Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-503-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Amplification of South Asian haze by water vapour-aerosol interactions" *by* Vijayakumar Sivadasan Nair et al.

Anonymous Referee #2

Received and published: 6 August 2020

This paper uses a regional climate model, RegCM4, to assess the contribution of aerosol hygroscopic growth to the total aerosol optical depth and examines various effects resulted from the aerosol-water vapor interactions in South Asia in winter. They include the positive feedbacks between aerosol hygroscopic growth-surface cooling-PBL relative humidity, the drying effects in the free troposphere, and the worsening of air quality. These effects are assessed by a set of designed model experiments targeted to isolate the change of AOD, temperature, and RH due to the radiative forcing due to the aerosol hygroscopic growth and due to the meteorological feedback. Several interesting results are presented, including the change of AOD and surface radiation due to the aerosol hygroscopic growth and resulting change of surface temperature and relative humidity. It is also interesting to see the opposite trends of RH in the PBL

and in the free troposphere that might be explained, at least partially, by the feedbacks from the interactions between aerosol and water vapor.

Having said that, there are several major concerns regarding the quality of the paper. My major and specific comments are presented below. Therefore, I recommend a major revision of the paper before it can be published on ACP.

Major comments:

1. The paper is particularly focusing on the effects of aerosol hygroscopic growth on two meteorological variables, T and RH, which is fine. But attributing the amplification of the winter haze in South Asia to the aerosol-H2O interaction is rather narrow-minded, because there is another important factor, the effects of aerosol-radiation interactions due to the presence of absorbing aerosols, which is probably a more dominant factor in amplification of the South Asian haze but was not addressed at all in the manuscript. I suggest adding one more model experiment, that is to assume all aerosols are not absorbing (e.g., single scattering albedo =1, or omit black carbon), to address that issue. 2. Feedbacks: The only met fields dealt with in this paper are surface temperature and RH. Other important met fields, such as clouds, precipitation, winds, and PBL height, are omitted. The authors argue that this is because some previous studies have explored these fields. However, at least these other effects should be mentioned and summarized in the context of comparing the magnitude of various effects on haze and air quality. 3. The title is about the amplification of South Asian haze, but there is no assessment of the haze condition in the paper, for example, the change of visibility or PM2.5 concentrations. Since the change of AOD does not equal to the change of haze conditions, it is necessary to assess the air quality in more direct way than what was presented in the paper. 4. The analysis and explanations are often confusing. It is not clear what the findings are from the present study and what are from other studies, and how consistent or different they are.

Specific comments:

Line 10-11: “This positive feedback mechanism plays an important role in the prevalence of wintertime fog and low air quality conditions over South Asia” – this point has not been demonstrated in the paper.

Line 70-74: These two sentences read like that so far, “there are very limited efforts to understand the implications of aerosol hygroscopicity on regional air quality”, “in this study we use a regional climate model to assess the contribution of the aerosol hygroscopic growth to the total aerosol optical depth over the IGP”. If the purpose is to fill the gaps on very limited efforts on the effects of aerosol hygroscopicity on air quality, assessing the contribution to total AOD does not help much in that regard.

Line 76-76: “we quantify the effects and feedbacks of the aerosol hygroscopic growth on the low air quality conditions over the region”: This part is lacking. No quantification on such effects on air quality is specifically addressed in this paper.

Line 83-84: 50-km horizontal resolution and 18 vertical levels: This is a very coarse resolution regional model. What is the model top?

Line 89-90: Please spell out the acronyms and provide references of each scheme.

Line 113-114: The growth factor is 1.37 and 1.49 for hydrophilic BC and OC, respectively. What are the growth factors for sulfate and nitrate?

Line 120: “climate feedbacks” is used here loosely. For the limited seasonal study like this one, “meteorological feedback” is more appropriate.

Line 121-122: How the clouds are formed in RegCM without aerosol “feedbacks”? Does aerosol provide CCN for cloud formation?

Line 142: “RegCM simulates reasonably well. . .” This is a subjective statement. Howe well is reasonably well? How well is "reasonably well"? Within 20%? 50%? 100%? compared to what? Needs more quantitative description.

Line 146-147, Comparisons between RegCM simulated aerosols with measurements:

Does the simulation include the feedbacks? Figure 2 shows that the modeled aerosols are in general lower than observations taken in the earlier years. What does this mean considering the fast increase of pollutant concentrations and AOD (see intro section)? Does it mean the model underestimations would be even more severe if the comparisons are done for the same years?

Line 165: “A good agreement” – again, this is a subjective phrase. With the figure, you should be able to quantify the agreement, e.g., the model calculated RH is 5-20% higher than the observed values. Are these differences good enough for this study?

Line 171: another “reasonably well”. Fig. 2f shows that the differences between simulated and observed PBLH range from 600m lower and $\sim$200m higher than observations. Is this reasonable?

Line 185-186: “The mean observed (ambient, dry) AOD” – how to observe dry AOD?

Line 186, sites: It would be better to follow the orders of a, b, c, d in Figure 3, i.e., Jaipur, Kanpur, Pune, and Gandhi College.

Line 193, the statement of Jaipur not showing much hygroscopic growth: The ambient AOD (0.51) doubles the dry AOD (0.26) in Jaipur (see line 186). This is quite a lot growth, certainly is not “not much”.

Line 203: “matches well” – how well is well? Please give some quantitative measures, including correlation coef., bias, RMSE, for example.

Line 206-207: 30%. “since the emission fluxes of anthropogenic aerosols are seasonally invariant”: this is from the model or the actual facts in the real world?

Line 207-208: “most of the observed variability of AOD is due to the variability in relative humidity”: This is unjustified to attribute the variability to the RH, because you have not assessed the AOD variability due to the changes of winds, chemistry, deposition, and transport.

Line 213: What is the AODdry/AODambient ratio over W india? The contour lines started at 0.3 and ends at 0.6. I suggest show the full range of the ratio with the contour lines.

Line 210-228: This paragraph is somehow confusing – it mixes the results from this study with findings from other studies, and it is difficult to unravel the message. It should state what are the findings from this study and how they compared with previous studies.

Line 245: How much is the cloud cover change? This should be included in Figure 5.

Line 249: RH increases by 2-4% in eastern India. Is this insignificant? The student t-test shows that it is statistically significant. Please clarify what “significant” means.

Line 249-251, “Hence. . .” This statement is not evident from the paper. So far the figures and text only show the effects of aerosol hygroscopicity on RH and T, but not the pathways from land-atmosphere interactions.

Line 253-254, “In other words. . .” But why the effects in the eastern India and Bangladesh is much weaker than in the middle IGP where the dimming effect is the strongest (Fig 4)? Need some better explanation.

Line 261, “along with the weak entrainment of dry airmass from the free troposphere”: Where is the evidence? Does RegCM show the weakening of the entrainment? What is the cause of that?

Line 261-262, “leads to an increase in relative and specific humidity at the surface”: The paper only shows an increase of RH, not specific humidity.

Line 267-268, “Most reanalysis datasets have a negative bias in simulating RH during winter”: The "reanalysis" datasets are based on the observation that should include all effects, unless the observations in South Asia are not input into the reanalysis and the RH is solely from the underline model simulations. Please explain.

Line 272, “drying in the 1-3 km”: Fig 6 shows the RH increased by 3% at 1 km, not decreased, due to total aerosol effect.

Line 272-273, “warming of the top of the boundary layer”: Where is the top of PBL? Fig 6 shows a cooling, not warming, from surface to almost 2 km due to total aerosol effect.

Line 287: “surface temperature increases”: But you have been showing the temperature decreases, not increases. This is so confusing.

Line 287-288, increase in water vapour content. . .reported by Mukhopadhyay (2017)”: Does that study use the same model with the same settings for wintertime simulation? What is your finding in this study?

Line 346-347, “present study also showed poor association with ambient AOD and PM2.5 but higher correlation is observed between the dry AOD and PM2.5”: Where are the associations/correlations are shown in this paper?

Line 350, “the mass concentration is dominated by the contribution from the coarse mode aerosols”: But PM2.5 is not coarse mode aerosols!

Line 306-375, “section 3.3 Implications on air quality”: This entire section is very weak. There are no quantifications on the haze condition and/or PM2.5 concentration change due to the aerosol hygroscopic growth and meteorological feedbacks. Maps of change of air quality (haze and PM2.5), similar to Figure 5 for T and RH, are needed to illustrate the implications.

Line 383: The increase in PM2.5 along the IGP has not been unequivocally demonstrated.

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