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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Implications on atmospheric dynamics and the effect on black carbon transport into the Eurasian Arctic based on the choice of land surface model schemes and reanalysis data in model simulations with WRF" *by* Carolina Cavazos Guerra et al.

Anonymous Referee #1

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Review of mansucript 'Implications on atmospheric dynamics and the effect on black carbon transport into the Eurasian Arctic based on the choice of land surface model schemes and reanalysis data in model simulations with WRF', by Cavazos-Guerra et al., for publication in Atmospheric Chemistry and Physics.

The study assesses the performance of the WRF-chem model for simulating black carbon concentrations in the Arctic. The model is evaluated using in-situ black carbon concentration observations from the Zeppelin Station located near Ny Ålesund.

Further experiments with WRF are performed to study how sensitive the model is to the choice of lateral boundary conditions, initial conditions, and land surface model used. In particular, simulations are performed using the ERA-Interim and ASR reanalysis products as initial and boundary conditions, and with the Noah Land Surface Model, and updated Noah Multi-Physics Land Surface Model. None of the model set-ups are found to be superior for all of the meteorological variables analysed, when compared with station data.

The WRF-Chem model was then run using the set-up with ASR for the initial and boundary conditions, and the Noah Multi-Physics Land Surface Model. The model if found to capture the overall seasonal cycle, with higher Black Carbon concentrations at the surface during winter compared with summer. However, there are biases in the absolute Black Carbon concentrations simulated, and the frequency distribution of concentrations do not match the observations. The problems for this may be the result of biases in the emissions data used as input data to the model. However, there are also known problems and in-consistencies with the methods used to measure black carbon concentrations, and there are very few black carbon measurements in general.

Overall, the study tackles a number of important and emerging topics for the scientific community. The study fits the scope of the journal well. It presents interesting results, and the text is easy to follow and understand. Therefore, I would recommend that the article be consider for publication, after the following comments have been addressed.

General comments: My main concern with the article is that the depth of analyses does not go far enough. I was left feeling somewhat disappointed at the end of the results section. After reading through the substantial methodology, there were only two very short sections on the implications of atmospheric dynamics on aerosol optical depth and black carbon deposition. The authors have conducted several very interesting experiments, and produced a number of exciting plots that contain a lot of information. I recommend that the authors take a little more time and consideration to interpret the results from these valuable analyses.

1. The comparison of the four different simulations (noah_asr, noahmp_asr, noah_ecmwf, noahmp_ecmwf) needs to be made much stronger. For example, what has a bigger impact: the choice of land surface model, or the choice of boundary / initial conditions. Which performs better: ASR or ECMWF? Under what conditions does each simulation perform best: Coastal? Mountainous? Continental? High Arctic? Urban? Summer or winter?

2. Figure 8 seems to show that the choice of LSM has more impact on the vertical structure of the lower 2 km of the atmosphere than the choice of boundary conditions.

3. In Figure 7 there is a huge amount of information. However, this is just passed off as – there is a large spread among the different simulations and stations. This result is not surprising or new, and is not worthy of publication in itself. The stations cover a variety of settings. Some stations are near the model's lateral boundaries; some are in the central domain; some are coastal; mountainous; urban; Arctic; mid-latitude etc. All of these factors will influence how well the model performs. For example, can you see that the choice of ASR or ERA-I matters more at the model boundaries? Does ASR perform better for the high latitude stations? For which stations does the use of Noah_MP have the biggest impact? I would recommend that you plot these Taylor diagrams for each individual station and look for patterns (not to be included in the paper necessarily). If there are no patterns, then this is a noteworthy result. However, only if the reader is convinced that these analyses have been conducted vigorously. At present, this is not the case.

4. Looking at Tables 4 – 5 and Figure 7, there appear to be some patterns. In particular, the use of Noah_MP reduces biases and RMS errors for the surface temperature, accept for high latitude stations during FMA. In general, the use of Noah_MP leads to an increase in surface temperature (reduced cold bias). While improving biases at the surface, the use of Noah_MP makes the representation of the vertical temperature profile worse. Noah_MP improves the surface winds and vertical wind profile. However, humidity biases are generally larger when using Noah_MP. ERA-I generally has

smaller biases and RMS errors for the mean sea level pressure than ASR. Clearly these relationships will not hold everywhere, but where do they hold, and why?

5. Would it be worth comparing precipitation in the different simulations?

6. I was disappointed that WRF-Chem was not used with the other experimental set ups. I think there needs to be further justification why this was not done? However, I would strongly recommend that these simulations are performed. Do you expect the same results with the different experimental set ups? The title of the study gives the impression that this is what you set out to explore.

7. It is stated that the purpose of Section 3.2 is to assess the model performance of simulating black carbon, and explore linkages with the atmospheric circulation. However, there is no discussion in this section regarding how model biases in the temperature profile, wind profile or humidity profile influence the aerosol optical depth or black carbon deposition at any of these stations. Would it not make more sense to show the vertical profiles for the Aeronet Stations in Figure 9, and the Black Carbon Station in Figure 10, in Figure 8, rather than the three stations you have chosen? The conclusion drawn, that the biggest problem with the model is the emissions data used as an input may very well be true. However, it is necessary to show that you have explored how model biases in the atmospheric circulation / boundary layer may affect the results. This is what your title suggests the main topic of the paper is about.

8. This may go beyond the scope of the paper, however, I was also expecting some discussion or analyses of how individual storm events transport black carbon to the station in Ny Alesund, and where the sources of this black carbon are.

9. Structure: The structure of the paper has a number of issues. In particular the methodology section is too long and it is difficult to find the key pieces of information in this section.

10. The model domain and resolution should be discussed at the beginning of Section

2, when the WRF model is first introduced. The choice of model domain needs to be discussed in relation to the questions you wish to address in this study. Where are sources of black carbon for the Arctic?

11. The Section: Comparison between ERA-Interim and ASR belongs in the results section rather than the methodology.

12. Land Surface Model – the discussion of land surface models needs to be more focused. What are the differences between Noah and NoahMP? How do you expect these differences to affect your results. There is a paper by Wang et al. (2010) that discusses deficiencies in the representation of snow in the Noah LSM. Are these accounted for in the versions of Noah used in this study?

Specific comments:

13. Abstract and Conclusions. Lines 31 – 33, and 658 – 680: "The results suggest that there are other factors to consider such as, for example, the accurate simulation of the Arctic sea-ice decline, stratosphere troposphere interactions, atmosphere-ocean interaction, and boundary layer processes." At no point in the analyses are these issues touched are. There is no plot of sea-ice concentration for each of the model simulations. This is not a coupled model, and I would expect that the sea ice extent is prescribed from satellite observations? Most of the stations the model is compared to are far outside the Arctic. I would expect that the resolution of the model and representation of local topography and urban surroundings might be a larger issue? I recommend removing these sentences from both sections.

Introduction 14. The introduction should be more focused. Discussing air-sea-ice interactions, and the decline of Arctic sea-ice is okay. However, how is this relevant to your work? The discussion needs to come back to this at the end of the paper if you wish to keep this in the introduction.

15. Lines 78-84: There should be more discussion here, and citations, of the source

regions and sources of black carbon in the Arctic. (e.g. Winiger et al., 2015)

Methods. 16. Line 153: Weather Research and Forecasting model. 17. Line 208: Is this issue fixed in Noah-MP? 18. Lines 237: ERA-Interim has 60 vertical model levels with the top level at 0.1 hPa. 19. Lines 240-242: Please add citations. 20. Line 320: Emissions. Please state in the text how the emissions are treated at the later boundary conditions. 21. Line 404: Aerosol observations. Please highlight more clearly in Table 3 and Figure 2, the seven Aeronet Stations, and the Zeppelin Station where black carbon observations were made. It may be useful to number all of the stations in the figure.

22. Line 597: Black Carbon. The recent study by Winiger et al. (2016) is worth considering here. They use the FLEXPART model to simulate black carbon concentrations at Zeppelin Station and in Abisko, Sweden. The model performs well in Northern Sweden, but underestimates the concentration at Zeppelin. This may be due to unaccounted local sources at Zeppelin.

23. Figure 11: For consistency, please make the x-scales in parts (a) and (b) match. 24. Figure 9: Show station numbers in part (a) to match part (b) 25. Figure 6: Why is this shown for the noahmp_ecmwf, when noahmp_asr is used with WRF-chem? 26. Figures 3 – 10: Please avoid using the same black colour to mark stations, plot contours and outline countries. It is very confusing. The resolution and size of some of the figures should be increased. 27. Figure 1: It is somewhat misleading to use the terms RMS and bias when comparing two reananlyses products. 28. Tables 4 – 5. Please use the same ordering in both tables for the four different simulations.

References

Winiger, P., A. Andersson, K. E. Yttri, P. Tunved, and Ö. Gustafsson (2015) Isotope-Based Source Apportionment of EC Aerosol Particles during Winter High-Pollution Events at the Zeppelin Observatory, Svalbard. Environmental Science & Technology, 49 (19), 11959-11966, DOI: 10.1021/acs.est.5b02644

Winiger, P., A. Andersson, S. Eckhardt, A. Stohl & Ö. Gustafsson (2015) The sources of atmospheric black carbon at a European gateway to the Arctic. Nature Communications 7, Article number: 12776. doi:10.1038/ncomms12776

Wang, Z., X. Zeng, and M. Decker (2010), Improving snow processes in the Noah land model, J. Geophys. Res., 115,D20108, doi:10.1029/2009JD013761

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