**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Water vapour and the equatorial mesospheric semi-annual oscillation (MSAO)" *by* R. L. Gattinger et al.

Anonymous Referee #1

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Review of ACP manuscript entitled 'Water vapour and the equatorial mesospheric semi-annual oscillation (MSAO)' by Gattinger et al.

General comments:

This paper is a modeling study focusing on the mesospheric semi-annual oscillation at low latitudes. A 1-D model is employed – accounting for tidal modulations – to model tidal and seasonal variations in a series of minor species in the MLT region. Comparisons between model simulations and satellite measurements are also described for selected species. The content of the paper is of interest to the middle atmosphere community, because the origin of the MSAO does not seem to be fully understood. The paper should eventually be published, in my opinion. However, the paper is not acceptable for publication in its present form, and major revisions are required. My

general criticism can be summarized in the following points:

1. The paper contains many little inconsistencies, that should be addresses (see specific comments below)

2. A discussion of the current understanding of the origin of the MSAO is missing. This should be an essential part of such a publication.

3. It is not discussed explicitly which of the processes considered in the model actually lead to the fairly good representation of the MSAO. Is it mainly the different vertical advection? To what extent do the different eddy diffusion coefficients for solstice and equinox conditions contribute to the MSAO? (What's the reason for differences between these coefficients for solstice and equinox?). I presume that the MSAO is mainly driven by different vertical advection due to variable tidal amplitudes. This and related aspects should be discussed in much more detail, and, e.g. the assumed tidal variations in vertical advection should be shown for both solstice and equinox conditions.

Specific comments:

1. Title: The title does perhaps not reflect the content of the paper, because water vapour – although being an important species – is only one of many species treated.

2. Page, 731, Abstract, line 14: 'At the equator, 90 km altitude, the derived eddy mixing rate is approximately 1×10^6 cm^2 s^{-1} and vertical advection 0.8 cm s^{-1}. For April the corresponding values are 4×10^5 cm^2 s^{-1} and 0.1 cm s^{-1})'

It's not clear which month the statement in the first sentence refers to. As is, the statements are partly contradictory.

3. Page 731, line 24: '.. the brightness being well correlated with the horizontal wind field.'

Does 'horizontal' mean 'zonal' or 'meridional' ?

4. Page 733, lines 10 – 23: This paragraph doesn't seem to be well organized. Dunker-

ton (1982) is cited suggesting that gravity waves drive the semi-annual oscillation, but the papers cited in the following sentences have little to do with the MSAO. Dikty et al. barely mention the semi-annual oscillation. I think the focus of this paragraph should not be on a general discussion of the agreement between models and measurements, but more on the current understanding of the origin of the MSAO.

5. Page 734, lines 7 – 17: It's not entirely clear to me why all these different H_2O (SMR & MLS) measurements are listed in the section 'data driving the model simulation' if only the ACE-FTS measurements are used for the model simulations. You only use ACE-FTS measurements to run the model, right? Then the other data sets don't need to be mentioned here. Perhaps I'm missing a point?

6. Page 734, line 11: 'At solstice the Aura group recommended an H_2O mixing ratio at 90 km of approximately 1 ppm,'

I don't quite understand what the implications of this statement are. Is this value used in this study? What about other times of the year?

7. Page 734, line 19: 'For the range from 15S to 30S the average ACE-FTS H_2O missing ratio for April is approximately 0.1 ppm'

What altitude does this statement refer to?

8. Page 734, line 14: 'For the initial conditions the ACE-FTS H_2O profiles from approximately 15N to 15 S are averaged.'

What time period is used for the averaging?

9. Page 734, line 23: 'The H profiles assumed in the model initial conditions were determined using data from a number of sources.'

This sentence contradicts the statement on page 735, line 2: 'Ultimately, the model simulation of H is derived from the measured H_2O profiles combined with the solar photo-dissociation by Lyman-alpha ..' Which statement is correct? If the latter is cor-

C147

rect, then all the difference sources of the H profile mentioned before don't have to be listed. If the first statement is correct, then you should mention explicitly which data set has been used. Just stating that the initial conditions came from a number of sources is not very helpful.

10. Page 735, line 24: 'They observed a maximum in turbulence at solstice WITH A TURBULENT/EDDY MIXING COEFFICIENT of approximately 2×10^6 cm^2 s^{-1}'

11. Page 736, line 4: 'For April in the equatorial region they inferred a downward wind of approximately 0.5 cm s^{-1} at 90 km altitude and for July a similar but upward wind.'

What local time are these values valid for? Due to the potentially strong tidal effects the statement is almost meaningless without mentioning the local time.

12. Page 737, line 18: 'Building of previous approaches, the non-linear methods employed in the current model to simulate vertical transport across layer intersections have been tested to limit numerical error propagation to an acceptable level.'

This is a very vague statement. What 'non-linear methods' were actually employed in this study? This should be discussed. What is an 'acceptable level'?

13. Page 737, line 21: 'For the 1-D model the tides are included as both temperature oscillations and vertical winds (Hagan et al., 1999). '

I suggest showing a sample plot with the tidal signatures in temperature and vertical wind.

14. Page 737, last line: I suggest changing 'and sunset ACE-FTS profiles is assumed, this is ..' to 'and sunset ACE-FTS profiles is assumed. This is ..'

15. Page 738, line 3: 'The baseline 90 km eddy mixing coefficients employed in the model ..'

Suggest mentioning explicitly that this is the vertical eddy mixing coefficient.

16. Page 738, line 5: 'Baseline background vertical advection in the 90 km region is assumed to be upwards at 0.1 cmsˆ(-1) for April and 0.8 cm sˆ(-1) for August'

OK, but it would also be relevant to mention the amplitude of the tidal variations in vertical winds. How were the baseline vertical winds determined? Empirically, or based on earlier studies?

17. Figures 2a and 2b: The two contour plots appear to be identical (I'm unable to tell whether there are any differences & the scale is the same), which is probably not intended, given that the H abundances should change as different H2O fields are used.

18. Page 738, line 13: 'From 90 to 100 km the H mixing ratio is approximately constant,'

Well, it certainly varies with altitude, but it's almost constant in time.

19. Page 738, line 16: 'From 85 to 90 km the April H values are approximately two-thirds the August values.'

Again, this is not seen in Figs. 2a and 2b, probably because one of the figures shows the wrong plot.

20. Page 738, line 21: 'Model H densities in Fig. 2a, b are clearly anti-correlated with the model O densities as shown in Fig. 3a, b.'

I'm not convinced this statement is correct. Firstly, what altitude are you referring to? The phase behavior of H depends strongly on altitude, as demonstrated in Figures 2a and 2b. Secondly, there appears to be a phase shift between the H and O variations, but it doesn't seem to be large enough for the two time series to be 'anti-correlated'.

21. Page 739, line 12: 'In both cases the tidal phase near midnight shifts from a maximum near 82 km to a minimum near 94 km.'

According to Figures 3a and 3b the maximum before midnight is at 92 km, not 82 km. Moreover, I'm unable to find a figure confirming this statement in Smith et al. (2010). Perhaps you intended to state that the overall local time behavior in the SABER O

retrievals is similar? The SABER O maxima don't seem to occur at the altitudes, where the model maxima occur. Perhaps I'm missing something here. If I'm wrong please correct me, and mention explicitly where this information can be found in Smith et al. (2010).

22. Page 739, line 13: 'The O tides derived from the UARS/WINDII observations by Shepherd et al. (2006) show similar variations with both altitude and local time.'

I'm puzzled by this statement, because Shepherd et al. (2006) do not derive O tides from WINDII measurements. This paper is mainly about the MSAO. Perhaps you intended to cite another paper?

23. Page 739, line 21: 'tides in Fig. 5b, this is expected' -> 'tides in Fig. 5b. This is expected'

24. Page 740, section 5: As already mentioned in the general comments above, a discussion of the direct origin of the MSAO in the 1-D model simulations should be included. Most likely, different tidal amplitudes and/or baseline vertical advection are mainly responsible for the MSAO.

25. Fig. 6: Please mention in caption which symbol corresponds to which year of the GOMOS observations

26. Page 740, line 20: 'for the OH* 9-4.' -> 'for the OH* 9-4 band.' or 'for the OH* 9-4 Meinel emission band.'

27. Page 740, line 20: 'Model volume emission rate (VER) profiles of OH* 9-4 are shown for April in Fig. 7a and for August in Fig. 7b.'

Are these VERs for all rotational lines of the band, or just parts of the band?

28. Figures 7a and 7b: Unit is missing (probably photons / s / cm3)

29. Page 740, last paragraph, on the OH(9-4) comparisons: Is the entire OH(9-4) band used here, including P, Q & R-branch, or only parts of the band? Were the integrated

limb emission rates inverted to vertical emission rate profiles? And then integrated vertically? Please provide more details on these comparisons.

30. Page 741, line 4: 'Slightly better agreement is achieved by arbitrarily increasing the rate of removal OH* (v' = 9) by a factor of three.'

Smith et al. (2010) used a value for this rate (for vibrational levels 8 and 9) which is a factor of 4 smaller than the Adler-Golden value. Smith et al. report that a value of 3×10^{-10} cm^3 s^{-1} (roughly the Adler-Goldon value) yields unrealistically large O values retrieved from SABER.

31. Page 741, line 15: 'are for the baseline eddy mixing COEFFICIENT of approximately 1×10^6 cm^2 s^{-1} '

32. Same line: Why 'approximately' ?

33. Caption Fig. 9, line 3: 'The large square locates approximately'

Why 'approximately', why not plotting the square according to the exact abundances of ozone and H2O measured by GOMOS and ACE-FTS?

34. Caption Fig. 9, line 5: 'The dotted line is for an assumed eddy mixing COEFFICIENT ..'

35. Page 741, line 21: 'Reducing the eddy coefficient causes a decrease in the H2O mixing ratio at 90 km, the result of the ongoing loss of water vapour by Lyman-alpha photodissociation'

The photodissociation of H2O certainly contributes to the effect, but the reduced upward mixing of H2O is also relevant.

36. Page 742, line 3: 'These simulations suggest that dynamical effects play a major role in the generation of the MSAO, as postuldated by Dunkerton (1982) three decades ago.'

C151

The discussion of the sources of the MSAO should be extended significantly. In a way, the most important question related to the MSAO is what it is actually caused by.

37. Page 742, line 19: 'an eddy mixing COEFFICIENT of approximately'

38. Page 742, line 22: 'the uncertainty in the derived eddy mixing rate is estimated to be less than 30% and in vertical advection to be less than 0.2 cms-1.'

This point is not discussed in the paper, and it needs to be clearly stated what assumptions or investigations these estimates are based on.

39. Page 743, acknowledgements, line 5: 'GOMOS on board SCIAMACHY' GOMOS is on board Envisat, as is SCIAMACHY.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 729, 2013.