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Comment on acp-2022-199

Anonymous Referee #2

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-RC2>, 2022

The title, abstract, and introduction of the study "Insight into seasonal aerosol concentrations, meteorological influence, and transport over the Pan-Third Pole region using multi-sensors satellite and model simulation" by M. Rai et al. suggest an investigation of the seasonal properties and the origin of aerosol over the Pan-Third-Pole by means of satellite observations and model simulations. However, the study mainly presents seasonal means of PM_{2.5}, PM₁₀, and AOD at 10 AERONET stations and an unidentified number between 8 to 12 stations in China, a WRF simulation, CAMS data, MERRA-2 reanalysis, and VIIRS satellite measurements for a single year. While the motivation and envisaged scope of the manuscript would fit the scope of ACP the presented data and analyses do not. In my opinion this manuscript is just a collection of arbitrarily selected data. The data sets do not agree quantitatively, and in case of the WRF simulation not even qualitatively. Despite these discrepancies no attempt was made to assess the uncertainties of the simulation and measurement data to identify the source of the discrepancies.

Concerning the scientific quality I have numerous concerns:

In this study 3 different reanalyses were used: NCEP/FNL with WRF, MERRA-2, and GDAS with HYSPLIT. What was the motivation to do this? It is known that reanalyses differ. In my opinion using multiple reanalyses is a source for discrepancies. This should be taken into account when estimating the uncertainties and comparing the results/data sets.

The methods section (Section 2) is rather a description of the models used, but not of the data. Moreover, it is not clear how the models were set up and used and how the data was prepared. Concerning WRF, what is the purpose of the used models? Why have they been selected? Which parameters were used from the MERRA-2 data? What was the further

treatment" of the AERONET data? A description of the Chinese stations is completely missing. Figure 1 indicates 12 stations in China, but according to the manuscript only 8 stations were used (l 205). Which stations and why were four left out? I have the impression that the data and methods are not sufficiently described to reproduce the results.

Did the WRF simulation really produce negative PM_{2.5} concentrations? E.g. in Xi'an 66.5µg m⁻³ were measured, whereas the WRF simulation underestimated it by 93.6µg m⁻³ (ll 235-237). This should be clarified in the text.

Concerning the differences between the WRF simulation and the measurement the authors stated repeatedly some speculation (e.g. l243-246, 248-246). To investigate the effect of e.g. the meteorological data, a sensitivity test with a different reanalysis, which is obviously at hand, could be performed.

Figure 5 gives the impression that the WRF simulation does not perform well in reproducing the AOD. In winter and spring it underestimates the AOD and in summer and autumn it overestimates the AOD. In the text mainly the problems in simulating dust are discussed. However, given the large (a difference plot WRF-observations would be helpful) discrepancies between simulation and observations, I have little confidence in the further results derived from the WRF simulation.

I wonder why the seasonal pattern for PM_{2.5} and PM₁₀ with maximum concentrations in winter and minimum concentrations in summer (Fig 4, first and second column) is the opposite to AOD with the maximum AOD in summer and the minimum AOD in winter (Fig5, top row).

It is still not clear to me how the trajectory analysis was performed. What was the purpose of doing forward and backward trajectories? Where were the forward and backward trajectories started?

While the title suggests that the focus of this study is put on the Pan-Third-Pole this study mainly presents and discusses the high aerosol load in densely populated regions (Indo-Gangetic Plain and East China) known for strong aerosol sources. The possible impacts of aerosol on the Pan-Third-Pole are stated in the introduction, but throughout the manuscript the authors avoid presenting the current knowledge on the sources and transport pathways and comparing it with their findings. They rather name studies (e.g. ll460 - 464; ll467-469) and remain vague. In my opinion the authors should contrast the state-of-the-art knowledge with their findings to make the advancements in their study clear.

Finally the presentation quality is poor. First of all most figures are too small. In most

figures the axis labels, labels, and even data points are barely visible (even when zooming in to 200 %). The manuscript was difficult to read due to the numerous unnecessary abbreviations, some of them were not even introduced (e.g. AP, PG, CA, TD), others not used consistently (e.g. SA & South Asia), others introduced multiple times (e.g. VIIRS). It was also difficult to follow the line of argumentation while working through ``authors XYZ stated that'' at the beginning of consecutive sentences (e.g. ll438-444,). Moreover there are many avoidable mistakes (e.g. Fig. 1 ``dark blue shade'' is actually black, ll 209-212 the latitude and longitude of the stations is swapped). Finally the language certainly needs thorough revision.

In my opinion, fixing the above mentioned severe aspects is more than a major revision. Hence I suggest to reject this manuscript.