Review comments on the submitted article "**Decline of Etesian winds after large volcanic eruptions in the last millennium**" by Misios S, Logothetis I, Knudsen M F, Karoff C, Amiridis V and Tourpali K.

General comments

This study uses data from the 20th Century Reconstruction project (20CR) and the output from the Last Millennium Ensemble simulations with the model CESM (CESM-LME) to investigate the impact of large volcanic eruptions on the summer circulation over the eastern Mediterranean (EMed) and, in particular, the persistent northerly flow, known as the Etesians. The authors present evidence that major volcanic eruptions in the Northern Hemisphere are able to weaken the pressure gradient over the EMed, reduce the intensity of the Etesians and their frequency of appearance. It is also shown that the well-known monsoon-desert mechanism weakens during summers after major eruptions. The effect of eruptions appears to be more obvious for eruptions before the 20th century when the volcanic radiative forcing is considered to be stronger. The work bears the necessary scientific merit and I reckon that it can have an important contribution to the topic because it is characterized by the element of novelty. However, the manuscript suffers at the writing and presentation level. Frequently, there is incomplete discussion on the results, the uncertainties involved and the dynamics explaining the aforementioned link. My recommendation is that the manuscript could only be ready for publication by Weather and Climate Dynamics after major revisions, mainly due to the large number of suggested comments. Below I make specific suggestions that aim at improving the manuscript.

Specific Comments

1) Presentation of the coincidence between volcanic eruptions and reductions in the frequency of Etesians: The presentation of results in Figure 1 should to be improved:

i) Overlaying the cyan and black lines (Figs. 1a and b) that represent CESM-LME all forcing ensemble and CESM-LME volcanic-only forcing makes it difficult to distinguish between the two. Perhaps I miss something but actually, in the text there is very limited discussion on the difference between the two that draws conclusions on the importance of the volcanic forcing alone compared to the all-forcing simulations. If so, does it make sense to show here only one curve? Otherwise, please comment on the difference between the two and the necessity of showing both of them here.

ii) Caption and description of Fig. 1c are problematic. It is not clear what the red and black lines represent. I can only speculate that the black line is the same as the black line (?) in panels 1a,b. But again the vertical range is suppressed and the line appears to very flat, which hinders the effort to draw conclusions on the timing with the volcanic eruptions.

iii) Grey line gives the outgoing clear sky SW radiation in the Northern Hemisphere, but units are not given here except that "scaling is relative". Perhaps units should be also given on the right hand side vertical axis.

iv) L135, L151 and caption of Fig. 1: More on the previous comment. NH, SW and TOA are not explained here. Could you please give more information about the physical basis that relates volcanic eruptions and coincident higher outgoing SW? Does "outgoing clear sky SW radiation" refer to radiation reflected by aerosols in cloud-free regions?

v) I find extremely hard to locate the dates of volcanic eruptions, which correspond to peaks in the grey line (Fig. 1) so as to check the coincidence with the weakening of the Etesians over the EMed. The authors should add several vertical lines that mark all major eruptions mentioned in Table 1. A solution could be to add an initial letter (i.e., P: Pinatubo) that should be also indicated in Table 1.

2) **Details about CESM-LME simulations.**

i) L82-85: Here the authors describe briefly the sets of simulations used but they do not disclose details on how the initialization of the various ensemble members was performed. What is the meaning of "twin ensemble of 5 members"? How was the initialization of these ensemble members made?

ii) Also, it may be helpful that the authors use different acronyms for results obtained with the use of simulations from CESM-LME all forcing, only volcanic forcing simulations and for the cases that all 17 members of CESM-LME are grouped together. For example something like, CESM-LMEaf, SESM-LMEvf and CESM-LMEt, respectively. These acronyms could be used consistently throughout the manuscript to alleviate confusion.

iii) Details of the spatial resolution of the CESM-LME simulations and the 20CR dataset should be also listed. In L125-126, the authors argue that PDF of NED is skewed towards higher values in 20CR when compared to the one corresponding to CESM-LME. But it is not explained how the resolution is expected to affect the PDF. Please clarify.

iv) L157-158 & 270-272: I am not sure that the ensemble size of 17 members is adequate to ensure that volcanic signature will emerge from natural variability. And even more so with respect to the effect of the ENSO on ISM. In recent studies, there is a tendency for the use of sets of at least 100 ensemble members to disentangle signal form natural variability.

v) L89-90 & L92: Is the volcanic forcing applied to the simulations available so as to confirm the hypothesis that the historical forcing was stronger? The terms "historical" and "recent" are frequently used in the manuscript, but sometimes very loosely. Please clarify.

3) **Detailed discussion on the synchronicity between volcanic eruptions and decline in the frequency of Etesians in Fig. 1c**:

i) L142: As mentioned above, the meaning of the dark and red lines is not clarified. I speculate that the red line represents NED frequency based on 20CR.

ii) L144: I disagree with the statement that both 20CR and CESM-LME do not show any significant decline after Krakatau (1883). Perhaps I miss something but I see many minima both in black and red lines (especially the red line – 20CR) in the 1890s, 1910s, 1930s, 1970s & 2000s. This is actually confirmed in the next sentence that refers to the eruption of Pinatubo, which is confusing.

iii) L149: Is it possible to check whether the forcing is actually realistic in CESM-LME simulations specifically during this eruption?

iv) L153: This is very true for 20CR and L-days but may be not so accurate for CESM-LME. But again the vertical axis range used to plot the black line in Fig. 1c should be different so as to depict finer variability. Now the line appears rather flat.

v) Surprisingly, there are many other minima in the red line that are not associated with volcanic eruptions. See for example the one a few summers prior to the 1883 Krakatau eruption. While many maxima in the grey line are not associated with minima in NED frequency, such as the case of early 1960s (1964?). This points out the relevance of other dynamical mechanisms (both in the tropics and mid-latitudes) that influence the summer circulation over the EMed.

4) **Discussion of the "epoch analysis" in Fig. 2**:

i) L154-155: Have you calculated the anomalies shown with respect to each ensemble separately?

ii) L156-157: In the text the authors mention that 17 thin grey lines represent the evolution for all 17 ensemble members. But in the caption of Fig. 2, it reads that results are shown for the 12 ensemble members of CESM-LME all forcing. Please clarify.

iii) L157: It could be helpful to draw with different colors the curves representing the anomalies for the 5 CESM-LME members that use volcanic forcing only. In this sense the impact of the different forcing will become more evident in the case that the authors want to extend the discussion on the role of the varying forcing.

iv) L167-172: I am concerned that the arbitrary choice of the base 5-year period prior to the corresponding eruptions shown in Fig. 2 might create a bias in the calculation of anomalies. It might work in many cases but it could be the reason why the Pinatubo eruption appears not to have an effect on the NED frequency. It could be that in the 5 years prior to the Pinatubo eruption, a strong ENSO event contaminates the 5-year base period. I suggest further analysis by using alternative 5-year periods or even a larger common period as a base to study all eruptions and specifically the Pinatubo eruption.

v) Overlaying the 20CR and L-days lines in Figs. 2a-c could be added for completeness.

vi) L162: What is an effusive eruption?

vii) L167: Could these runs be among the 5 CESM-LME with volcanic forcing only?

viii) L161-166: Why the high-latitude Laki eruption is expected to have a faster and short-lived influence? Is it related to the proximity of Iceland to the North Atlantic storm track that facilitates a faster spreading of the volcanic ash around the Northern Hemisphere? And why is it expected to be short-lived?

ix) L173: "see gray line in Figure 2" > "see gray line in Figure 1" ?

x) L174: Which signatures are inconsistent during the 21st century?

xi) L177: I cannot locate the Kuwai eruption in Fig. 3.

xii) L179: But in Fig. 2d above the reduction of NED frequency for +1 year is not present in CESM-LME (for which results are shown here in Fig. 3).

xiii) L179-182: This is a very interesting result. But what determines that the decline of Etesians will appear in the summer of the same or next year? Why the high-latitude eruption in the Northern Hemisphere are particularly effective in changing the summer circulation over the EMed?

5) **Waning of the Etesians after volcanic eruptions**

i) What are the units in the contours depicted in Fig. 4? Consistent with text the units should be hPa and not Pa. This should be clarified in the caption.

ii) L200-202: I absolutely agree with the inference that the low pressure system over the Middle East becomes shallower after major volcanic eruptions though I would not call it "Anatolian low" because it is not located over the Anatolian plateau. It is evident that the Etesians wane because the pressure gradient weakens over the EMed. But it is also evident that a swallower high pressure system over the Balkans has a contribution to the weakening pressure gradient over the EMed. Therefore the authors need to describe this contribution as well, otherwise the discussion of Fig. 4 is incomplete.

iii) L203-205: north-east > northeasterly wind anomalies? I do not agree that the anomalous flow has the same direction over the Arabian Sea and the Bay of Bengal. In anything, in Fig. 4a, arrows are directed in opposite direction over these two regions.

iv) L208-210: I do not see any anomalous northwesterly flow over the Arabian Sea in Fig. 4d. Interestingly, the anomalous flow is easterly in CESM-LME (Fig. 4d) but westerly in 20CR (Fig. 4e) over the Arabian Sea. Could the authors comment on this discrepancy?

6) **Waning of the monsoon-desert mechanism after volcanic eruptions**

i) L213-215 & Fig. 5: Contours and their units are not explained in the caption. Shades of blue in the contours appear to represent ascending motions and have negative values as expected (units Pa/s). But, unless I miss something, anomalies are expressed in units (-Pa/s), which is confusing.

ii) L215-217: The monsoon desert mechanism is not thought (by the studies mentioned here) to represent a closed circulation or overturning circulation (Walker-type circulation) with ascending motions over south Asia and descending motions over the EMed. If anything, Rodwell and Hoskins (1996) and Tyrlis et al (2013) present evidence corroborating the notion that the monsoon induces a zonal asymmetry that interacts with the mid-latitude westerlies resulting in enhanced subsidence over the EMed. See for example the discussion in pgs 1396-1397 in Rodwell and Hoskins (1996).

iii) L223-224: "as perhaps expected from the strongest decline in the Etesian winds". This inference is not clear to me. How is the decline in the Etesians related to the anomalies in ascent and descent over India and the EMed?

iv) L229-230: Actually there is no clear reduction of ascending motions over the "box region" in Fig. 5e. I can see a blue area over continental India & Bay of Bengal and a red area further to the south.

v) L237-241 & Fig. 6: Please consider carefully the description of units. In the caption units are referred to as (-Pa/s) while in the label of the horizontal axis as (Pa/s).

vi) L234-234: “Moreover, we identify an almost linear relationship between changes in ISM strength and NED anomalies”. I am looking for a punch line in this paragraph describing the results shown in Fig. 6. Is it that there is a linear relationship between ISM strength and NED anomalies or that stronger volcanic eruptions can produce a stronger decline in the frequency Etesians? To put in another way, Fig. 6 is composed of years when volcanic eruptions occurred and it does not describe the climatological strength of the monsoon-desert mechanism, as inferred by the above sentence.

7) L245-247: Here the authors infer that cooling over the EMed is due to reduced adiabatic heating. Although that someone would expect that a reduction in the subsidence over the region would lead to a reduction of the adiabatic warming, evidence about this is not provided in any of the figures. Interestingly, in the Introduction (L33-34), it is mentioned that the Etesians have a cooling effect over the EMed and someone could expect that their decline would be associated with surface warming and not surface cooling, as depicted in Fig. 4a-c. More detailed analysis is required before inferences, such as the above, are reached.

8) L251-253: I do not agree that volcanic eruptions have a trivial effect on SLP over the Balkans. Figs 4a-c suggest that negative SLP anomalies prevail over Europe and expand towards the Balkans. This anomaly contributes to the weakening of the pressure gradient over the EMed. Thus, the decline of the Etesians is not caused only because the thermal low over the Middle East becomes weaker.

9) L18-20, L72 & L279-280: Could the authors comment on the dynamics that cause the weakening of the “Anatolian low”? As mentioned above, it is evident from Figs 4a-c that actually the impact of the volcanic eruption is the appearance of a dipole of SLP, with negative SLP anomaly over Europe and positive SLP over the Middle East.

Minor comments

L12-14: This is a long sentence and a bit difficult to understand, please rewrite.

L18: Late summer months?

L18-20: This is a long and complicated sentence. As mentioned above, I find it difficult to understand the dynamical link between the weakening of the monsoon-desert mechanism and the weakening of the Anatolian low.

L26: What does “Balkan high” refer to?

L27-29: Something is missing in this sentence. Please rephrase.

L30: What do the authors mean by “as they are synchronized with the summer monsoon”? With the Indian summer monsoon?

L30-31: What do the authors mean here by “synoptic system”?

L32-33: It is not clear how the Etesians are “amplified by a large-scale subsidence established in summer months under the influence of the Indian and Asian summer monsoon”. Please clarify.

L37: “increased atmospheric blocking activity over Europe”

L42-43: What do the authors imply by “vice versa”? I think that this sentence could be removed.

L75-77: This is a confusing sentence. Please rewrite.

L91: Does “1” correspond to a footnote?

L119-122: Long and confusing sentence. Please rewrite.

L131: What is the meaning of “muted” and “punctuated” here?

L155: “In addition” - > “For completeness, “

L230: “in negligible” - > “is negligible”

L259: Do you mean here “anomalous temperature gradients”? Please clarify.

L261-262: I might be wrong. But isn’t Pinatubo a volcano in the tropical region?

L271: “positive?”?

Caption of Fig. 1: “volcaninc”->”volcanic”

Caption of Fig. 2: Please add explanations for orange and red lines.

L12, 19, 23, 25, 44, 56, 73, 120 and possibly elsewhere: It may be correct to write “over the eastern Mediterranean” or “over the central Aegean”.

L12 and elsewhere: It may be better to replace “Etesian winds” with “Etesians”.