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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The high Arctic in extreme winters: vortex, temperature, and MLS and ACE-FTS trace gas evolution" *by* G. L. Manney et al.

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One of the reviewers was unfortunately not able to submit a detailed review after a positive first assessment, in spite of repeated reminders and extensions of the submission deadline. I therefore add my own thoughts to the report of the second reviewer.

The paper "The high Arctic in extreme winters: vortex, temperature, and MLS and ACE-FTS trace gas evolution" by G. L. Manney et al. presents a detailed overview of the meteorological conditions during the three ACE validation campaigns at Eureka. It includes data from several satellite instruments (SABER, MLS, ACE-FTS), ground-based instrumentation (LIDAR, radio sondes) and compares it to results from different

models.

The paper is well written and provides an excellent background for interpretation of the results from the ACE validation campaigns which will be presented in other papers of this special issue. It also presents a discussion of three very different NH winters which in its own is interesting to read and highlights the large degree in variability both in time and space that is characteristic of the NH stratosphere and mesosphere. This is of particular relevance when using measurements from one specific site (Eureka) for validation of satellite data.

The problem I have with the paper is that in my opinion, it tries to include too many different things instead of focusing on its central part which from my point of view is the description of the dynamical evolution in the three years. In its current form, the manuscript deals with - among other things

- the description of the evolution of stratosphere and mesosphere in the three winters based on measurements and model results
- a comparison of different satellite measurements and model results with some aspects of model verification
- a comparison of LIDAR temperature profiles with satellite and model profiles which in part could be seen as qualitative validation of satellite profiles but also includes some discussion of possible LIDAR problems when an inappropriate seeding value is used
- a comparison of satellite and model temperatures with radio sonde data which again could be seen a s a qualitative validation
- a comparison of temperature data from three satellite instruments sampled at the time and place of ACE measurements, again a qualitative validation

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- plots of CO, H_2O, N_2O, O_3, HCl, and HNO_3 from MLS and CO and H_2O from ACE-FTS with a very brief discussion

While the various comparisons of the different data sets are interesting and provide confidence in some features observed from several platforms while raising questions about others, they do not necessarily add a lot of information for the discussion of the meteorological evolution of the winters. At the same time they are not quantitative enough for a validation of the ACE-FTS temperature measurements as it is very difficult to draw firm conclusions from the colour coded plots shown on top of each other.

The same is true for the discussion of the model data - it is instructive to compare model and measurements as done in Fig. 4 and the model problems are quite obvious but it is not clear to me what we learn in addition by Figs. 7 - 13 apart from the fact that different model (versions) have different problems and that the measurements disagree in the upper parts of the profiles. Full model verification would have to go into much more detail on the model side and in its present form, the comparison remains somewhat superficial.

Finally, the discussion of the trace gas measurements is very brief and I do not see the additional value of showing data from MLS and ACE (and again SABER for T) unless it is intended as a first quick look validation. A detailed discussion of how the meteorology affects chemistry would be a separate study and clearly is out of the scope of the paper.

In summary, I think that the paper could actually be improved by removing some of the data shown in the plots and focusing the discussion more on the meteorology of the three winters and how it affects the measurements at Eureka. I also recommend removing some of the qualitative comparisons between different data sets which in my opinion do not add much to the main point of the paper and are not quantitative enough for a validation of ACE-FTS measurements.

Once the paper is changed in this sense and also the comments of the reviewer are

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into account, I will be happy to accept it for publication in ACP.

Minor comments:

- Abstract: Introduce acronym SABER
- geo-location of Eureka varies through the text - is that intentionally?
- page 10249, line 17: "seen best" - I think it can *only* be seen in the model data
- page 10254, discussion of the LIDAR profiles at high altitude. If the problem is with the seeding value used for the LIDAR profiles, why don't you use a more realistic value at least for a few examples to test if this improves the agreement with the satellite derived data?
- page 10263, "how these conditions affected transport and chemistry" - I think the discussion is really very much limited to transport. Chemical composition is touched, mainly with respect to the effect of descent but chemistry in the sense of chemical evolution is not really discussed.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10235, 2007.