Review of Misios et al. *"*Decline of Etesian winds after large volcanic eruptions in the last millennium*".*

This is an interesting and novel paper. I thank authors for doing this work and the editor for giving me opportunity to review it. Misios et al., using simulations from CESM-LME, show decline in strength and occurrence of summer Etesian winds following explosive volcanic eruptions. Such understanding will help improve weather predictions in the region. They show that warmer summer over India following eruptions weakened ISM that eventually declined Etesian winds. I have one major theoretical concern on their results on temperature anomalies post eruptions and how that would affect ISM. Please see my comments below.

General Comments

We would like to thank both reviewers for their thoughtful, encouraging, and constructive comments. Reviewer 1 provided a very detailed list of comments, emphasizing the mechanism and the relation to the ISM. Reviewer 2 acknowledges the implications of our work in improving seasonal prediction of the summer circulation in the Mediterranean region and expressed concerns on the modelled effects in the ISM region. Our reply to Reviewer 2 demonstrates that a) the summer warming over India is a robust feature in the CESM-LME simulations for the strongest eruptions at least. This evidence is further supported by independent model studies. 20CR also demonstrates a summer warming over India after Pinatubo. We also demonstrate that this is a summer season affect, as speculated; CESM-LME shows a cooling in the post-eruption winter, consistent with previous studies.

Key changes in the revised manuscript

- We include a new Figure 1. The figure numbering in the original version thus has increased by one. The new figure 1 is supplementary Figure 1, with the addition of displaying the climatology of SLP and surface winds in JA for the CESM-LME, as recommended by Reviewer 2.
- As recommended by Reviewer 1, in Figure 2 (old Figure 1) we have merged all-forcing and volcanic forcing times series. Hopefully, this simplifies the reading and improves the clarity of the manuscript.
- We have updated the discussion of Figure 5 (old figure 4). We have added a new supplementary figure 2 where we present surface temperature anomalies after subtracting the zonal mean value. This emphasizes the dynamic response of surface temperature anomalies. In the EMed, the amplified cooling is related to the reduced descending motion (as manifested by Omega velocity anomalies in Figure 6), and hence less adiabatic heating.
- Reversed color scaling is used in Figure 6 to be consistent with the units of Omega velocity (-Pa/s).
- New Figure 7 is updated with new Omega velocity units (-Pa/s).

Below, we reply to each comment and describe the changes to the manuscript resulting from them. (*new or revised text in italics*)

Major concerns

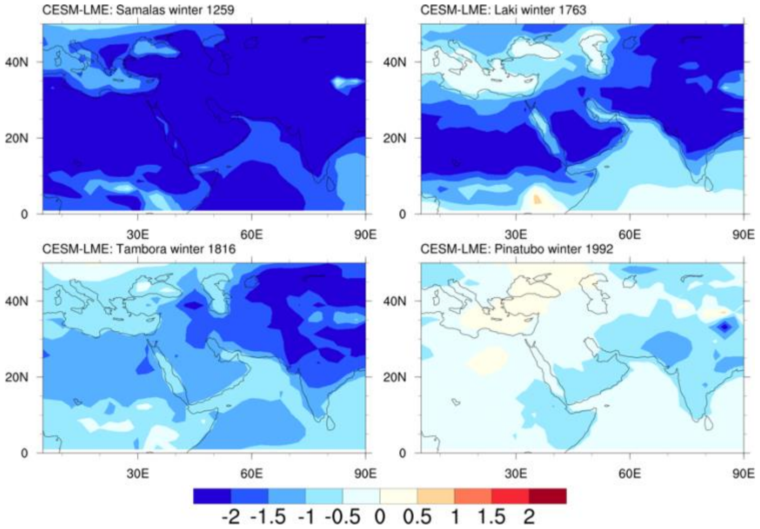
Figures 4 and 5

I am surprised to see positive summer temperatures following a few eruptions over India. In Stevenson et al., 2016 (Figure 3), using CESM, they show negative tmp anomalies in winter for 1 and 2 years post eruptions when plotting for entire Tropical and Northern eruptions. In my experience (not published), also see negative anomalies in India for spring season as well for tropical and norther eruption composites. I have not looked at CESM tmp anomalies for summer season, so maybe it is a summer thing or maybe some eruptions have such effect.

If this is not too much work, I would like to see summer temperature anomaly plots for n=0, 1 and 2 years post eruption for each eruption and also a composite plot by eruption type (all Tropical and all Northern eruptions).

Reply: Yes, indeed Stevenson et al paper shows negative temperature anomalies over India but for the winter months, as the reviewer correctly mentions. Our analysis focuses on the summer months, and this explains the difference. To start with, the independent work of Dogar and Sato (2019) shows positive temperature anomalies in the summer season both in an observational product and high-resolution simulations with a different model (see their figure 1). The contrast between the DJF and JJA temperature anomalies over India is also implied (India is marginally covered) in figure 3 in Dogar, Stenchikov et al. (2017), with warming anomalies in the summer season (cooling in winter) in year +2 after the El Chichon and Pinatubo eruptions. This suggest that temperature anomalies over India are strongly dependent on the season, and a possible explanation is found in the summer cloud cover anomalies. A reduced cloud cover in summer related to a weakened ISM and an increased direct SW heating should warm the surface.

Prompted by the comment, we have calculated winter (DJF) temperature anomalies in the first post eruption year after Samalas, Laki Tambora and Pinatubo (January gives the year). This figure should be compared with (new) Figure 5. We find cooling anomalies over India as suggested by the reviewer. This confirms our argument that a warmer India is a summer-time feature.



Reply Fig. 1 Anomalies of surface temperature (K, shaded), 10m winds (m/s, arrows) and SLP (Pa, contours) in the post-eruption winters (DJF) following Samalas, Laki, Tambora and Pinatubo.

Are these anomalies in temperature, wind, and velocity for the year of eruption, or 1 or 2 years post eruptions? I think the authors say first summer post eruption, suggest making it clear in caption as well.

Reply: These anomalies refer to the year of the maximum decline of etesian winds. For the tropical volcanoes, this is +1 year while for the high-latitude volcanoes this is at the eruption year. We suspect that the rather similar time lags for every tropical and high-latitude eruption might be related to the way the volcanic forcing is implemented in the model. This is commented in Section 4 *"Eruptions of unknown dates are assumed to begin in April and peak in June-July, a simplification that leads to very similar time evolution of the volcanic forcing (Stevenson, Brady et al. 2016). This might lead to an overestimation of the volcanic forcing in the summer months for some of the unknown eruptions."*

Furthermore, warmer temperatures in Indian land compared to the Indian Ocean region (as seen in Figure 4), would increase land-sea thermal gradient (that is increase ocean-to-land pressure gradient) and that is supposed enhance southwest ISM winds and rainfall. See, Ramesh and Goswami (2007) that show warmer ocean than land in past decades reduced the land-sea thermal gradient that declined the Indian monsoon rainfall. What is authors' take on this fact?

Ramesh and Goswami, 2007. Reduction in temporal and spatial extent of the Indian summer monsoon. *Geophysical Research Letters.*

Reply: We think the key mechanisms are those described in Stevenson et al. The model simulates a cyclonic wind pattern over the Arabian sea which weakens the Somali jet. Moreover, the model shows a tendency for positive temperature anomalies in the Equatorial Pacific suggesting a response similar to Positive ENSO. This should impact the Walker circulation by reducing the upward motions over Indian and Southern Asia. This is the mechanism proposed by Kumar, Rajagopalan et al. (1999) Reduced cloud cover and increased SW heating should result in the surface warming evident in post-eruption summers as simulated with the CESM-LME.

Other minor comments

Introduction

I very much liked the technical content of the Introduction. But I think it would be nice to have a short paragraph on the value of Etesian winds after its Introduction in first paragraph. So a second paragraph on: what changes in climate conditions we see due to changes in winds, and what regions and who (people) are affected by those changes, what are its socioeconomic effects with specific examples, and ending the paragraph with a note on why we need to study this wind system.

The authors give some hints on socioeconomic impacts, but do not provide specific examples at the end of first paragraph. Similarly, at the beginning of third paragraph, they give only mentioned the region is a climate change hotspot, but then quickly move forward on what they plan to do.

I would recommend making one separate paragraph on discussing the societal impacts of changes in Etesian winds. The authors can condense the last two paragraphs to make one to cover the increase in words in adding a new paragraph.

Reply: This is a very useful comment. Indeed, Etesian winds are, as we mention in the last paragraph of the Summary and conclusions, *"important for ecosystem services, wildfire prevention, air quality forecasts, tourism, energy production and economic development (Athanasopoulou, Protonotariou et al. 2015, Dafka, Toreti et al. 2018)*". The suggestion of the Reviewer, however, requires a dedicated paragraph which we fear will defer readers from the main focus of the paper. We have added one more reference at the very end of the first paragraph of introduction and we encourage the interested reader to find many additional studies in those papers "*thus regulating summertime conditions and affecting several environmental and socio-economic sectors (e.g. Athanasopoulou, Protonotariou et al. 2015, Dafka, Toreti et al. 2018 and references therein).* "

Line 39-41 I would recommend against the use of word 'thought' when making statement based on published work that have proven the fact stated.

Reply: Now it reads "*… and is amplified by …*"

Line 53-54 The mismatch exist maybe because the ISM has actually declined in the past few decades (Ramesh and Goswami, 2007; Kumar et al., 2020)

Ramesh and Goswami, 2007. Reduction in temporal and spatial extent of the Indian summer monsoon. *Geophysical Research Letters.*

Kumar et al., 2020. Recent unprecedented weakening of Indian summer monsoon in warming environment. *Theoretical and Applied Climatology.*

Reply: Thank you for this comment. We have changed the sentence to *"Such positive trends in model simulations, however, are not supported by observations of Etesians over the past decades (Poupkou, Zanis et al. 2011, Rizou, Flocas et al. 2018) and negative trends in Etesians could be associated with a weakening of the ISM in the past few decades (Kumar, Naidu et al. 2020)."*

Section 2.2 (labeled as 2.1)

Can the authors add a sentence on how 'typical (common)' is that the wind is strongest at the selected grid of study (37.5N, 25.E)? Do the authors mean, observations show strongest winds at this particular location over certain period? I would recommend to be specific when the author say 'typical'.

Reply: We mean that the strongest wind in this location is found in July and August. We have rephrased the sentence "*so as to select the months typically demonstrating the strongest wind speeds under the influence of monsoon convection over northern India (Tyrlis and Lelieveld 2013)*"

And, maybe it is more appropriate to discuss model performance compared to observations/ reanalysis in the first section under Results.

Reply: We have included a new Figure 1 (essentially the old Supplementary Figure 1) and some additional discussion in Section 2.2 to address this suggestion. The model performance in relation to ENSO variability is discussed in Section 4, "*The CESM model also suffers from over-active ENSO variations compared to the observations, which need to be considered when disentangling direct volcanic effects and ENSO.*"

Line 131-132 Can you support your statement by doing the significance test for the trend over the past century? This result, positive trend in NED and WSP over the 20th century, does not align with monsoon behavior, which is declining over the past several decades (Kumar et al., 2020). I am sure this study is focused on volcanic forcing and not long-term trends, but still it makes sense to add a sentence on this discrepancy.

Reply: A very valid point. We have tested the significance of the trends using the non-parametric Mann-Kendall test (implemented in ncl language as trend_manken function). Over the 1800-2005 period, we find a Theil-Sen estimate of linear trend in the NED for all-forcing CESM-LME runs of 0.018 days/year (p=1), while over the 1900-2005 period it is 0.025 days/year (p=0.99). In contrast, the volcanic-only ensemble shows no trends for both periods (0 days/year). This means that the CESM-LME supports our statement about "*an increasing number of Etesian days and WSP in the all-forcing simulation*".

However, our choice of presenting all-forcing and volcanic-only forcing time series in Figure 1 separately might add some level of confusion to the readers, as pointed out by Reviewer 1. So, in the revised text we decided to merge all-forcing and volcanic-only forcing (black and cyan lines in old figure 1) in a single line representing a grant ensemble of 17 realizations. This is valid given the focus of the paper on volcanoes and not on trends. For this reason, the sentences "*Periods of muted interannual variability are punctuated by periods of enhanced activity, with some evidence for positive trends over the last century, suggestive of an increasing number of Etesian days and WSP in the all-forcing simulation. This is consistent with independent model simulations of future greenhouse gas emissions resulting in higher WSPs and NEDs.*" are modified and moved to Section 4: Discussion and Summary.

Line 195 zonal mean temperatures subtracted from what? You mean subtracted from temperature 5 years prior to eruption?

Reply: No, at the chosen year after the eruption we remove the zonal mean value. This comment motivated us to change the text accordingly to facilitate an easier reading. In the revised version, we present temperature anomalies without subtracting zonal mean. Additionally, we have added a new supplementary figure 2, where we present surface temperature anomalies after subtracting the zonal mean value.

Conclusion I like the way the authors have not only summarized their findings, but concluded with so what do we learn now which we did not know prior to this work.

Reply: Thank you.

References

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