

Interactive
Comment

***Interactive comment on* “Diurnal variations in middle atmospheric water vapor by ground-based microwave radiometry” by D. Scheiben et al.**

Anonymous Referee #1

Received and published: 9 April 2013

Review of Scheiben et al., Diurnal variations in middle atmospheric water vapor by ground-based microwave radiometry This paper shows comparisons between observed diurnal variations of water vapor, and variations seen in WACCM. The paper is generally well written, and the WACCM analysis is good. I do have some major concerns with the presentation of the comparisons, and, particularly, with the error estimates presented for the ground-based measurements. Only near the end of the paper is it admitted that “The discrepancy between modelled and observed diurnal cycle below 1 hPa could be attributed to instrumental effects.” This is an admirably honest statement which must be made (I think it should extend to the 1 hPa data as well), but it is clearly inconsistent with the error bars on Figures 2 and 4. A casual reader would look at the Figures, see the error bars, and conclude that there must be a serious problem with the model – a conclusion which is never drawn in the paper. A more thorough

C1127

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



reader will conclude that the authors wrote the study and at the end realized that the original error bars were too small.

I understand the difficulty of accurately calculating error bars, particularly in cases where the causes of the error are not all clearly understood. Before publication at least two changes should be made regarding the error bars. First, take the discussion of errors from Section 4 and move it to so that it is together with the error discussion. Secondly, edit the captions to Figures 2 and 4 so that they clearly state terms are included in these error bars.

Suggestions and questions follow:

3863, line 25+: Apparently all of the data shown here has been taken with upgraded instruments which provide more profiles per day. The implication is that it would not be possible to perform this study without this upgrade and that therefore only the upgraded data is used (is this true?). But the Haeferle et al. study was clearly performed before the upgrade, showing that it is not necessary to use the upgraded instruments. Please clarify.

3864, line 10: “To reduce these baseline artifacts, a polynomial fit of order 3 and a sine-fit with 6 periods are applied to the measurement. This leads to a loss of measurement sensitivity on lower altitudes and is the main reason why the instruments are not sensitive below 35 km altitude.” This is a modification of a ground-based microwave retrieval which can have important consequences for the retrieved vertical profile in the stratosphere, yet there is very little detail and no reference given. Is it fit as part of the optimal estimation procedure? The Tschanz manuscript referenced here suggests so, but it gives a somewhat different description of the baseline, and also mentions that the two instruments use different fits. How have the investigators determined the altitude sensitivity for the results shown here? Is it different for the two instruments? How large are the fitted waves and polynomials and how does this compare to the signal?

Presumably there is a diurnal variation in the tropospheric optical depth. What efforts

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

have been made to ensure that these are not affecting the data? How is this accounted for in the error estimates? Do diurnal variations in rain have any significant affect on the data?

3869, line4+: “The sharp phase shift in the WACCM data from December to April ...”. This makes it sound as if the phase shift is from December to April. Please rewrite as, perhaps, “The December to April WACCM data shows a sharp phase shift ...”.

3870, line 14: “In the mesosphere, the observations and the model data agree well with each other.” The authors simply cannot make this statement. Based on Figure 5, the agreement breaks down at ~ 0.2 hPa in the December to April data, while the amplitude for the June to October data in Figure 5 looks to be about an order-of-magnitude larger throughout almost the whole of the mesosphere in the measurements when compared to the model. None of this is consistent with “agree well”.

3871, line 28: “The previous study by Haefele et al. (2008) ...”. The lack of comparison with this previous study is troubling, since one would hope for some consistency between these results and those. How does this study compare with the Haefele study at lower altitudes? “However, the amplitude in the current study is approximately twice as large.” – Are the authors saying that they have 100% measurement contribution, and the previous study had only 50%? This would only possibly be true, if at all, over a very small altitude range.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 3859, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)