

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

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Please find my replies to each comment below. The supplement file contains the updated script.

Kind Regards, Sebastian Dikty

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

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what progress has been made and what is still not understood. → I have moved the last paragraph of the introduction to almost the beginning of the Introduction. It contains some focus you were asking about, so the reader receives the main objectives right from the start. Afterwards I go through selected references and explain the main results of the respective paper. The question as to what is still not understood is dealt with in the Discussion.

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? → SABER data was only available till mid 2007 at the time of analysis. So I chose the complete years 2003–2006. It is certainly true that we are comparing HAMMONIA to the early years of SABER observations, but including all years would mean a complete start from the scratch. As to my knowledge daytime variations did not show a decline in the amplitude with the declining phase of solar cycle 23.

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. → I mistakenly swapped Fig. 4 and 5. This error has been corrected.

4. The problems with the 1.27 μm ozone retrieval at twilight have been discussed by Zhu et al. (2007). → Thank you for the additional reference. I have added it accordingly.

5. There is something wrong with the HAMMONIA temperature anomalies at 18 h (Figure 6). → I have checked and found that there is steep increase in temperature at 18h. This caused the contours to be drawn in this manner. I have removed data from 18h for the contours to be drawn without the nightly increase.

6. It is unexpected that you get “spurious” temperature signals in SABER considering

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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. → I have screened for anomalous profiles twice. Once when computing the area weighted zonal mean profile and once when a complete time series has been produced.

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 O_x 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. → According to your hypothesis there should be a difference between January and July (Sun-Earth distance), which is not the case. The difference is rather between solstice and equinox. Nonetheless, I believe you are right by taking the tidal induced temperature variation into consideration. I have added this aspect to the script.

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

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species that are involved in the loss reactions. → A comprehensive numerical model also demands for a sophisticated cluster to run on and time to prepare. The model data as presented in this study has been taken from earlier model runs and does not contain e.g. ozone loss rates.

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. → I have added the aspect of tides affecting the temperatures and thus the ozone to the discussion. Although temperature does not agree well between model and observation, I have to point out that temperature variations are very small in the order of only up to 1.5 K above 80 km (i.e. 0.01 hPa) and less below, where tides do not play any role.

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. → Achatz et al. are referring to diurnal tides and not to ozone. The signature as seen in Fig. 6 is in ozone not in tides directly.

Editorial comments

1. (p. 2006; l. 20) Change “effected” to “affected” → Changed accordingly.

2. (p. 2009; l. 13) Change “circulates” to “circles”. → Changed accordingly.

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? → I made changes to the caption and in the text to clarify what I mean with “deviation from the mean in %”

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Please also note the supplement to this comment:
<http://www.atmos-chem-phys-discuss.net/10/C5762/2010/acpd-10-C5762-2010-supplement.pdf>

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

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