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Geoscientific
Model Development
Discussions

Interactive comment on "Simulations over South Asia using the Weather Research and Forecasting model with Chemistry (WRF-Chem): set-up and meteorological evaluation" *by* R. Kumar et al.

R. Kumar et al.

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Reply to the Comments of Reviewer #1

We are thankful to the reviewer for his careful and thorough evaluation of the manuscript and the recommendation for publication. Below, we give a detailed response to each of the comments raised by the reviewer. Reviewer's comments are in regular font and replies are in bold font characters.

Comment

Model Setup:

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Interactive Discussion

Discussion Paper

I could not (yet) find the cited companion paper Kumar et al in ACPD and for clarification it might be worth extending the model setup description, particularly the treatment of aerosols which might directly affect meteorological parameters (Are aerosol radiative feedbacks treated explicitly? Are aerosol indirect effects treated explicitly? Is dust treated explicitly?).

Reply

We apologize for the confusion. The companion paper was submitted to ACPD but it was suggested by the topical editor that the paper is more suitable within the scope of GMD. Therefore, the companion paper is resubmitted to GMD and is now available in GMDD (http://www.geosci-model-dev-discuss.net/5/1/2012/gmdd-5-1-2012.pdf).

The aerosol module used here is based on the Modal Aerosol Dynamics Model for Europe/Secondary Organic Aerosol Model (MADE/SORGAM) (Ackermann et al., 1998; Schell et al., 2001). The feedback from the aerosols to the radiation scheme has been turned on in the simulation. Dust emissions are calculated online within the model using landscape and meteorological information. Now, this information has been added in the revised manuscript (page 9, 173-177).

Comment

India comprises various different climatological regions. For readers not so familiar with the geography of India, it might be helpful to mention that (e.g. location of deserts, regions of moist climate) in addition to showing the surface elevation in fig.1. This would further help the reader to follow your explanation of the spatial and temporal variability in temperature and water vapour (p. 3080, l. 14-17).

Reply

We thank the reviewer for this suggestion. We have added the following sentences to the revised manuscript in Section 2 (page 8, line 155-162): “The northern part of South Asia with the Himalaya Mountains has a montane and temperate climate, while

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the southern part is surrounded by oceans and experiences a mostly moist climate. The driest weather prevails in the Great Indian Thar Desert (∼24o-30o N; ∼70o-75o E) in western India. Forested regions are generally found at high altitude and high rainfall regions in northern, north-eastern and southern parts of India. Further details regarding the topographical features, land-use and classification patterns in India can be found on the website of the Ministry of Environment and Forests, Government of India (http://moef.nic.in/index.php)." The colour coding of Figure 1 has been changed and oceanic regions (Arabian Sea, Bay of Bengal and Indian Ocean) are named in the revised figure.

Comment

Figure 2, Text on p.3080 - The 2m Temperature shows a distinctive North-South gradient especially during winter but not during autumn. Can you comment on that? Or is the colour code just misleading?

Reply

There is also a small temperature gradient in autumn, but it is less than 5oK and given the colour scale does not show in the Figure. This gradient is more visible in Figure 4 illustrating AIRS and WRF temperature at 700 hPa. We mention this in the revised manuscript (page 17, line 350-351). Please also see the response to the comment below.

- You state that north of 20o; Temperature shows a stronger seasonal cycle than further south. Could you add 1 or 2 sentences why this is the case? Reply The stronger seasonal cycle north of 20o in temperature is associated with a stronger seasonal cycle in solar radiation. The seasonal amplitude of the modelled solar radiation at the surface over the regions north of 20o is estimated as 300-400 W m-2 while over the southern regions it is estimated as about 200-250 W m-2. Now, this has been mentioned in the revised manuscript (page 17, line 356-358).

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Comment

Figure 3, Text on p.3080 This section is rather descriptive. Could you add a few explanations for the predominance of certain wind directions? Mentioning the (predominant) synoptic situations during the different seasons would help the reader to see that the model is capable of simulating various different weather patterns correctly.

Reply

As suggested, a brief explanation for the predominance of wind directions has been added in the revised manuscript (page 18, line 367-379). The modified text is as follows: “During winter, surface temperatures over South Asian land-masses are lower than over the oceanic regions. This leads to the development of a high pressure area over land and a low pressure area over the ocean, causing a low level north-easterly air flow near the surface over most of the model domain. Over the Himalayan region, including the Tibetan Plateau, the wintertime wind patterns are generally south-westerly. During the transition from winter to spring, land regions warm up rapidly leading to the formation of heat lows over the subcontinent and cold highs over the oceanic regions. Thus, springtime near-surface winds are nearly zonal over the regions north of 20oN while winds are northerly over the Arabian Sea, and southerly over the Bay of Bengal. The continuous heating of land mass during spring leads to the development of the South Asian monsoon during early summer and south-westerly near-surface winds prevail during summer. Surface temperature again decreases over land from summer to autumn and consequently the winds again change to a north-easterly direction.”

Comment

Figure 5, Text on p. 3080, l.24 – p. 3081 l. 10 Looking at the scatter plot and especially at the frequency analysis of summer it appears as if the model and AIRS are a perfect match – in contrast to the index of agreement and the coefficient of determination (however, the figure is very small and difficult to read). I am a bit surprised by that as the frequency analysis for water vapour also shows a difference between WRF and

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AIRS in summer and the scatter for water vapour is large in summer than in winter. Is the reason that you see differences in the temperature only at 2 pressure levels? Following this, I did not really understand why the difference in temperature should be 'so large' only at those 2 levels (925 hPa and 500 hPa). Can you comment on that?

Reply

We are sorry for the confusion. The index of agreement (d) and coefficient of determination (r2) at 700 hPa, for which the scatter plots are shown, are greater than 0.85 as already mentioned in the manuscript. Largest differences between model and AIRS water vapour during summer are likely due to large spatial variability of water vapour associated with spatially varying influence of South Asian monsoon in this region and simulations of Indian summer monsoon are difficult due to its anomalous characteristics in the tropical circulation. Although r2 and d are smaller at 925 and 500 hPa, the differences in temperature as indicated by RMSE and MB are not very large and are comparable to those at other levels. Now, this part has been revised to provide suitable information (page 22, line 487-490).

Comment

p. 3084, l.22, 23: Why should the agreement be automatically worse if less samples are available (if you analyse only 'pairs' as explained in section 3.4)?

Reply

We tried to express that statistcally the larger sample sizes are expected to give a better estimate of the overall atmospheric state.

Comment

Evaluation of u, v wind: Have you thought about evaluating the wind direction and wind speed additionally with observational datasets, such as are available from radiosonde?

Reply

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Yes, we made an attempt to evaluate the wind speed and directions using radiosonde observations but unfortunately radiosonde data for winds were appearing to be erroneous. Although the magnitude of radiosonde winds were close (RMSE < 2 m s-1) to modeled winds but the variation in radiosonde winds was found to be in steps of about 1 m s-1 during all the months and such consistent stepwise (1 m s-1) variations cannot be attributed to natural atmospheric variability. Due to this reason, we did not have enough confidence in using this data for comparison with the model. Hence, we have not used the radiosonde wind data for the comparison.

Comment

Overestimation of summertime rainfall: I am fully aware that one can endlessly test different convection parameterisation schemes and none will give perfect results at the end. However, as you mention sensitivity studies: Did you perform tests with other convection schemes for summer? If yes, was precipitation always overestimated? Did you have any other particular reason for choosing the Kain-Fritsch scheme?

Reply

We conducted a ten day (10-20 July 2008) WRF-Chem run using the Grell-Devenyi (GD) convective scheme. With the GD scheme, we found a significant underestimate of TRMM precipitation during this period. The correlation coefficient between TRMM and WRF-Chem was found to be greater for the Kain-Fritsch (KF) (r = 0.67) scheme compared to the GD scheme (r = 0.26). Other statistical metrics including POD, FAR and frequency bias also indicated a better model performance for KF versus GD. Therefore, we chose to run the model with the KF scheme.

Comment

You mention the coarse model resolution as a potential error source for the overestimation of precipitation: Would an increase of the horizontal model resolution not most likely lead to an even stronger overestimation?

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Reply

The increase in model resolution improves the representation of geographical features such as topography, land-use, vegetation cover, surface albedo etc. The improvements in geographical features can improve the simulations of meteorology and rainfall. In fact, improvement in modeled rainfall distributions has been seen with finer spatial resolution in other studies (e.g. Li et al., 2008).

Comment

Section 4.6 needs either further explanation or skip the part referring to chemistry. I do not fully understand the differences in NOx: - At first I am not totally surprised to see the largest differences in NOx (e.g. shorter lifetime compared to CO and O3), direct local emissions with large spatial differences) - May be it is misleading to present the differences in Percent. The regions with maximum differences seem to be as well the regions with highest NOx emissions (e.g. the highly populated areas along the coast) so presumably absolute mixing ratios in those regions are high? - Do you use a lightning-NOx parameterisation? That should actually rather lead to higher column NOx when more precipitation is simulated (assuming that a large fraction of precipitation is convective precipitation and that several of these precipitation should be accompanied by lightning). - I also do not immediately understand why higher NOx should be correlated to reduced precipitation – NO and NO2 are (almost) not soluble so a reduced scavenging due to reduced precipitation cannot be an explanation. But maybe indirect effects of reduced scavenging? Changes in temperature and change in the NOx/NOy ratio? This certainly needs further explanation.

Reply

The main objective in Section 4.6 is to identify effects of meteorological errors on simulations of O3, CO and NOx mixing ratios. The results are shown as percentage difference to give an idea of the errors that can be induced by meteorological errors into the chemistry simulations. The spatial distributions of absolute values of ozone, CO and

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NOx are shown in the companion chemistry paper (Kumar et al., 2012). There are no changes in the anthropogenic emissions. In addition, these simulations do not include a lightning-NOx parameterization. Therefore, changes in NOx mixing ratios cannot be attributed to the lightning-NOx process. Instead, NOx changes are due to the indirect loss of HNO3 via wet scavenging. The reduction in precipitation results in a longer HNO3 lifetime allowing it to transform back to NOx via photolysis. This is confirmed by analysis of modeled spatial HNO3 distributions, which indicated strong increases in HNO3 in areas of reduced precipitation and enhanced NOx. These explanations have been added in the revised manuscript.

Comment

Figures The quality of some figures is not so good; specifically:

Figure 1: Could you change the colour code? I can hardly see a difference between 400 m and 1600 m, it all just looks pinkish to me. I have the same opinion about figure 13.

Reply

As suggested, we have changed the colour code in both Figures 1 and 13.

Comment

Figure 2, 4, 10: The plots are too small. It is very difficult to see, e.g. differences between the AIRS data and the simulated values. The labels referring to the colour bar are also too small, I can hardly read them.

Reply

Label sizes in all the Figures have been increased in the revised manuscript. Given the limited space, it is very difficult to increase figure sizes. But statistics of the comparison between AIRS and simulated data are also listed in tabulated data (Table 3-9).

Comment

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I also find Figures 5, 6, 8, 9 very small.

Reply

Now, we have increased the figure sizes as much as possible. As mentioned above, we cannot think of a way to increase the figure size further given the space constraints.

Comment

Caption of Figure 2: You write: 'For the case of precipitation, data above 1400 mm are not shown and regions shown by orange colour are implicitly having values above 1400 mm.' Isn't there a contradiction in the sentence?

Reply

Sorry for the confusion. We rephrased this sentence to: For precipitation the color scale is limited to 1400 mm, but actual rainfall amounts can exceed this limit.

Comment

The number of figures in this article is very large and at least Figure 15 could be deleted, from my point of view.

Reply

We agree with the reviewer that the number of figures is rather high, but would prefer to keep Figure 15 as it demonstrates well the impact meteorology has on chemical constituents at different altitudes in the atmosphere.

Comment

Rephrase p.3093, l.11, 12 (the grammar of this sentence is not correct)

Reply

Thank you for pointing this out. We revised the sentence.

References Li, J., Sorooshian, S., Higgins, W., Gao, X., Imam, B., Hsu, K.: Influence

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of Spatial Resolution on Diurnal Variability during the North American Monsoon, J. Climate, 21, 3697-3988, doi: http://dx.doi.org/10.1175/2008JCLI2022.1, 2008

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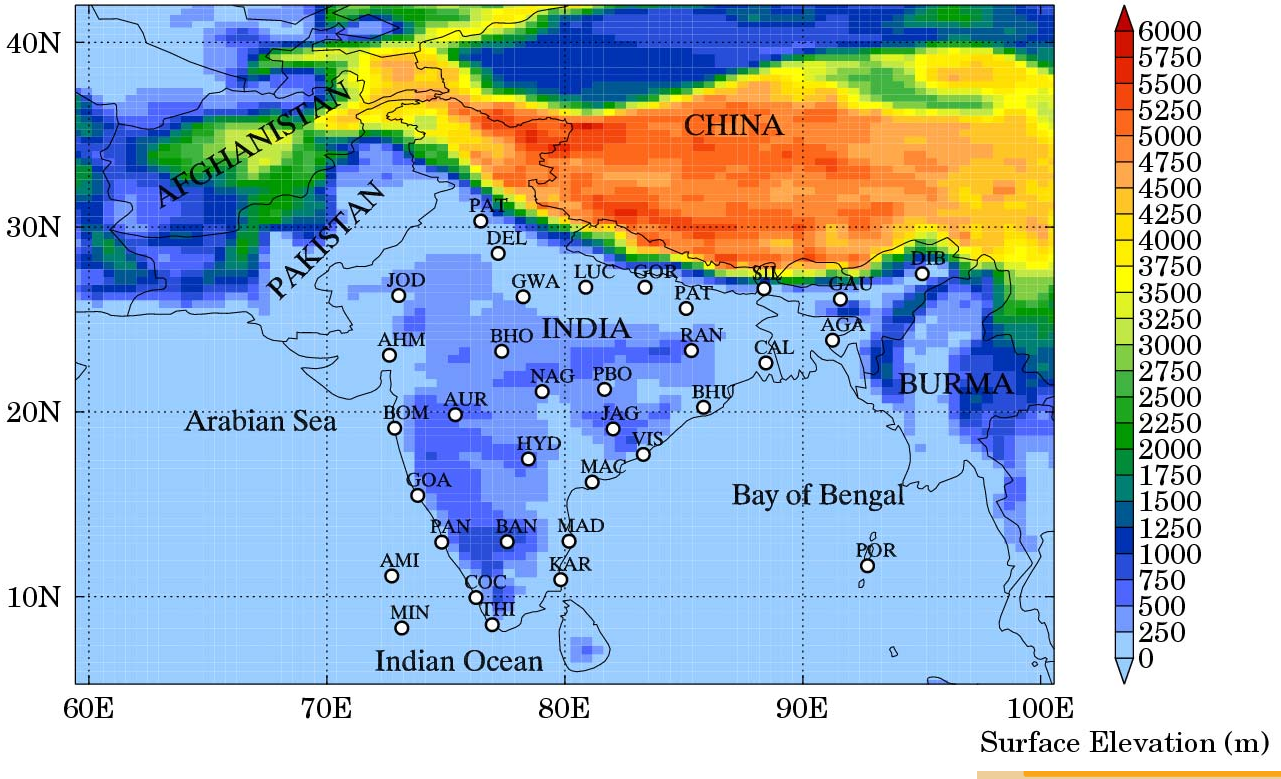

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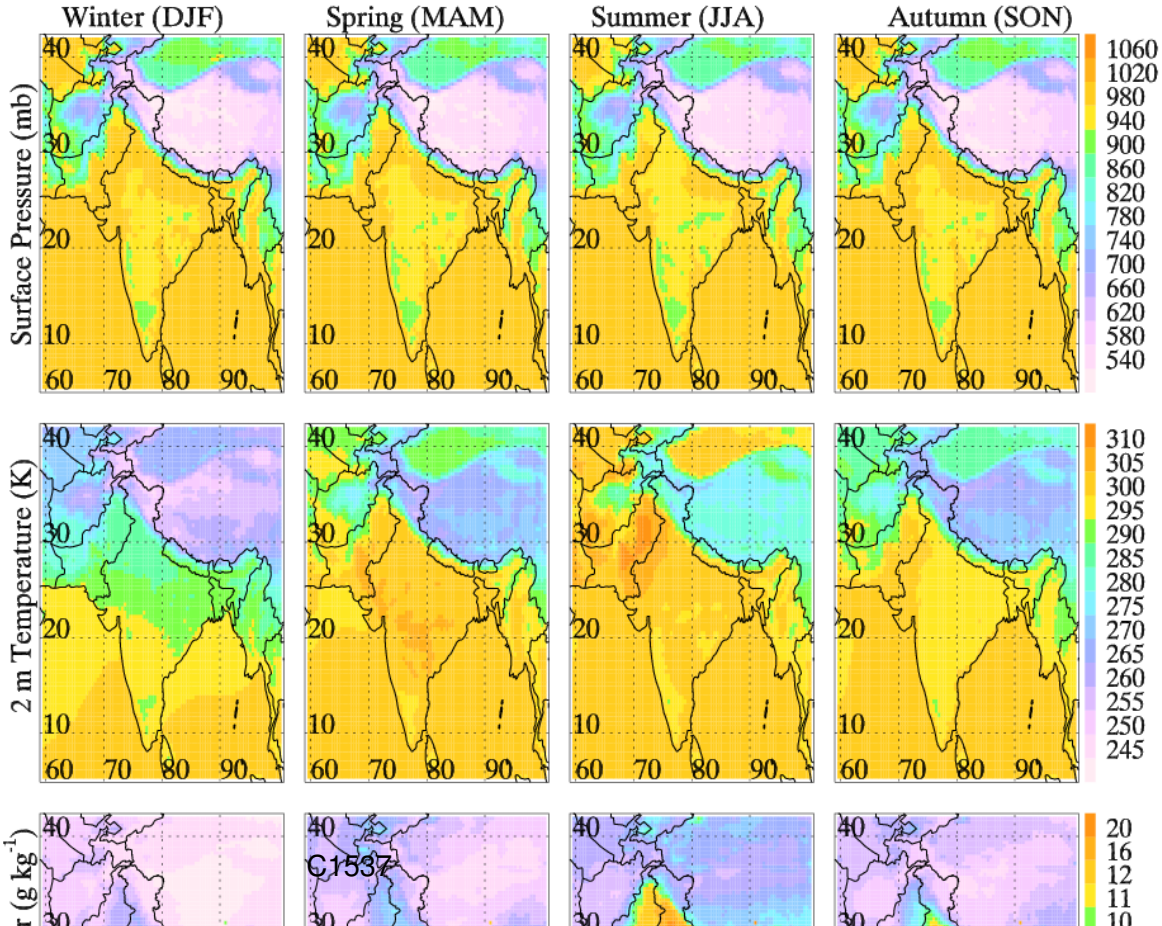

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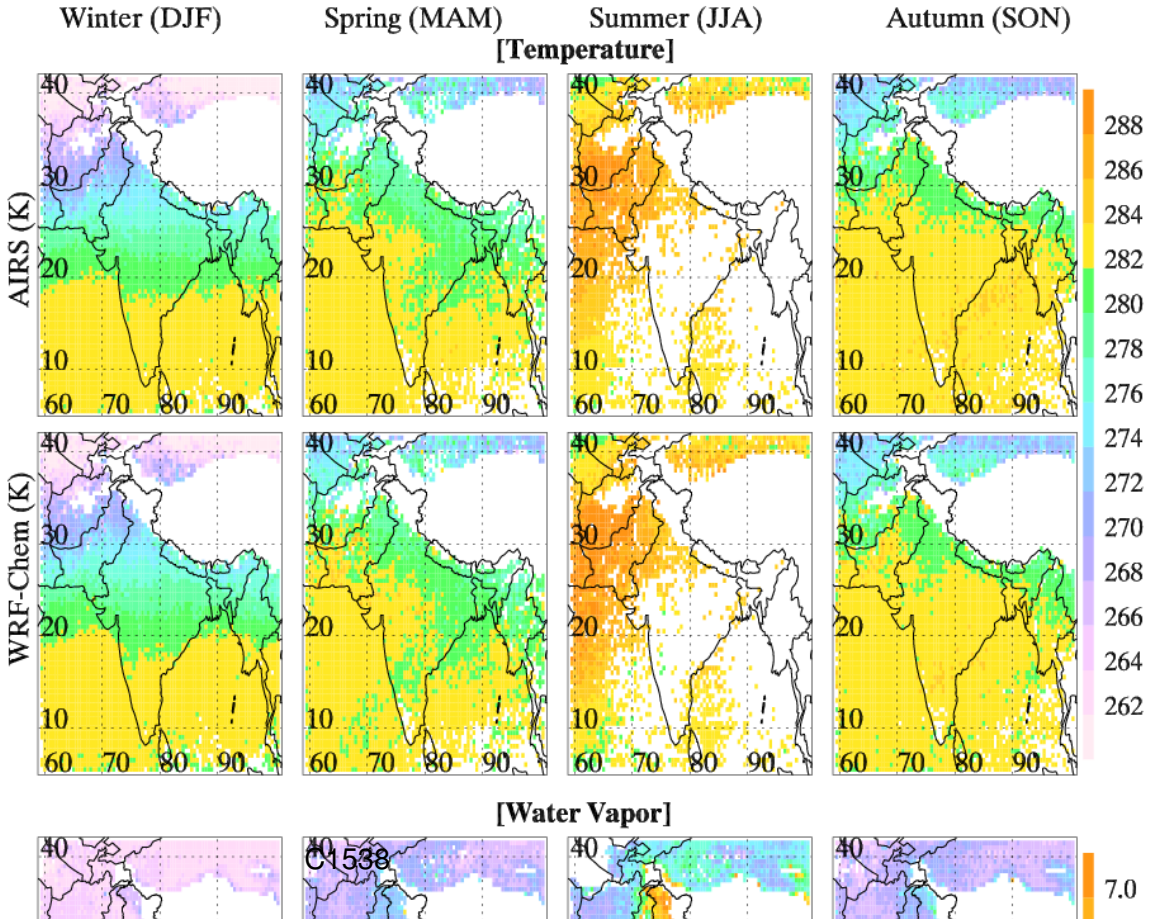

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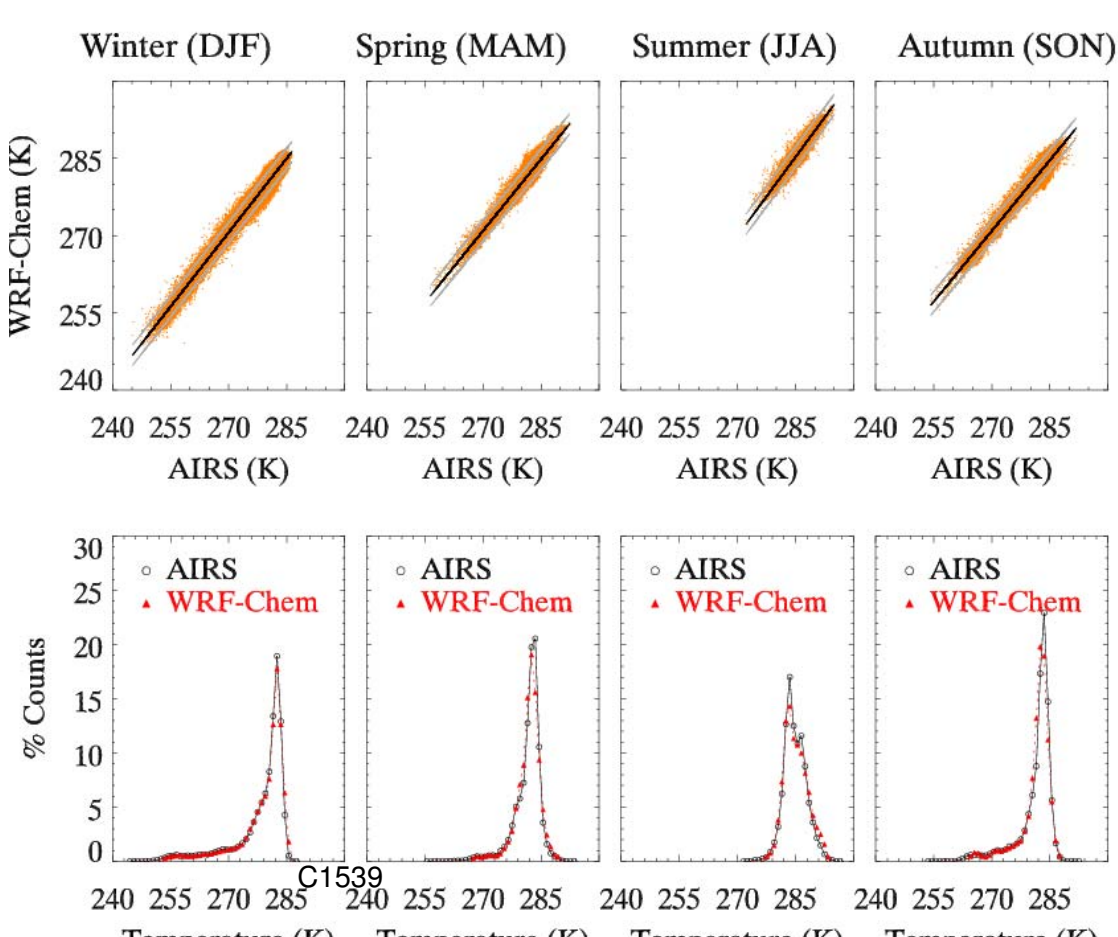

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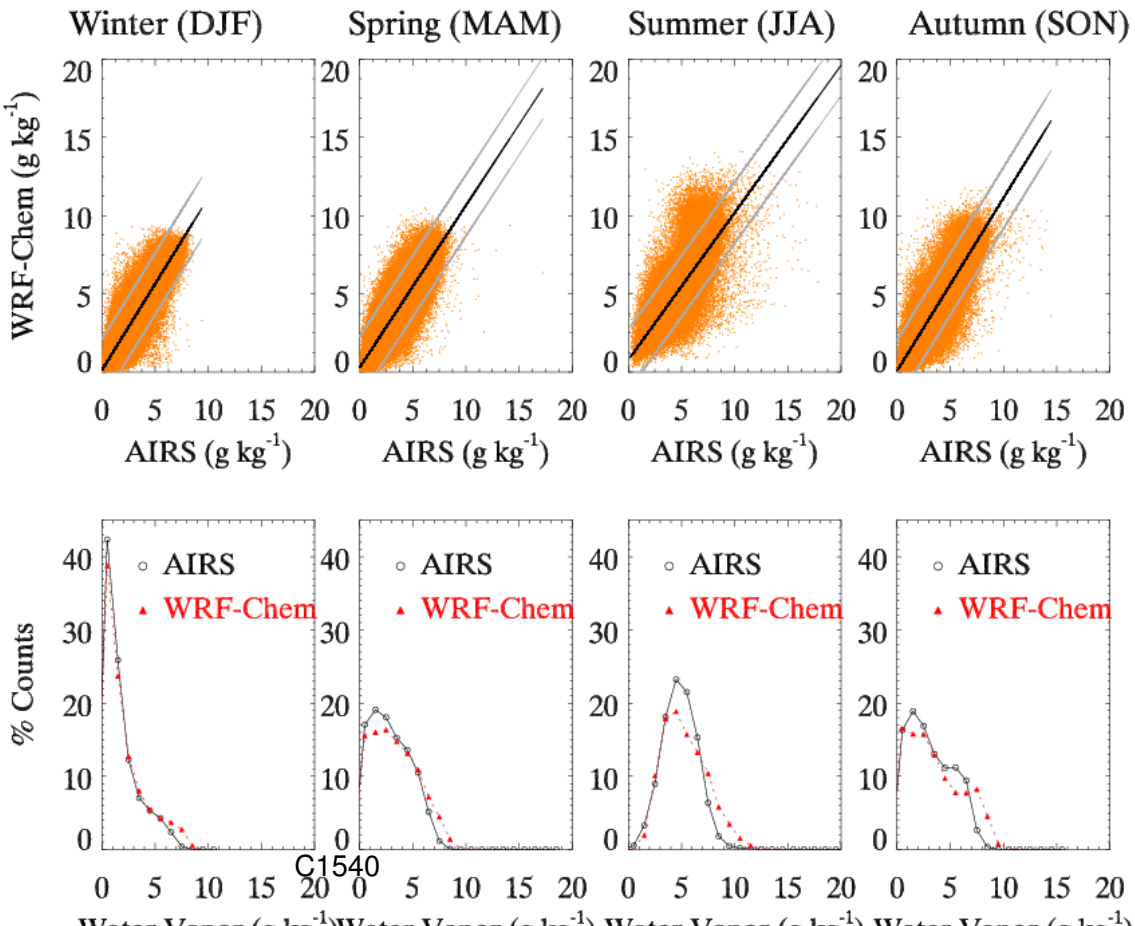

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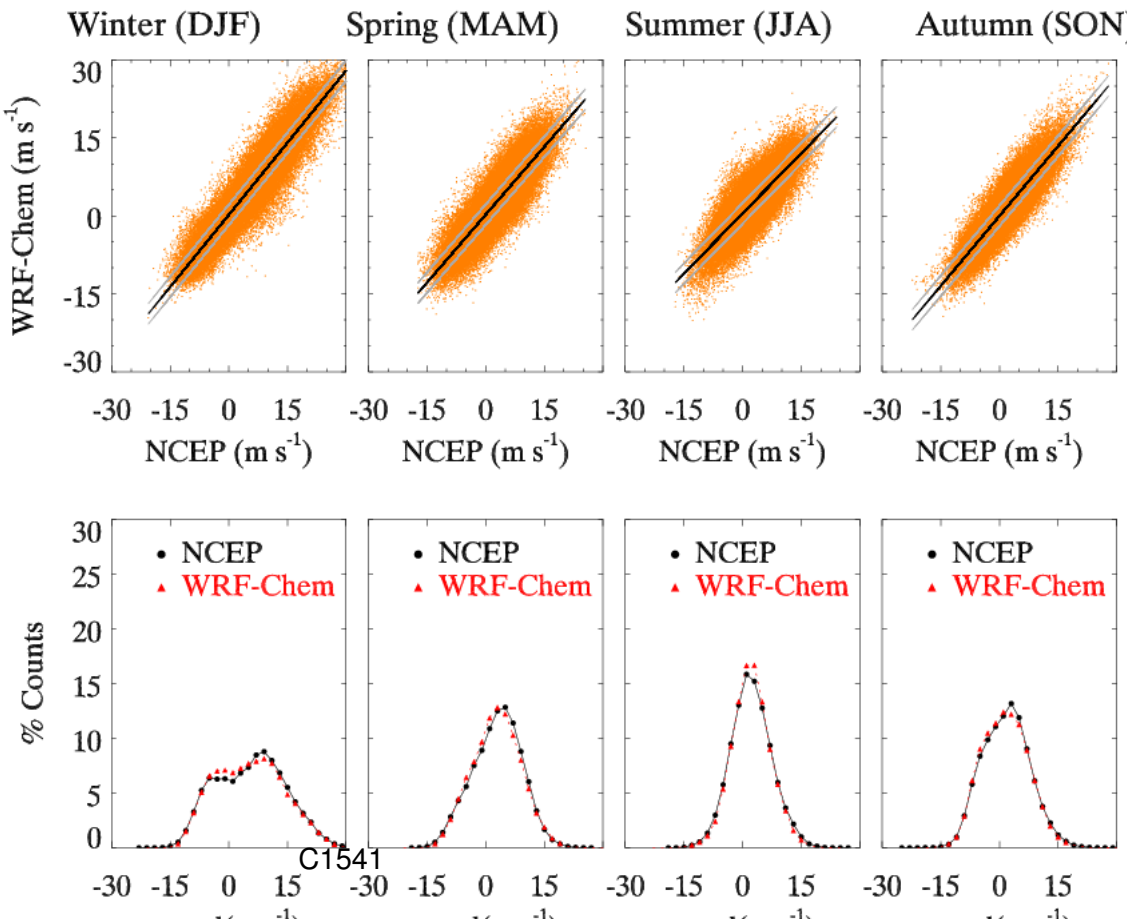

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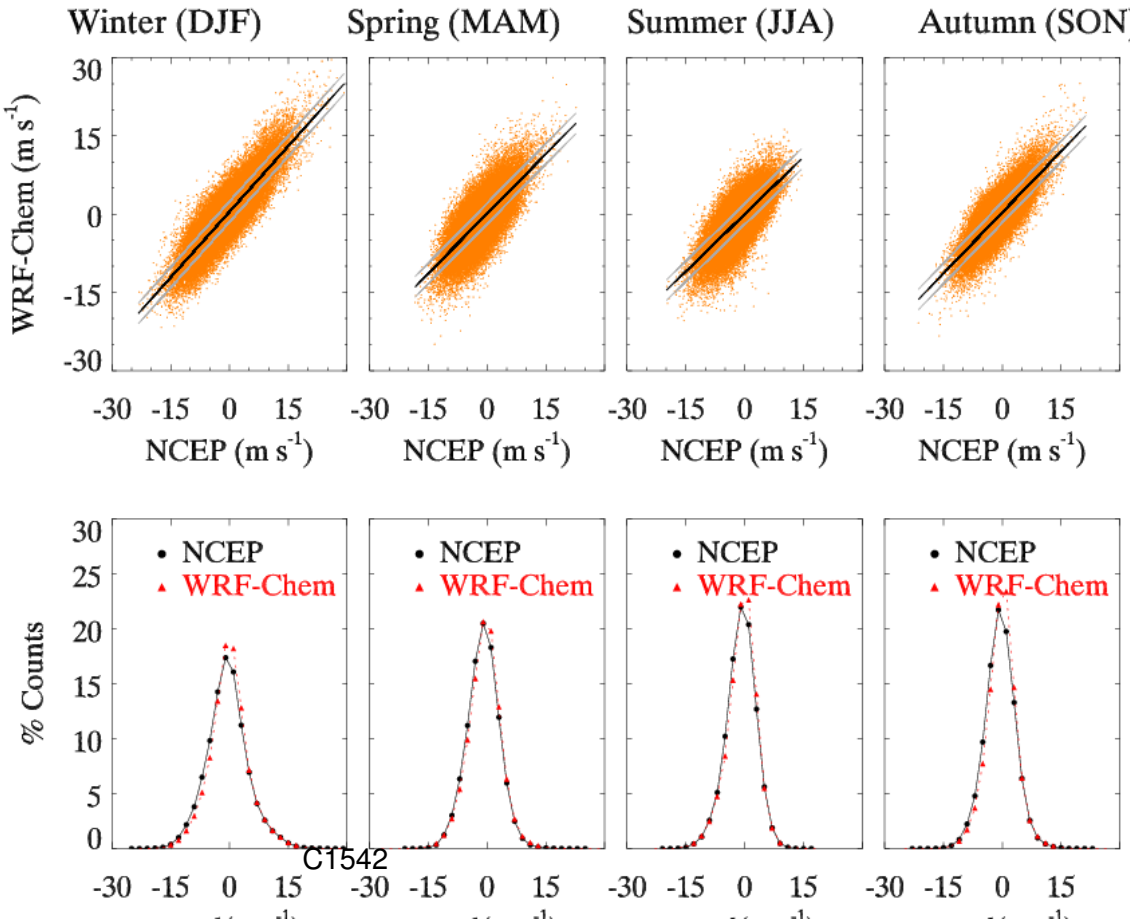

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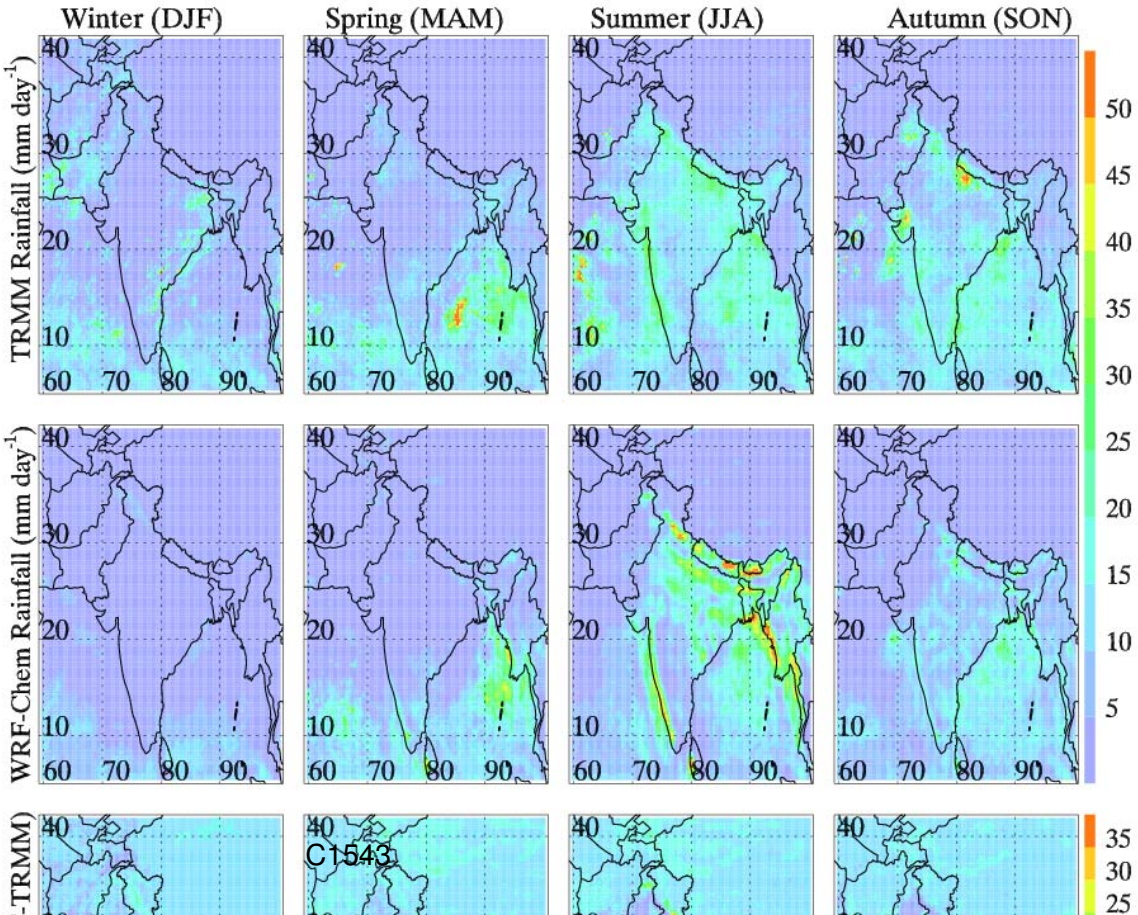

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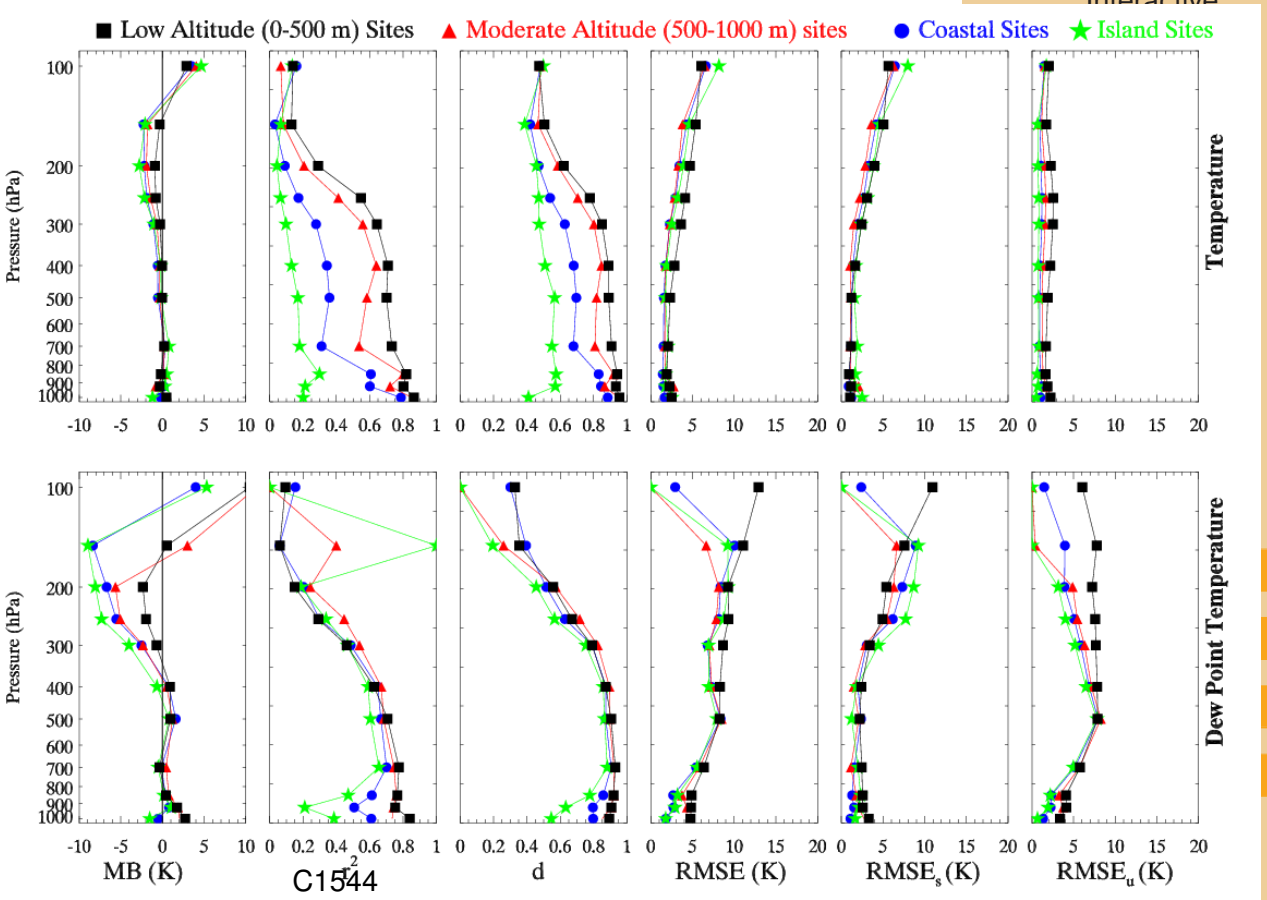
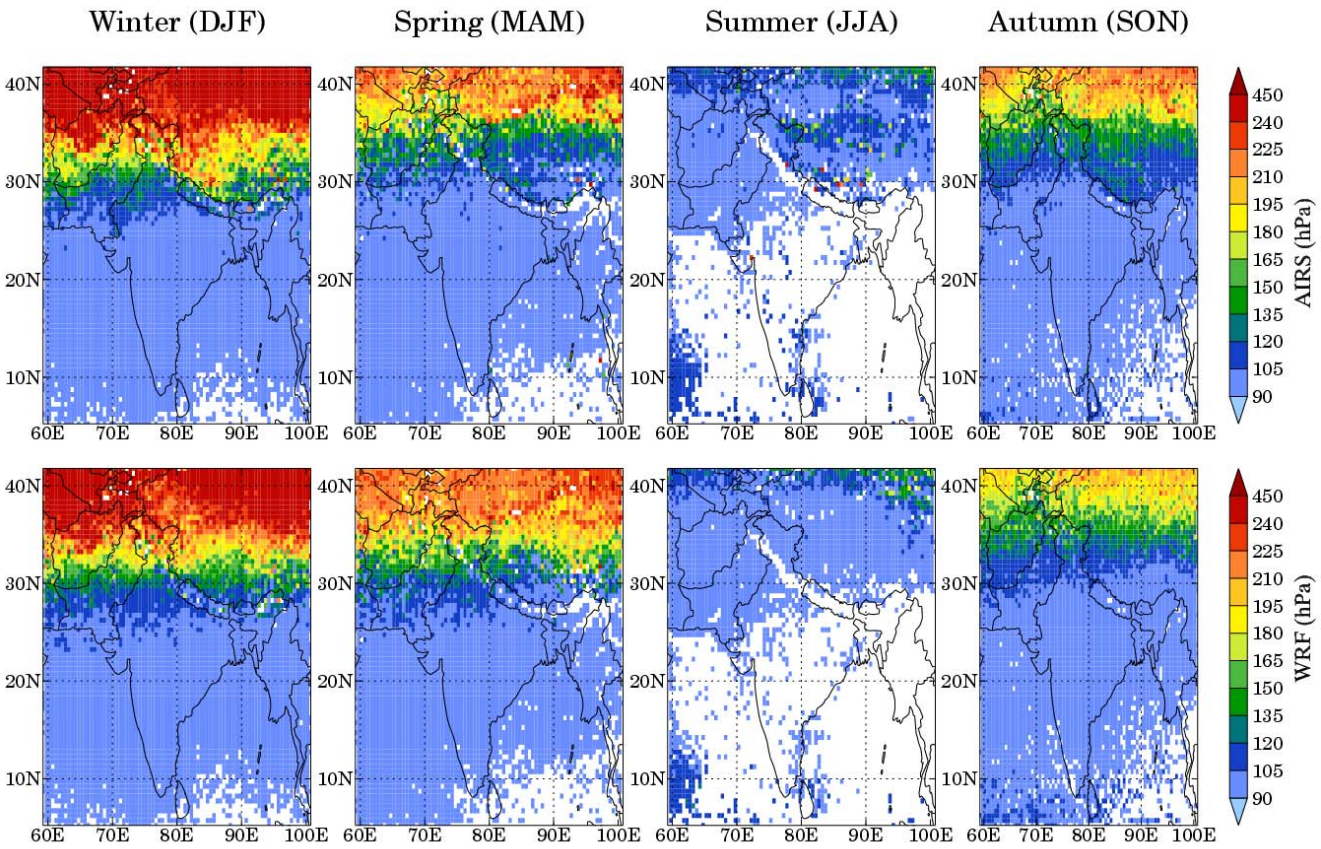
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