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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Daytime ozone and temperature variations in the mesosphere: a comparison between SABER observations and HAMMONIA model" *by* S. Dikty et al.

Anonymous Referee #1

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General Comments:

This short paper compares the variability of mesospheric ozone from SABER satellite observations with that from a numerical model. The results are new and will contribute to the assessment and understanding of mesospheric ozone. The paper gives quick evaluation of proposed mechanisms but should give a more in depth presentation of the relevant ideas and how the current results fit with them.

The authors discuss two processes that could be responsible for daytime ozone variations: solar zenith angle variations and diurnal tidal interactions. They decide that the former are responsible for both daytime local time variations and for seasonal varia-

tions. The problem with this is the assumption that a single process controls variability throughout the mesosphere. A look at Brasseur and Solomon (2005), figure 5.3, shows that the Ox lifetime is less than a day below 80-85 km and is much longer than a day above 85 km. Transport of Ox will not be effective in the lower part of the region they consider (pressures higher than $\sim$0.005 hPa) but will be above there.

The variations are presented as percentages, a format that accentuates the signal in regions with low ozone. For an evaluation of the ozone in the upper part of the analyzed domain, it would be useful to see more detail about the variations there. Perhaps some additional contours could be added to the plots in Figures 4 and 5 or you could also show the variations of mixing ratio (not percentage difference).

Specific Comments:

1. The introduction needs more focus. It should lay out clearly what is understood and not understood about daytime ozone in the mesosphere. The references to previous studies are useful but it would also be useful if the paper gives some guidance about what progress has been made and what is still not understood. 2. Why is the SABER analysis for 4 years only? There are now 8 complete years of data available. And since you are comparing with HAMMONIA simulations at solar minimum conditions, wouldn't it make more sense to choose recent rather than early years for the SABER data? 3. The labels at the tops of Figures 4 and 5 do not match the captions or the description in the text (p. 2013; l. 18). Based on discussion at the top of p. 1014, I am guessing that the labels at the top of the figures are correct and the captions and text are wrong. 4. The problems with the 1.27 um ozone retrieval at twilight have been discussed by Zhu et al. (2007). 5. There is something wrong with the HAMMONIA temperature anomalies at 18 h (Figure 6). 6. It is unexpected that you get "spurious" temperature signals in SABER considering the large amount of data averaged to produce Figure 6. Did you look carefully at this? Perhaps there are some anomalous profiles that should have been screened. 7. On page 2014 and 2015, the higher ozone during equinox periods is interpreted as being due to increased solar input. If this were the cause, then

one would expect ozone to be substantially higher in January, when the Earth is closest to the sun, and lower in July. I would guess that the Earth-sun distance effect (6%) is larger than the small seasonal change in solar zenith angle averaged over 20°S-20°N. You could easily check this and give quantitative numbers for the magnitude of the variation in the averaged O2 photolysis rate at a few altitudes. On the other hand, you cite evidence that Ox at altitudes above 80-85 km (where its lifetime is long) responds to transport by the diurnal tide. Ozone also is quite sensitive to temperature variations, including those associated with the tides. Since the diurnal tide is much larger during equinoxes than during solstices, this is a more likely explanation for the seasonal cycles at 84 and 88.5 km than the changes in solar input. 8. Most of the discussion about the chemistry is contained in a single long paragraph spanning pages 2015 and 2016. This was hard for me to follow. Proposed explanations by Ricaud et al. and Marsh et al. are described very briefly and then partially rejected. Since there have been improvements in the observations and numerical modeling since the time of those papers, this topic deserves additional discussion. You should have all the tools for a more thorough look at the question. For example, with a comprehensive numerical model, it should be possible for you to present a more quantitative analysis: e.g. calculating the ozone loss rather than just showing a few species that are involved in the loss reactions. 9. I am confused about your statement that the chemistry may play a more important role in the upper mesosphere. Ozone comes into equilibrium rapidly. Therefore, its concentration depends on the chemical composition and temperature of the ambient air. So it is valid to assume that chemistry plays a role but, if long-lived species such as H2O and O are transported, transport will also. And if the tides control the daytime temperature change, that will also contribute. 10. From Figure 6, it is evident that the phase of the diurnal tide in HAMMONIA differs from that measured by a half cycle. This contradicts the comment on p. 2016 (l. 24-26) that the phases agree well.

Editorial comments

1. (p. 2006; l. 20) Change “effected” to “affected” 2. (p. 2009; l. 13) Change “circulates”

to “circles”. 3. (captions for figures 4-7) Please give some more information, either in the text or the caption itself. By “deviation from the mean in %”, what mean are you referring to? Altitude dependent or a single value? Using all months and years?

References

Brasseur, G.P. and Solomon, S. Aeronomy of the Middle Atmosphere, Third Edition, Springer, 2005.

Zhu, X., J.-H. Yee, and E. R. Talaat (2007), Effect of dynamical-photochemical coupling on oxygen airglow emission and implications for daytime ozone retrieved from 1.27 ?m emission, J. Geophys. Res., 112, D20304, doi:10.1029/2007JD008447.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2005, 2010.