Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-471-RC1, 2021
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Comment on acp-2021-471

Anonymous Referee #1

Referee comment on "Aerosol responses to precipitation along North American air trajectories arriving at Bermuda" by Hossein Dadashazar et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-471-RC1, 2021

Summary

Hossein Dadashazar et al. investigated the impact of precipitation on aerosol particles along the airmass trajectories. Specifically, they investigated observed data from Bermuda locating in the North Atlantic Ocean east from the U.S East Coast. They studied how the mass concentration and volume size distributions of particulate matter is affected by the precipitation. They show that trajectories coming from North America during wintertime had the highest accumulated precipitation causing large reductions in the PM2.5/ΔCO (defined as "PM with aerodynamic diameter less than 2.5μm normalized by the enhancement of carbon monoxide above background"). They observed that changes in PM2.5/ΔCO were most sensitive to accumulated precipitation up to 5mm. In addition, their study was accompanied with GEOS-Chem model simulations providing information on wet scavenging versus convective scavenging, and a recent case study from aircraft field campaign ACTIVATE.

Overall, the manuscript is very well written and structured, and the scientific methods are accurate. Studies concerning the scavenging of observed aerosols due to precipitation along airmass trajectories are sparse (Tunved et al., 2013, is the only one I am aware, and it has an arctic location), thus these types of studies are very much welcomed in our field. You could emphasize the sparseness of these studies (thus highlighting the need for yours) by referring to Tunved et al.

I do recommend this study for publication, and it is well in the scope of ACP. I have listed some questions and suggestions below.

Specific/minor comments:

PM2.5 normalization with ΔCO: Could you elaborate a bit more why this kind of normalization is useful/more suitable instead of using the PM2.5 data as it is? You have given references to other studies using this technique in lines 406-417 where you describe what you have done, but a short sentence justifying the technique is missing.

Table 1: In the caption you mention the GEOS-Chem data. This might be matter of opinion, but I would move those lines into the text as you have anyway only the observational data in the table.

I did not notice any information of the data coverage/missing data for the observations during the investigated time between 2015-2019. This information should be added to the supplement at least for readers to know e.g., if the representability of different seasons differs.

Section 2.1: You average the data into 6-hourly values. Do you use mean, medians, or something else? This was not mentioned.

Section 2.3: As you mention, you show first figures based on the 10-day back trajectories, but then limit the analysis to 4-day back trajectories. Why? I think some justification should be added. In Section 3.2 you shortly mention "to focus more on transport closer to Bermuda" but this still leaves an open question why to look those 10-day trajectories at all

Section 2.3.2: You use Haversine formula to calculate the distance matrix needed for the clustering. As you say, Haversine distance is calculated between two points in Earth. Are your final distances then based only to the endpoints of the trajectories or do you somehow average over the whole trajectory i.e., how is your final distance matrix constructed?

Line 230: there is a typo "GOES-Chem" which should read "GEOS-Chem"

Figure 4 and related text: You combined the 8 clusters into 2 to enhance statistics. This is now related to the data coverage which was not mentioned. With your current trajectories you should have approximately 4(trajectories per day)x365(days per year)x5(2015-19) = 7300 which would be well enough statistics for 8 clusters. However, I assume the observational data is not complete which reduces the number of data rows to be used in the precipitation analysis? You mention in Table 2 caption the table in SI has number of data points for each table entry, but it does not give general picture of the available data as it has the high-and low APT categories.

Figure 5 and 6: As your y-axis ends at zero, the figure looks like the lower end of the boxes is cut out. Maybe show a bit of the negative axis too (we all know there is no negative rain) not to have this effect?

Figures 6-8: It would be interesting to see how these differ for the cluster 2 as the airmasses have different characteristics, and as shown in Fig. 5, clearly lower APT too. These could be SI material.

Section 3.4: Why the GEOS-Chem simulations have only 2016-17 and not the same time period 2015-2019 as the observations? If I understand correctly, you did not do any “direct” comparisons (meaning having exactly the same time periods/simultaneous observations from GEOS-Chem and measurement data) between these datasets, is there a reason for that? Wouldn’t it be possible to output e.g., some aerosol concentrations from the model too? It seems so based on the description from section 2.5.

Figure 10 and related text: Why is the time over land excluded here?

Section 3.5: What is this “Min. Alt. Legs”, was it explained somewhere?

Line 707-708: What are the numerical values reported here? I am not 100% sure after reading the text.

Line 720: State again here what the abbreviation “APT” stands for to make the conclusions more independent.

Line 732: What do you mean here with the “large-scale precipitation”?

References

Tunved, P., Ström, J., and Krejci, R.: Arctic aerosol life cycle: linking aerosol size distributions observed between 2000 and 2010 with air mass transport and precipitation at Zeppelin station, Ny-Ålesund, Svalbard, Atmos. Chem. Phys., 13, 3643-3660, 10.5194/acp-13-3643-2013, 2013.