Atmos. Chem. Phys. Discuss., 13, C1539–C1549, 2013
www.atmos-chem-phys-discuss.net/13/C1539/2013/

Atmospheric Chemistry and Physics Discussions

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

R. L. Gattinger et al.

edward.llewellyn@usask.ca

Received and published: 19 April 2013

Atmos. Chem. Phys. Discuss., 13, C145ûC152, 2013 www.atmos-chem-phys-discuss.net/13/C145/2013/

Authors' response to the Anonymous Referees' comments. These are delimited with triple asterisks.

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

Anonymous Referee #1

This paper is a modeling study focusing on the mesospheric semi-annual oscillation at

low latitudes. A 1-D model is employed û accounting for tidal modulations û 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. *** The discussion of the equatorial MSAO in the Introduction, para. 3, has been reorganized and expanded, specifically with respect to the gravity wave driven solstice westerlies and equinox easterlies. ***

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 is the reason for differences between these coefficients for solstice and equinox?). *** As now noted more explicitly in the Abstract and in the Introduction the objective is not to explain the MSAO but to determine the differences, or limits, in the underlying dynamics between equinox and solstice and possibly thereby to provide some insight into the MSAO drivers in future studies. Concerning the relative importance of vertical advection vs eddy diffusion Figure 9 shows that both are significant. Some text has been added to the Figure 9 comments. ***

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. *** The added comment in Simulations specifically notes the source of the April and August diurnal tidal phases and amplitudes, from Hagan et al (1999), in MSAO 'Data driving the model simulation', last para. Again, we do not speculate on the MSAO driving sources. ***

Specific comments:

1. Title: The title does perhaps not reflect the content of the paper, because water vapour û although being an important species û is only one of many species treated. *** Perhaps "The roles of vertical advection and eddy diffusion in the equatorial..." is a better title and we have amended it accordingly. ***

2. Page, 731, Abstract, line 14: At the equator, 90 km altitude, the derived eddy mixing rate is approximately 1 x 10ˆ6 cmˆ2 sˆ(-1) and vertical advection 0.8 cm sˆ(-1). For April the corresponding values are 4 x 10ˆ5 cmê2 sˆ(-1) and 0.1 cm sˆ(-1). It is not clear which month the statement in the first sentence refers to. As is, the statements are partly contradictory. *** See amended Abstract - August and April are specifically included now. ***

3. Page 731, line 24: æ.. the brightness being well correlated with the horizontal wind field. Does horizontal mean zonal or meridional? *** Meridional added, from original paper. ***

4. Page 733, lines 10 û 23: This paragraph doesn't seem to be well organized. Dunkerton (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. *** Introduction para 3 now deals with MSAO and para 4 deals with photochemical models. ***

5. Page 734, lines 7 û 17: It's not entirely clear to me why all these different H2O (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? *** Since reliable measurements of upper meso H2O mixing ratios are only now becoming available we have included this comparison to show that there is satisfactory agreement amongst the various observations. ***

6. Page 734, line 11: At solstice the Aura group recommended an H2O 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? *** Again, this is to show independent H2O measurements are in approximate agreement. ***

7. Page 734, line 19: For the range from 15S to 30S the average ACE-FTS H2O missing ratio for April is approximately 0.1 ppm What altitude does this statement refer to? *** Added 90 km as a qualification. ***

8. Page 734, line 14: For the initial conditions the ACE-FTS H2O profiles from approximately 15N to 15 S are averaged. What time period is used for the averaging? *** Added 5 days as a qualification. ***

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 H2O profiles combined with the solar photo-dissociation by Lyman-alpha. Which statement is correct? If the latter is correct, 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. *** Text modified to point to Thomas for initial H conditions. As for H2O case above, the measured H

mixing ratio at 90 km can be questioned. Hence we prefer to retain the references to show the agreement is at least qualitative. ***

10. Page 735, line 24: They observed a maximum in turbulence at solstice WITH A TURBULENT/EDDY MIXING COEFFICIENT of approximately 2 x 10ˆ6 cmˆ2 sˆ(-1). *** Modified throughout text. From another Reviewer all references to eddy now use 'diffusion'. ***

11. Page 736, line 4: For April in the equatorial region they inferred a downward wind of approximately 0.5 cmˆsˆ(-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. *** These values are derived from monthly means of zonally averaged meridional winds. See the expanded text, the term 'prevailing' has been included to aid in clarification. ***

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? *** This has been restated, the error limit less than a few percent now included. ***

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. *** We do not believe that this would be very informative as it would simply duplicate Hagan et al. ***

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 .. *** Changed. ***

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.

*** Changed. ***

16. Page 738, line 5: Baseline background vertical advection in the 90 km region is assumed to be upwards at 0.1 cmsê(-1) for April and 0.8 cm sê(-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? *** Please see Simulations section, para 4, see "From simulations aimed at agreement with the MSAO observations etc. ***

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. *** Corrected figure is now included. Thanks again. ***

18. Page 738, line 13: From 90 to 100 km the H mixing ratio is approximately constant,Æ Well, it certainly varies with altitude, but it's almost constant in time. *** The mixing ratio is approximately constant with altitude (not the density which is the displayed quantity) and results in no vertical tidal effect. The text remains unchanged - for now.***

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. *** This is OK now that Fig. 2b has been corrected. ***

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. *** This line has been removed. Instead, more detailed comments on the change of H with local time have been added

to the previous paragraph. ***

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). *** See Tides section, para 1. for an expanded explanation. Two specific altitudes are now chosen for local time comparison, 82 km and 94 km, since those times appear specifically in the SABER and in the WINDII papers. The comparison now cites the figure numbers used in each case and concludes the agreement is within 2 hours of LT. ***

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? *** Thank you again - Changed to Russell et al 2005 as intended. ***

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

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. *** As we have mentioned elsewhere this study does not directly address the MSAO origins, rather the prevailing conditions for the two MSAO extremes. ***

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

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. *** Added. ***

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? *** Clarified.***

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

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. *** Please see expanded text, comments on VER to zenith. ***

30. Page 741, line 4: Slightly better agreement is achieved by arbitrarily increasing the rate of removal OH* (vÆ = 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 x 10ê(-10) cmê3 sê(-1) (roughly the Adler-Goldon value) yields unrealistically large O values retrieved from SABER. *** The reference to Smith et al. is now included in text. However, we would note that the matter of O concentrations is still a matter of major debate.***

31. Page 741, line 15: are for the baseline eddy mixing COEFFICIENT of approximately 1x10ˆ6 cmˆ2 sˆ(..1) *** Changed. ***

32. Same line: Why æapproximately? *** 'approximately' removed. ***

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? *** Changed to relate size of square to measurement precision for H2O and O3. Also see discussion in section on Figure 9, we have stressed the limits on eddy diffusion and vertical advection. ***

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

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. *** The intentionally abbreviated comments have been limited to first order effects. ***

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. 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. *** Again, the intent here is to 'measure' some parameters in the MSAO event rather than to focus on MSAO sources. The MSAO sources summary in Intro para 3 indicates the topic is still not fully explained, even with the availability of modern 3D models. ***

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

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. *** See expanded discussion for Figure 9 on measurement precision for O3 and H2O, and the related limits on eddy diffusion and vertical advection. ***

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

on board Envisat, as is SCIAMACHY. *** Changed. Thanks again.***

*** End of Reviewer #1 Comments ***

Atmos. Chem. Phys. Discuss., 13, C203ûC204, 2013 Interactive comment on Atmos. Chem. Phys. Discuss., 13, 729, 2013. Atmosphericwww.atmos-chem-phys-discuss.net/13/C203/2013/ Chemistry

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

Anonymous Referee #2

Received and published: 21 February 2013

1. This paper addresses the equatorial MSAO using a 1-D photochemical/dynamical model and satellite composition observations. It contains a fairly representative literature survey.

2. It is unclear what the motivation for this study may be. This interesting middle atmospheric phenomenon certainly deserves using state-of-the-art modeling techniques. One-dimensional modeling of this type, that uses eddy diffusion to replace horizontal advective processes, dates back to the 1960s. *** The state of 3D modeling of the MSAO is briefly summarized in the Introtion, para 3, the conclusion being that the 3D simulations do not yet completely explain the MSAO observations. See revised Abstract e. g. "The objective here is not to address directly the complicated driving forces of the MSAO, but rather to employ a combination of observations and model simulations to estimate the limits of the some of the underlying dynamical processes." Further, Introtion para 4 - "The objective here is not to address directly the complicated driving forces of the MSAO but rather to employ a combination of observations and model simulations to estimate the limits of the some of the dynamical processes." And in Section 'Interactions between vertical eddy diffusion and vertical advection' see the conclusion that limits have been placed on those two parameters using the approach

described here. ***

Another major shortcoming of this study is the lack of direct comparison of many of the individual species modeled with relevant measurements. *** The authors believe that including more comparisons would not add to the focus of the manuscript. ***

3. Minor comments: (a) the following terms:: eddy mixing and eddy mixing rate (Abstract), eddy turbulence (pg 734, line 1), eddy turbulence (pg 735, line 22), turbulence (pg 735, line 24), eddy mixing (pg 735, line 26), eddy mixing rate (pg 735, line 28), eddy mixing (pg 736, line 18), eddy mixing (pg 737, line 10), eddy mixing coefficients (pg 738, line 3), eddy mixing rates (pg 740, line 17), eddy ACPD13, C203ûC204, 2013 mixing (pg 741, section 6, line 16, line 18, and line 25), eddy coefficient (pg 741, line 20), eddy mixing coefficient (pg 742, line 18), eddy mixing rate (pg 742, line 22), eddy mixing (pg 742, line 25), and eddy mixing (pg 743, line 4) should be replaced by eddy diffusion or eddy diffusion coefficient, as appropriate. *** All changed. ***

(b) On page 736, line 24, CO and CO2 are given also as carbon monoxide and carbon dioxide, however none of the other species in the list is identified in the same way here. *** Species not previously spelled out in the preceding text are now included, in 'Simulations of the relevant species profiles', para 1. ***

*** End of Reviewer #2 Comments ***

*** The authors wish to thank the Reviewers for all of their excellent comments and suggestions. ***

The revised manuscript is attached as a supplement pdf file.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C1539/2013/acpd-13-C1539-2013-supplement.pdf