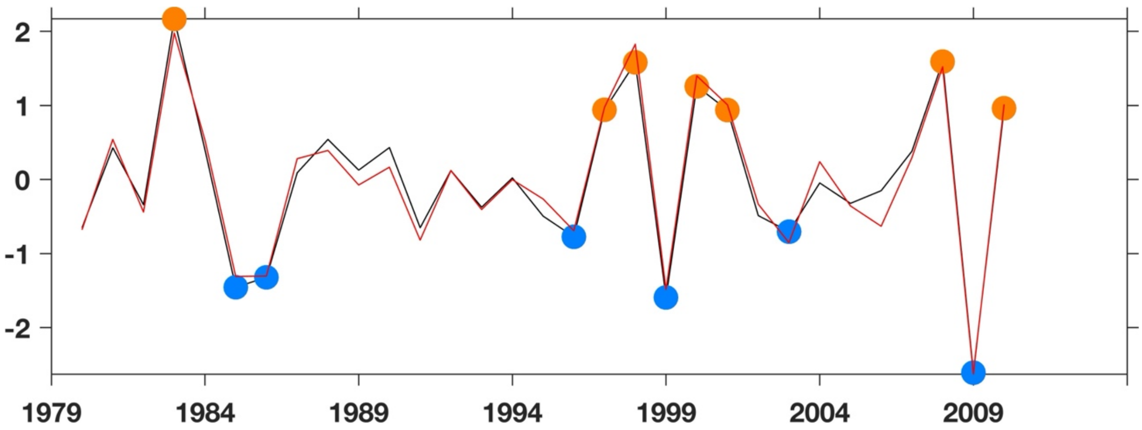


**Fig.R3**. Dakar Index for (black) ERA5 and (red) ORAS5. Orange and blue dots denote Dakar Niño and Niña events in ERA5 as given in Fig. 4 of the manuscript.

As Oettli et al. (2016), ORAS5 indicates that net heat flux is responsible for Dakar Niño in 1-2 months advance. Horizontal advection is also comparably important to the Dakar Niño. The magnitude of the estimated horizontal advection is roughly consistent with that of $ROM_P$(please see Fig.R3). Between ORAS5 and ROMP, the horizontal advection is comparable, and heat flux shows a difference in magnitude: ROM simulation underestimates the heat flux partially because shortwave radiation anomalies due to dust could be represented as we describe above.

Following this correction and additional analysis, we modified significantly our discussion and conclusion. Please see lines 22-25, 329-350, and 371-373. We added Fig.R2 and R3 as new Fig. S6 and Fig.R1 are replaced with Fig.11.

Regarding the isolation of heat budget section, we added some texts in the introduction to guide the readers to heat budget analysis. Please see lines 41-42, and 74-76.

**Reference**:


Chen, S.-H., Huang, C.-C., Kuo, Y.-C., Tseng, Y.-H., et al. 2021: Impacts of Saharan Mineral Dust on Air-Sea Interaction over North Atlantic Ocean Using a Fully Coupled Regional Model. JGR-Atmosphere, https://doi.org/10.1029/2020JD033586.

Pietkäinen, J.P., O´Donnell, D., Teichmann, C., Karstens, U., et al., 2012. The regional aerosol-climate model REMO-HAM, Geosci. Model Dev., 5, 1323-1339, 10.5194/gmd-5-1323-2012.


**Surprisingly, the equation used for the heat budget does not even contain a term for the surface heat fluxes that, however, show up in Fig. 11 without ever being introduced before. This Figure then suggests that it is the horizontal advection that is mainly driving Dakar Niños and Niños, another process that has not really been introduced so far.**

**REPLY**: Thank you very much for raising this point. This is completely our mistake. We added net hat flux term in the heat budget equation and some descriptions. Please see lines 317 and 321-322.

**To address these major comments, I would advice the authors to first show that Dakar Niños are related to the wind field (Fig. 5) and that the wind variability is intensifying in the scenario simulation (Fig. 8), explaining the increase in SST variability. Then there could be a section on the different mechanisms that are related to wind variability, namely upwelling, advection and latent heat flux and how each of them is changing.**

**REPLY**: Thank you very much for this constructive comment. Our philosophy is that first we would like to show the future change of SST variability (Fig.2) so that we could draw attention from readers. Showing sequentially the change of other oceanic properties and connection to SST variability (Figs.5-7) could extend the overview of change in Dakar Niño. Then, in Section 4, we discuss in detail why Dakar Niño is amplified focusing on winds and heat budget. As we replied to Comment (A), we added more introductive descriptions on the plots, we would suppose that the flow of story now increases readability. However, since Section 4.1 becomes a long section after the revision, we divided wind change and heat budget to Section 4.1 and 4.2.

Minor points:

**1) I don't understand what is meant by lines 17/18 in the abstract (see also major comment B above).**

**REPLY**: As we replied to the Major Comment B, that means that temperature and vertical velocity are correlated with SST at deeper layer in the future. So, we rephased that part of abstract. Please see lines 18-19.

**2) Instead of the current Figure 1, I would suggest to show a map for the coupled domain with SST standard deviation in shading and mean SST overlaid in contours as well as the box indicating the Dakar Niño region. Then this area of high SST variability can be referred to early in the manuscript.**

**REPLY**: Thank you very much for the comment. We re-plotted Fig. 1 following the comment and referred the Fig.1 in the introduction as well. Please see the new Fig.1 and lines 28 and 40.

**3) Related to major comment A above, the transition in line 160 is not very clear. Please state explicitly that you are now referring to future changes.**

**REPLY**: We rephrased that part for better connectivity. Please see lines 163-164.

**4) In the discussion of the Dakar Niño index time series (lines 172 to 177), please also refer to the change in standard deviation visible in Fig. S1.**

**REPLY**: We added it. Please see line 181-182.

**Specific comments:**

**- line 21: "more important" instead of "more explainable"**

**REPLY**: Replaced.

**- line 25: "Climatologically" instead of ""From a climatological aspect"**

**REPLY**: Replaced.

**- line 29: "northern boundary" instead of "northern end"**

**REPLY**: Replaced.

**- line 30: "joins" (typo), "North Equatorial Current"**

**REPLY**: This is not a typo, but that is what we meant.

**- line 59: "studies" or "analysis" instead of "surveys"**

**REPLY**: Replaced.

**- line 59: add recent study on future evolution of Benguela Niños by Prigent et al., (2024)**

**REPLY**: Here, we mention "equatorial" Atlantic variability while Prigent et al. studies Benguela Niño, which is not equatorial. Instead, we cited Prigent in the conclusion section.

**- line 76: "configuration … is" or "configurations … are"**

**REPLY**: Replaced.

**- line 77: "… atmospheric component, namely the limited-area…"**

**REPLY**: Done.

**- line 78: "… and a global oceanic component, which is the Max-Planck Institute Ocean Model…"**

**REPLY**: Done.

**- line 85: "this" instead of "those"**

**REPLY**: Replaced.

**- line 99: "shading shows" instead of "shade show" Is the topography height actually relevant for the study? Maybe it would be more instructive to show SST instead.**

**REPLY**: As we replied to minor comment #2, we replotted SST in Fig.1.

**- line 110/111: Not sure what is meant by "The steep SST gradient is consistent with the SST seasonal cycle"**

**- line 126: What is meant by "in a whole year"? "during the whole year" or "for all calendar months" maybe?**

**REPLY**: Corrected to "during the whole year" as suggested.

**- line 136: "absolute" (typo)**

**REPLY**: Corrected.

**- line 137: "meridional" (typo)**

**REPLY**: Corrected.

**- line 141/142: This is not a full sentence and it is not clear what is meant. Please rephrase.**

**REPLY**: Rephrased it.

**- line 146: "focus on"**

**REPLY**: Corrected.

**- line 148: Please remove "for fairness comparison with observations"**

**REPLY**: Removed.

**- line 149: "averaged over 9ºN - 14ºN"**

**REPLY**: Added.

**- line 152: "The yellow box indicates the Dakar Niño index region."**

**REPLY**: Corrected.

**- line 172: "in ERA5, there are" instead of "ERA5 counts"**

**REPLY**: Corrected.

**- line 197: Part of the sentence seems to be missing.**

**REPLY**: Corrected.

**- line 198: It's the connection between the Dakar Niño index and the wind field that is not well simulated.**

**REPLY**: Corrected.

**- line 204: Why is the climatology shown here? What is the connection to the preceding or following text?**

**REPLY**: As we replied to Major Comment A, we corrected the connectivity and the texts on Fig. S4 are given in the explanation of Fig. 5. Please see lines 214-215.

**- line 253/254: "more intensely than the ocean"**

**REPLY**: Corrected.

**- line 266: "In the climatology"**

**REPLY**: Corrected.

**- line 297 and in many following sentences: "ocean mixed layer" instead of "ocean mixing layer"**

**REPLY**: Corrected.

**- line 338: "in contrast to" instead of "in discrepancy against"**

**REPLY**: Corrected.

**Second reviewer**

**I am not satisfied with the revision and the responses provided by the authors. Particularly, the authors continue to only focus on the alongshore wind, while the mechanism behind the Dakar Niño/Niña is more complex, as already highlighted in the review.**

**REPLY**: Thank you very for the comment. In the first revision, we elaborated our discussion by adding the heat budget analysis and we suggested an importance of horizontal advection for Dakar Niño. In this revision, we added a heat budget analysis for reanalysis data (please see our reply to a major comment as below) and we recognized that surface heat flux is responsible for Dakar Niño, but our ROM simulation appears to underestimate the contribution of heat flux. According to Oettli et al. (2016), shortwave radiation plays a vital role in inducing the Dakar Niños. So, it is likely that our ROM fails to represent such shortwave radiation anomaly and it is important to investigate what makes the shortwave radiation variability, probably, by cloud and/or dust from the Sahara. This point is quite important and insightful, but at this moment, it is out of scope and will be explored in the future.

**To the credit of the authors, they have performed some heat budget (as suggested in the review) but the results are quite different from those found in Oettli et al. (2016), which is fine. But the differences are quite drastic and seem to invalidate previous studies. This must be thoroughly discussed in the manuscript. Also, the heat budget is not performed on observation/reanalysis. This should be done and shown at least as supporting information, to compare with ROMp heat budget (Figure 11).**

**REPLY**: Thank you very for the comment. Yes, heat budget analysis for reanalysis data is important to elaborate our discussion. Therefore, we performed a heat budget analysis with ORAS5 (approximately 0.25ºx0.25º resolution). Please note that ORAS5 does NOT provide vertical velocity, and we could not estimate vertical advection. First, we checked the Dakar Niño/Niña events in ORAS5 comparing to ERA5 as shown Fig.R1.

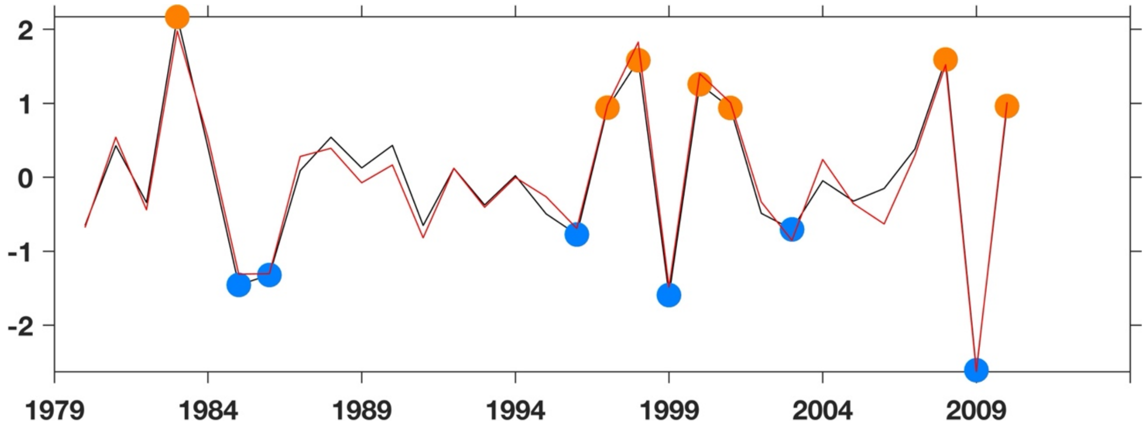


**Fig.R1**. Dakar Index for (black) ERA5 and (red) ORAS5. Orange and blue dots denote Dakar Niño and Niña events in ERA5 as given in Fig. 4 of the manuscript.

Actually, ORAS5 performance is quite similar to ERA5 and then, we can regard that the Dakar events of ORAS5 are same as ERA5. Then, we calculated the heat budget of ORAS5 in the events as given in Fig.R2.

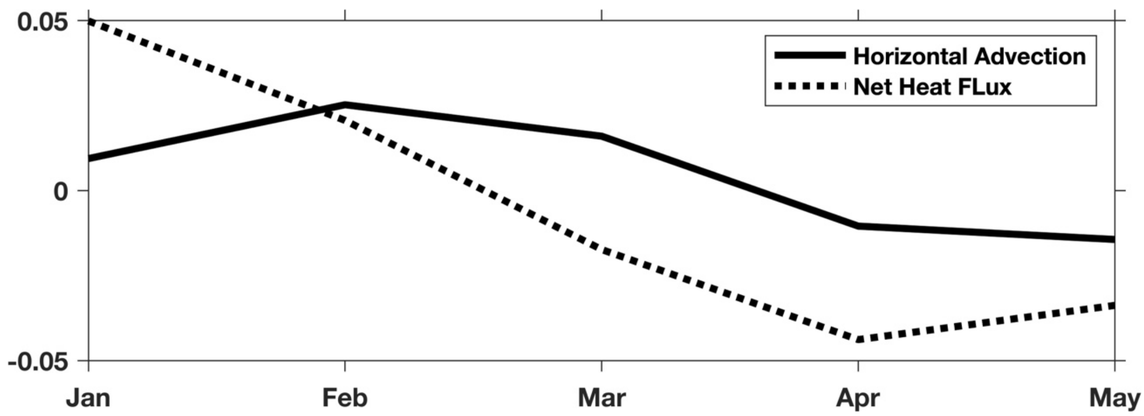


**Fig.R2**. Composite differences in the heat budget in the box of 9N-14N and 20W-17W between the Dakar Niños and Dakar Niñas for ORAS5.

As Oettli et al. (2016), ORAS5 indicates that net heat flux is responsible for Dakar Niño in 1-2 months advance. Interestingly, horizontal advection is also comparably important to the Dakar Niño. The magnitude of the estimated horizontal advection is roughly consistent with that of $ROM_P$ (please see Fig.R3).

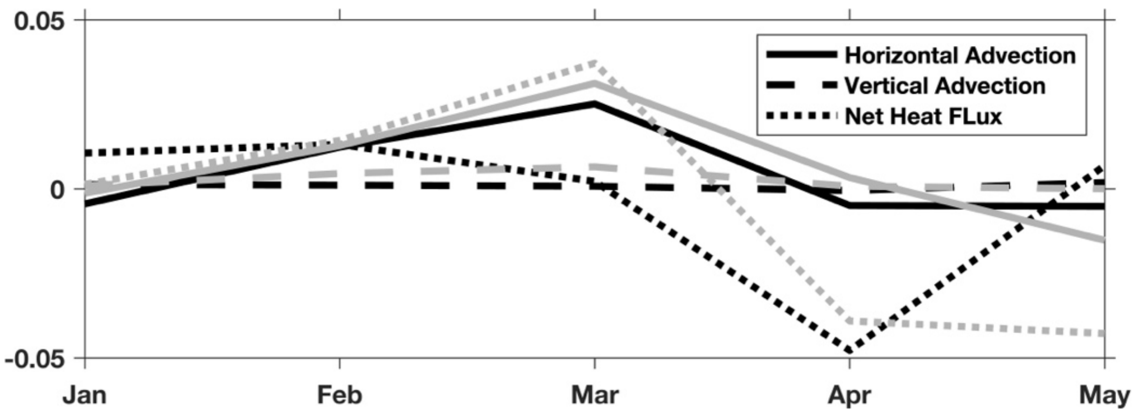


**Fig.R3**. Corrected version of composite differences in the heat budget in the box of 9N-14N and 20W-17W between the Dakar Niños and Dakar Niñas for the ROM simulations. Black is for the present ($ROM_P$) and grey is for the future climate ($ROM_F$).

During this revision, we re-visited our previous heat budget for the ROM simulations and we found that the computation of heat flux was wrong. Here, we show a correct heat budget of the ROM simulations in Fig.R3.

As Fig. R3 shows, the ROM simulations also show a vital role of surface heat flux as Oettli et al. (2016) and ORAS5 analysis. In March, the heat flux intensifies more in the future and this strengthened heat flux can explain the stronger Dakar Niño/Niña. This larger heat flux anomaly is due to the stronger surface wind anomaly as Fig. 8 shows (correlation of heat flux and meridional wind stress is 0.88 over the Dakar Index box in March). In addition, mixed layer depth anomaly is also larger in the future and this can help enhancing surface heat flux contribution as we described in the previous manuscript.

However, in $ROM_P$, the heat flux anomaly for the Dakar Niño events seems to be underestimated especially in January and February compared to ORAS5. Oettli et al. (2016) showed that shortwave radiation anomaly is a main contributor to the positive heat flux anomaly inducing Dakar Niño, but it is not discussed what generates the shortwave radiation anomaly in details. There are two possible internal factors: cloud and aerosol. Especially, the focusing region is in the vicinity of the Sahara where dust emission is the largest. As Chen et al. (2021), the dust from the Sahara is quite important in surface heat budget in the north tropical Atlantic and they showed a cooling effect of dust on SST. Because ROM implements "climatological" dust forcing (Pietikäinen et al., 2012, we cited this in the revised manuscript), the heat flux anomalies may be not well represented inducing the Dakar Niños. It is very insightful to investigate how dust anomalies can induce shortwave anomalies and consequently, SST anomalies, but it is out of scope of this study. This point should be made in one of future works.

We added Figs. R1 and R2 as supplemental information as new Fig. S6 and corresponding descriptions. Moreover, we corrected Fig. 11 and our conclusion in line with correction of heat budget of ROM. Please see lines 22-25, 329-350, and 371-373. Fig.R3 is replaced with Fig.11.

**Reference**:

Chen, S.-H., Huang, C.-C., Kuo, Y.-C., Tseng, Y.-H., et al. 2021: Impacts of Saharan Mineral Dust on Air-Sea Interaction over North Atlantic Ocean Using a Fully Coupled Regional Model. JGR-Atmosphere, https://doi.org/10.1029/2020JD033586.

Pietkäinen, J.P., O´Donnell, D., Teichmann, C., Karstens, U., et al., 2012. The regional aerosol-climate model REMO-HAM, Geosci. Model Dev., 5, 1323-1339, 10.5194/gmd-5-1323-2012.

**Also, the Figure 4 is interesting, but not really discussed. We can see that the years of Dakar Niño/Niña are more or less the same than in Oettli et al. (2016), but using ERA5. Which is an important result. But with ROMp, the results are different. A few years of observed Dakar Niño/Niña are detected (1985, 2003 for example). But there are also important discrepancies that must be thoroughly discussed. What explains the strong 1999 Dakar Niño in ROMp, when it was a strong Dakar Niña? Same applies to 2009. Also, 1988 and 1989 were neutral, but ROMp simulated 2 consecutive Dakar Niños. Finally there are 6 consecutive Dakar Niñas between 2002 and 2007. This doesn't argue in favor of ROM**.

**REPLY**: Because our ROM simulations are forced by MPI-ESM "historical" and RCP585 scenario runs from 1950 to 2100 (already mentioned in Section 2, please see lines 88-91), we do not expect that the Dakar Niño/Niña events in $ROM_P$ occur in line with ERA5. We added one sentence to notify the setting of ROMP in the Section 3 as well. Please see lines 178-180.

Regarding the consistency with Oettli et al. (2016), we also added some descriptions to justify our results of ERA5. Please see lines 175-176.

**It should be noted thath there are a lot of issues with the citations throughout the text (i.e, open parenthesis not correctly placed). This makes the text difficult to read**.

**REPLY**: Thank you very much for the comment. We checked carefully the manuscript and corrected the wrong parentheses throughout the manuscript.

**I therefore recommend major revisions.**

**Some comments (non-exhaustive list):**

**L.145: "To assess the Dakar Niño...". Not sure what the authors mean here. Is it to assess the existence of the phenomenon? To assess the simulation of Dakar Niño in ROM? Please elaborate.**

**REPLY**: This expression seems a bit confusing and therefore, we deleted "to assess the Dakar Niño".

**L.235: "[...] the Dakar Nino is driven mainly by changes in alongshore local surface wind". No, it is not, as already said in the review.**

**REPLY**: We changed "driven by" to "associated with" as Oettli et al. (2016) say. Please see line 249-250.

**L.312-315: Does it mean that ROM generates abnormal coastal SST warming/cooling by horizontal advection? How about ERA5 (hence the mandatory heat budget). Could this explain the differences between ERA5 and ROMp described above?**

**REPLY**: As we replied to the major comment #1, we calculated the heat budget for ORAS5 reanalysis (Dakar Niño/Niña events are almost identical with ERA5). Actually, ORAS5 result is similar to Oettli et al. (2016) showing an important role of surface net heat flux and importance of horizontal advection. On the other hand, $ROM_P$ underestimates the surface heat flux anomaly. However, horizontal advection in ORAS5 and $ROM_P$ are roughly consistent.

**L.287: "The SLP anomaly gradient runs along..." The gradient is across. It is part of the generation process.**

**REPLY**: Corrected. Please see line 301.