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Comment on acp-2022-199

Anonymous Referee #1

Referee 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-RC1>, 2022

Review comments on ACP-2022-199

Insight into seasonal aerosol concentrations, meteorological influence, and transport over the Pan-Third Pole region using multi-sensors satellite and model simulation by Rai et al

Using observation, satellite and model outputs, the authors investigated the Spatio-temporal aerosol concentration, the influence of meteorological conditions, and underlying aerosol transport mechanisms over the Pan-Third Pole region. They demonstrated the seasonal distribution and spatial variability of aerosol across this wider domain and tried to explore model performance. Even though they have mentioned and highlighted the whole PTP region apart from IGP and China, there is nothing demonstrated in this study over this wide domain. At this level, there isn't enough scientific proof or knowledge enhancement to properly connect the underlying facts as well. With the following factors in mind, I feel the manuscript will be enhanced if the authors make the following changes and, more importantly, if they can eliminate numerous unnecessary discussions.

Recommendation: Major revision

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 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.

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.

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

4) A large drop in PM_{2.5} 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.

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 re-inventing something that's been done before. Any such exaggeration should be avoided.

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.

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?

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

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.