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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol optical depth over the Arctic: a comparison of ECHAM-HAM and TM5 with ground-based, satellite and reanalysis data" *by* J. von Hardenberg et al.

Anonymous Referee #2

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1 General summary

The paper describes comparisons of aerosol fields from two models (ECHAM-HAM and TM5) with in-situ, satellite and reanalysis data over the Arctic region for the period 2001-2006 . The challenges of this type of exercise are numerous, including the fact that this region is under-observed both at the ground and from satellite due to the high albedo (problematic for aerosol optical depth retrievals). To alleviate this problem, the authors make use of reanalysis data. While they make clear the point that this dataset

is not meant to replace direct observations, they show it has some usefulness in proving clues regarding seasonal and interannual variability. The two chemical transport models (CTMs) are found to have some degree of skill in representing the Angström parameter but significantly underestimate the observed Aerosol Optical Depth (AOD) values. Sensitivity studies to changes in emissions, model resolution and parameterization of wet scavenging are shown and results are discussed.

This study offer some good insights in the problem of aerosol modelling for the high latitudes. Parameterizations are not necessarily developed to perform well at high latitudes and there are little observations for model development and independent verification. The sensitivity studies point to the crucial role of the wet removal processes, and indicate the need for tuning the parameterizations to obtain better agreement in AOD. However, this does not help with reproducing seasonality, which point towards problems with the model climate. For this reason, this study would benefit from an investigation of the transport characteristics and the cloud/precipitation fields in the ECHAM-HAM and TM5 models. These aspects are usually neglected when looking at results from CTMs as there is the natural tendency to focus more on atmospheric chemistry aspects rather than the meteorological context at large. However, for a region such the Arctic where transport is the only source of aerosols, and wet removal processes are the main mechanisms for removals (gravitational sedimentation plays a minor role because the aerosol is dominated by small particles), it is important to look exactly at these aspects.

The paper is well-written and easy to read. I would therefore recommend its publication while recommending to the authors to add a section with an overview of the models' "climate" (i.e. winds, cloud cover and precipitation) over the period 2001-2006. I would also recommend reconsidering the conclusions. Given the lack of right seasonality in the model AOD, I am not convinced that only working on more complex parameterizations for wet removal processes is the best solution.

2 Specific comments

23, 20-24 : very well worded!

24, 17-20 : wind fields are also provided by the MACC reanalysis for the period 2003-2006. It might be worth it to use the winds from MACC rather than ERA-Interim when available as those wind fields would have been produced with the same (and more recent) model version that has produced the aerosol fields.

28, 5-10 : the words of caution on the use of the MACC reanalysis data for this region are very appropriate. In the MACC reanalysis all data above 60 degrees of latitude were blacklisted. Better agreement at one station (Alert) and worse agreement at another (Summit) does not necessarily mean much. However, it is still obvious that the ECHAM-HAM and TM5 models have a problem of underestimation of AOD at most stations when compared to available ground-based data and reanalysis.

30, 18 why not show the plots of the sea-salt and the organic carbon?

30, 29 this is interesting. Perhaps this is an overestimation of sea-salt in the reanalysis. It would be great to have other (independent) observations. Unfortunately the period under study is precedent to the launch of the CALIPSO satellite (April 2006). Could the authors think of datasets other than the MODIS data for model assessment?

31, 20 did you use any multiplicative factor for the black carbon and organic matter emissions from wildfires? It appears to be something needed in order for model AODs to match satellite remotely sensed AODs. Please see these publications for further reference:
J. W. Kaiser et al., 2012: Biomass burning emissions estimated with a global fire assimilation system based on observed fire radiative power *Biogeosciences*, 9,

527-554.
Huijnen, V. et al, 2012: Hindcast experiments of tropospheric composition during the summer 2010 fires over western Russia, *Atmos. Chem. Phys.*, 12, 4341-4364, doi:10.5194/acp-12-4341-2012,

34, 5-6 Most likely.

34, 20-25 Could also be due to wrong cloud and precipitation fields in the model. Since the wet removal is so crucial, then it's important to make sure that the models have a reasonable representation of the atmospheric branch of the water cycle.

36, 10-20 I am not convinced by the explanation given in lines 15-19 that chemical and physical processes are more important than transport because the model showed little sensitivity to being run in climatological mode or nudged using analyzed winds. Lack of the right seasonality and wrong spatial structure for arctic aerosols can only be related to 1) errors in transport or 2) errors in emissions, and the authors exclude both. I may be missing something, but to me it's not possible that it all comes down to the wet scavenging.

37, 1-10 Again, I am not sure that looking at improving scavenging processes alone is a good idea. The fact that the AOD matches better with observations in magnitude when using the Bourgeois and Bey (2011) parameterization but the seasonality is still wrong, means to me that focusing on the wet removal processes is not the way to go. You may just be getting higher AODs for the wrong reasons. A more in-depth look at the model wind, clouds and precipitation fields for the Arctic would be more instructive.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8319, 2012.