

Atmos. Chem. Phys. Discuss., author comment AC1  
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## Reply on RC1

Mukesh Rai et al.

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Author comment on "Insight into seasonal aerosol concentrations, meteorological influence, and transport over the Pan-Third Pole region using multi-sensors satellite and model simulation" by Mukesh Rai et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-199-AC1>, 2022

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1) One would expect, from the title of the manuscript, that the authors had conducted research on the Tibetan Plateau and Himalayas region in addition to the outer PTP region. However, they have only some information about India and China. So this is an unnecessarily misleading part of the title, abstract and even introduction of this study; this should be avoided.

Response: Thank you for the comments. We agreed and made necessary changes and add more information (I353-368).

2) I'm not sure how authors can make a very crucial assertion like AOD values are maximum across IGP when two models and two observations produce four findings. Despite the fact that WRF-Chem shows that northern China has higher AOD values than IGP, the other three choices show that the signal endures between seasons. As a result, such a remark in the abstract is not appreciable but also misleads readers.

Response: We agree. Taking this notion in mind that we have modified the misleading statements. Instead of just using IGP and East China, we have added other regions where AOD endures throughout the simulation period in (I42-43). We also added more information in the result section (I363-368).

3) In this study, the authors emphasize aerosol concentration over IGP. If a similar quantity of aerosol concentration is evident above China, however, it is rarely emphasized. This is not a scientifically sound practice.

Response: Thank you for highlighting this issue. We tried our best to avoid such discrepancies. Now we have modified this section (I353-362).

4) A large drop in PM<sub>2.5</sub> is seen in major Indian cities, according to Sing et al 2021 (10.1016/j.scitotenv.2020.141461). However, the authors make no mention of this in the manuscript. The recent downturn in China, on the other hand, is explicitly acknowledged.

Response: Thank you for recommending this paper. We have included the finding from this paper in our manuscript (I112-114).

5) I am not sure if there is anything unknown to unravel the effect of meteorological conditions on the spatiotemporal distribution of aerosol over the Himalayas. Many studies have already documented these factors in these regions. Furthermore, they stated in each part that the results are comparable to past studies. So, it's evident that they're simply reinventing something that's been done before. Any such exaggeration should be avoided.

Response: Our study region comprises dynamic geophysical features including arid, semi-arid, and mountain to flat land. Previous studies either focus on the Southern flank of the Himalayas, the Tibetan Plateau, or South East Asia. Thus, we attempt to showcase how different meteorological parameters play a role in aerosol concentration distribution and transport mechanism in the synoptic scale which owns unique geophysical features.

6) Again, I'm not sure why the authors place so much focus on the Third Pole location. Seasonal Spatio-temporal fluctuation of aerosols and total AOD is just discernible over IGP or China in the signals from Figures 4 and 5. As a result, neither the title nor the abstract should be expected to reveal this.

Response: We thank the reviewer for pointing out the inconsistency in the result presentation of our manuscript. We sought irregularity in our manuscript and presented more results. We added more findings and discussed more in detail other than the Third Pole region (I351-357).

7) If the model has a dust aerosol bias, which is indicated as being resolved by masking, how can authors assure that this is not causing problems in surrounding regions through transport?

Response: Thank you for highlighting this issue. In line (I347), our choice of the word "mask out" was meant to be that our model did not mimic the general feature of AOD values over arid/semi-arid than the reanalysis dataset.

8) Due to the low-quality figure in figure 6, the circulation pattern is difficult to interpret.

Response: Thank you. Now, figure 6 quality is improved.

9) If authors are perfectly aware that the bias comes from the chosen domain, why do they continue to use it? It appears that these are unsubstantiated claims with no scientific basis. The conclusion is littered with ambiguous assertions that should be avoided.

Response: We appreciate the concern raised by the reviewer which is valid. The previous study is largely from South Asia and East Asia. Our motivation is to present the synoptic scale picture of aerosol distribution and transport dynamics over geographically complex regions. Yes, we are aware that bias is there but we intend to use modelling as a tool and showcase the general features of the aerosols and aid more information over the region. This study underscores the bias correction which was obviously at hand but due to computational cost that could not perform. Further studies are needed to improve the model performance, especially over complex topography.

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2022-199/acp-2022-199-AC1-supplement.pdf>