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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variation in middle atmospheric ozone by ground based microwave radiometry at Ny-Ålesund over 1 year" *by* Franziska Schranz et al.

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The paper by Schranz and co-workers presents stratospheric and mesospheric ozone observations in the Arctic with a particular focus on the diurnal variation. Ground-based ozone observations from two different microwave radiometers are compared with simulations from the SD-WACCM model. In addition to the analysis of the diurnal cycle, intercomparisons of the two instruments between each other and with independent satellite (MLS) and ozone sonde observations are presented. I found this paper very interesting to read; it is generally well written, clearly structured and the methods seem to be sound and robust. I recommend publication in Atmos. Chem. Phys. after

consideration of the following, mostly minor, comments.

Specific comments and technical corrections:

P1L13: "task" is a funny word in this context

P1L15: not clear what the purpose of this statement is: there are also variations on shorter time scales than diurnal and longer time scales than inter-annual

P2L2: No, the other way round: photolysis becomes more important than recombination

P2L7: this sentence is a bit flawed: the magnitude of the diurnal cycle does not depend on the solar zenith angle. Do you mean variation of solar zenith angle?

P3L1 (and throughout the document): "arctic" -> "Arctic"

P3L15: "NDACC instrument" -> "OZORAM which is part of the Network for the Detection of Atmospheric Composition Change (NDACC)"

P3L20: "dynamical events" is jargon: try to be more specific

P3L25: Not sure if a historic review of Ny-Alesund is justified here. However, I do think it is relevant to provide information about the history of ground-based microwave and ozone sonde observations at Ny-Alesund.

P3L33: "very good opacity" alone not meaningful. Better give threshold opacity needed or signal-to-noise (or similar) as a function of opacity. More importantly, this is instrument specific. Would be good to briefly discuss how that affects the two microwave instruments at 110 and 142 GHz differently.

P4L26-31 and Fig.2: The discussion of different ozone observations in different viewing directions is very interesting but seems to be slightly out of place within the instrument description. I would encourage the authors (but this is not essential to this paper) to expand the discussion a bit on this point, e.g. by providing information on the difference between ozone in different directions as a function of time.

P5L21: do you mean 1.2° latitude and 6° longitude ??

P6L16: "strength of 10%" not immediately clear: Either provide more information or just state that nudging is done up to 50km and then linearly decreasing in strength with no nudging above 60km.

P6L22: again, opacity threshold may be useful

P6L24: whether or not ozone decreases across the vortex edge depends on altitude!

P7L32: Can you give more information how the tropospheric correction is done for GOMOS-C: Even if the retrieval starts at the tropopause, tropospheric opacity has to be taken into account somehow.

P8L1: What does scaling of a standard O2 profile mean?? What is scaled? I don't think O2 is scaled.

P8L11: this statement is likely true only for this particular year. In other winters strong variations in mid-winter may be possible.

P8L21-23: why is the annual change in geopotential height relevant at this point?

P10L28: You may want to compare this to Sinnhuber et al., J. Atmos. Chem., 34, 281-290, 1999, their Fig. 7, for stratospheric ozone change as a function of solar zenith angle.

P11L9: what is a "super diurnal cycle"?

P11L23: I believe it has to be GOMOS measurements, not GOME measurements!

P12L12-21: I don't fully understand your arguments for possible differences between GROMOS-C and SD-WACCM: Any averaging kernel related effects are already taken into account when comparing with the convolved profiles, I believe?

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